



# Economic evaluation of designing in the unknown : Design theory-based models beyond decision theory under uncertainty

Agathe Gilain

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**THÈSE DE DOCTORAT**  
**DE L'UNIVERSITÉ PSL**

Préparée à MINES ParisTech

**Economic evaluation of designing in the unknown**  
**Design theory-based models beyond**  
**decision theory under uncertainty**

**Évaluation économique de la conception dans l'inconnu**  
**Modèles basés sur la théorie de la conception, au-delà de la théorie**  
**de la décision dans l'incertain**

Soutenue par

**Agathe GILAIN**

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**et de l'Échange**

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## Résumé

La théorie de la décision fournit des techniques qui permettent de calculer la valeur de l'information et de guider des décisions dans l'incertain. Ces dernières sont très pertinentes pour le calcul économique dans l'incertain. Mais elles ignorent la valeur de l'inconnu et il est largement reconnu qu'il est inapproprié, voire trompeur, d'utiliser dans l'inconnu des techniques de calcul économique dérivées de la théorie de la décision. **Dans ce contexte, cette thèse cherche précisément à développer, dans le cadre théorique de la théorie de la conception, des techniques qui permettent d'isoler et d'évaluer la valeur de l'information dans l'inconnu.** Nous nous intéressons à l'évaluation économique de l'inconnu contenu dans un type particulier de situations : les situations dans lesquelles on ne s'attend à priori pas du tout à trouver de l'inconnu. Nous appelons les inconnus dans ce type de situation les « *inconnus cachés* » ou « *non visibles de manière évidente* ».

Nous avons développé ces techniques d'évaluation économique dans l'inconnu avec l'objectif d'évaluer l'impact d'*inconnus cachés* au niveau de phénomènes de conception réglée et de conception innovante identifiés sur l'industrie aéronautique commerciale : une dynamique fortement croissante des coûts de développement au niveau du secteur d'une part, et deux cas d'investissement chez Airbus d'autre part. À travers le prisme des modèles économiques traditionnels dérivés de la théorie de la décision, ces phénomènes semblaient particulièrement intrigants. Étaient-ce des anomalies ? En mettant en évidence et en évaluant l'impact et le rôle d'*inconnus cachés*, nos techniques de calcul basées sur la théorie de la conception fournissent de nouveaux cadres explicatifs pour des phénomènes qui apparaissent incompréhensibles. Ces techniques relèvent que l'impact économique d'inconnus cachés peut être considérable. L'*inconnu caché* peut entraîner de très forts coûts. Et il peut générer des profits colossaux, à condition que les méthodes adaptées pour détecter et explorer efficacement l'inconnu aient été déployées.

Le résultat selon lequel l'impact économique des *inconnus cachés* est quantifiable et peut être substantiel souligne l'importance de continuer à chercher comment l'évaluation économique de l'inconnu pourrait être outillée plus systématiquement et plus généralement, au-delà des phénomènes spécifiques étudiés dans cette thèse : l'enjeu associé à un tel outillage est de piloter l'impact économique de l'inconnu, en particulier d'éviter ses effets néfastes et d'exploiter son potentiel économique prometteur. Cela soulève aussi des questions organisationnelles par exemple en termes de transformations requises par les *inconnus cachés* à fort effet économique.

Ce travail de recherche a donné lieu à une thèse par articles. Un article porte sur une forme spécifique d'*inconnu caché*, l'expansion fonctionnelle, dont nous évaluons l'impact économique. Deux articles visent à mettre en évidence et isoler l'inconnu et sa contribution à la création de valeur économique dans deux cas de projets d'investissement identifiés chez Airbus. Ces trois articles sont en annexe de ce document (Section 8), après une synthèse des contributions de notre travail de recherche au sujet de l'évaluation économique de la conception dans l'inconnu (Sections 1 à 7).



## Abstract

Decision theory provides computational techniques which quantify the value of information and guide decisions under uncertainty. These latter are very relevant for economic calculation under uncertainty, but they ignore the value of the unknown. And it is now well proved that such techniques are unsuitable, even misleading in situations that contain unknown.

If design theory extends decision theory to the unknown, it has not gone as far as providing computational techniques to assess the value of information in the unknown. **In this context, the object of this thesis is precisely to develop, within design theory-based models, techniques that enable to isolate and assess the value of information in the unknown. We are interested in the economic evaluation of the unknown contained in a particular type of situation: situations in which one does not expect to find the unknown at all. We call the unknowns in such situations the 'hidden unknowns' or 'not obviously visible unknowns'.**

We have developed techniques of economic evaluation in the unknown with the aim of evaluating the impact of the 'not obviously visible' unknown at the level of rule-based design and innovative-design phenomena identified in the commercial aircraft industry: a cost escalation trend on the one hand, and two Airbus investment cases on the other hand.

Viewed through the prism of traditional decision theory-based economic models, these phenomena seemed particularly intriguing. Anomalous? By highlighting and assessing the impact and the role of the 'hidden unknowns', our design theory-based computational techniques provide new explanatory frameworks for economic phenomena that seemed anomalous. They also reveal that the economic impact of 'not obviously visible unknown' can be substantial. It can entail high costs. And it can generate huge returns, provided appropriate design methods suitable to detect and efficiently explore the unknown have been deployed.

Beyond the specific phenomena studied in this thesis, the finding that the economic impact of 'hidden unknowns' is quantifiable and can be large emphasizes the importance of further investigating how 'economic evaluation in the unknown' could be tooled more generally and systematically in order to manage the economic impact of the unknown, and in particular avoid its detrimental effects and exploit its promising economic potential. And it raises organizational questions (in terms of organizational transformations required by 'hidden unknowns' with high economic impact)

This research work gave rise to a paper-based thesis. One paper studies the impact of a special form of discreet unknown (that is functional expansion-induced unknown) on development cost escalation phenomena. Two papers are dedicated to highlighting and isolating the unknown and its contribution to the creation of economic value in two puzzling investment cases identified in Airbus. These three working papers are appended to this document (Section 8), after a synthesis of the contributions of our research work on the topic of 'economic evaluation of designing in the unknown' (Sections 1 to 7).

*À Brigitte M.*

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Je n'ai pas compris et j'en fus impressionné. Je suis toujours impressionné par l'incompréhensible, car cela cache peut-être quelque chose qui nous est favorable.

Romain Gary, *Gros-Câlin*

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## 1. Introduction: motivations for evaluating economic performance in the unknown

### 1.1. Developing calculation techniques for economic evaluation – a long-standing issue

The development of theories and tools to assess situations in economic terms and generate quantitative information that will indicate a ‘rational’ course of action (the rationality being that of the theory in question) and guide decision-making is a long-standing problem.

For example, in the late 19th century, the second industrial rise called for the development of economic calculation techniques to manage expensive investments in new, high-volume production technologies. There was a challenge of ensuring that the economies of scale induced by the increased production volume would compensate the high cost of the new production technologies (Chandler, 1990). Such questions encouraged the development of economic notions and techniques (e.g. the first production functions, the theory of marginal productivity) which are now well established in industrial firms, and still taught nowadays.

**In the early 20<sup>th</sup> century, one major challenge for economic calculation concerned the treatment of uncertainty.** In the previous example, the parameters involved in the decision are fairly foreseeable and identifiable (parameters associated with a new production technology), and the level of uncertainty is relatively low. But when the level of uncertainty is higher, economic performance measurement issues become trickier. The difficulties raised by uncertainty for economic calculation were identified from the early 1900s, both among practitioners, who used ‘rudimentary’ approaches to deal with it (Pezet, 1996) and among scholars theorizing the notions of ambiguity, uncertainty, radical uncertainty (Knight, 1921; Keynes, 1921). The Cold War years confronted economic decision-makers to more uncertainty-intensive situations (innovative New Product Development projects, R&D activities...) and triggered major advances in Operational Research (linear programming, multiple-criteria decision analysis, etc.), namely to develop calculable decision rules that would solve resource allocation and task planning problems.

The formalization of decision theory in the 1950s/1960s, in particular Savage’s (1954) Subjective Expected Utility Theory (described in *The Foundations of Statistics*) has provided a solid theoretical basis for economic calculation under uncertainty. Indeed, it has provided economists with a model of rationality, that is a ‘Bayesian rationality’<sup>1</sup>. Economists have largely adopted this model to describe how economic agents make rational choices under uncertainty (Giocoli, 2013; Binmore, 2007; Keifer and Nyarko, 1991). They sometimes refer to Savage-Bayesian models (Keifer and Nyarko, 1991). In Savage-Bayesian models, Savage’s (1954) axioms lay down the rules that the decision-makers’ beliefs need to meet in order to be consistent with probability calculation. The rational behavior is modelled by expected utility optimization: in the face of a set of choices, the decision-maker chooses the alternative which maximizes his/her (subjective) expected utility. Learning is a byproduct of the maximizing expected utility behavior. And it takes place in a Bayesian mode: following the collection of new information, the decision-maker will update his/her a priori beliefs

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<sup>1</sup> Giocoli (2013) explains that Savage’s (1954) Subjective Expected Utility Theory contributed to economics in an unintended way: Savage’s (1954) original goal was to propose the foundations of a new kind of statistics, where the statisticians would behave as economic agents. This project was a failure. But economists took up Subjective Expected Utility Theory as a model of rationality (they had been looking for such a model for a hundred years!)

according to Bayes rule, turning *a priori* beliefs into better informed, less uncertain *a posteriori* beliefs.

Subjective Expected Utility Theory and Savage's axioms have been the subject of a series of criticism and debates in the economic discipline. These criticisms, starting with Allais' (1953) paradox, could very well have led economists to give up expected utility models. Quite the contrary, economists have rather attempted to answer and address successive paradoxes and contestations, by enhancing, refining the axioms, but keeping the original expected utility model (Lainé, 2014)<sup>2</sup>. This model, which economists have adopted and kept, entails major advances from the viewpoint of the mathematical treatment of uncertainty. Indeed, Savage's (1954) axioms cover subjective beliefs. And subjective probability (consistent with Savage's axioms) becomes the variable that models uncertainty (Hey, 1990). Put differently, subjective probability models the lack of knowledge of the decision-maker about the decision space, i.e. about the decisional alternatives, their consequences and about the states of the world. With subjective probability as an uncertainty variable, a mathematical treatment of Knightian uncertainty becomes possible (this latter can be 'reduced' to Knightian risk<sup>3</sup>). **The formalization of decision theory has contributed to the development of a spectrum of analytical techniques (from decision trees (Wald, 1950), the earliest ones, to real options, the most recent ones (Myers, 1977; Trigeorgis, 1997) that enable to compute the value of uncertainty-reducing information and that indicate a rational decisional way thanks to the expected utility criteria.**

In sum, the articulation of conventional deterministic economic notions (Net Present Value, net benefits, costing estimation, etc.) with decision theory and deriving techniques offers a means to rigorously address economic problems under uncertainty (for example, problems which involve the selection of the best alternative between various investment projects). Cases of the deployment of economic evaluation tools based on decision theory can be found in the oil industry (Charreton and Bourdairé, 1985; Hatchuel and Moisdon, 1987) and in the automotive industry (Hooge, 2010), with subjective probability distributions being built based on expert opinion. But such an application is not straightforward. Cognitive biases (Kahneman and Tversky, 1974) or limited rationality (Simon, 1955) may lead actors to make decisions that differ from the decision theory-based rational way of doing. The rationality of decision theory-based tools may also conflict with the functioning of the organizations which attempt to deploy them<sup>4</sup> (Hatchuel and Moisdon, 1987), raising in turn difficulties in implementing these economic calculation practices.

This thesis leaves these problems aside to concentrate on an issue of a different nature. It does not focus on situations where the computational means are already available, and suffer

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<sup>2</sup> According to Lainé (2014), contemporary microeconomics of decision-making under uncertainty can be seen as the heritage of all the responses to the series of paradoxes and contestations that emerged in economics.

<sup>3</sup> Knightian risk (Knight, 1921) corresponds to 'measurable uncertainty': the occurrence of an event can be evaluated in a numerical way (with probabilities). In contrast, Knightian uncertainty corresponds to proper, unmeasurable uncertainty: the event is identified but the one cannot quantify its probability of occurrence

<sup>4</sup> real organizations differ from the ideal organization required by decision theory, that is 'a unique decision-maker who simultaneously takes into account all the aspects of the problem being dealt with' (Hatchuel and Moisdon, 1987)

from biases, limitations, applicability difficulties, etc. when deployed in a real organization context. It focuses on situations where economic quantification itself is problematic, from a computational point of view, i.e. on situations which seem to be orphaned by economic calculation, and which seem to call for new developments in terms of economic evaluation techniques. And **after ‘uncertainty’, it is ‘the unknown’ that seems to require new developments in economic calculation.** In what follows, we explain how the unknown raises difficulties for economic calculation from a modelling point of view (subsection 1.2) and from an empirical point of view (subsection 1.3).

## 1.2. Theoretical motivations: from ‘uncertainty’ to ‘the unknown’, a new challenge for economic calculation

### The unknown – definition used in the thesis

Let us consider a collective of actors located in an organization, which, on the basis of its knowledge, expertise, and methodologies (i.e. methodologies for generating hypothetical scenarios of the future), is able to identify a certain number of variables that characterize a situation (e.g. that characterize a project):  $\theta_1, \theta_2, \theta_3, \dots, \theta_n$ .

**Some of these  $\theta_i$  may be affected by uncertainty:** the actors do not associate them with a deterministic value, but only with a distribution of probabilities.

Figure 1a. below illustrates that if we suppose that the variables  $\theta_1, \theta_2, \theta_3, \dots, \theta_n$  take their values from discrete (subjective) probability distributions which represent the collective’s beliefs, a finite number  $k$  of possible combinations  $\{\theta_1, \theta_2, \theta_3, \dots, \theta_n\}$  can be forecasted, with a discrete probability of occurrence  $p_i$  for each possible combination  $\{\theta_1, \theta_2, \theta_3, \dots, \theta_n\}_i$ . A combination  $\{\theta_1, \theta_2, \theta_3, \dots, \theta_n\}_i$  can be assimilated to a ‘**state of the world**’, and the set formed by  $\{\theta_1, \theta_2, \theta_3, \dots, \theta_n\}_i, i=1..k$  constitutes the ‘state space’ associated with the situation. This state space is relative to the collective’s knowledge, expertise, methods, etc. One can make a parallel with the ‘subjective space of possibilities’ theorized by Faulkner et al. (2017) or the ‘scenario spaces’ in Feduzi et al. (2020).

**Under uncertainty, decision theory and deriving techniques can be leveraged to guide collective action under uncertainty, in particular to identify the utility-maximizing decisional way and to assess the value of information under uncertainty (the value of uncertainty reduction) (Figure 1b).**

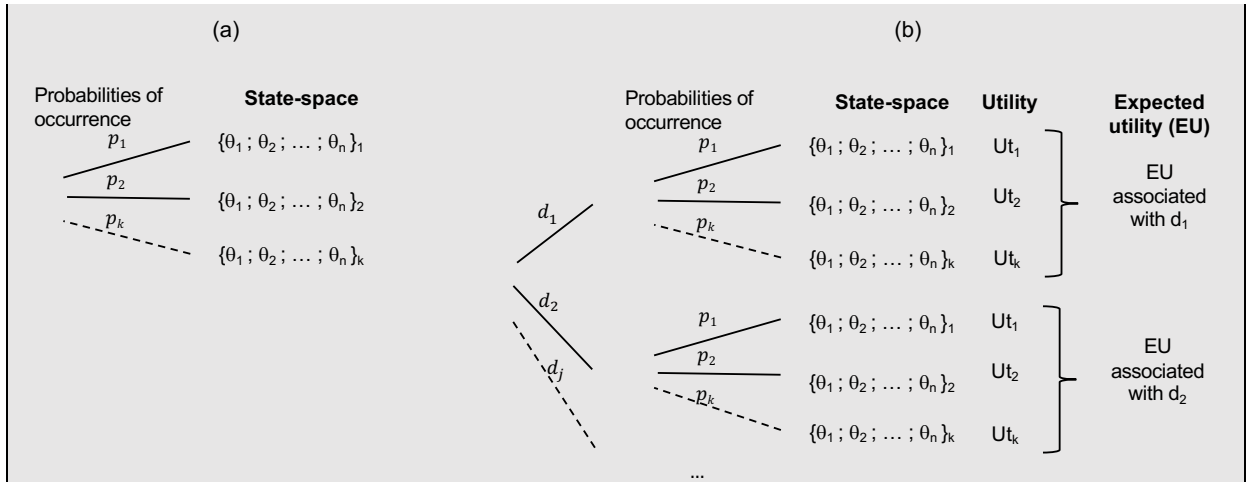


Figure 1a. A state-space representation in an uncertain situation

Figure 1b. Decision theory-based techniques to guide action under uncertainty

The description offered by the state-space  $\{\theta_1, \theta_2, \theta_3, \dots, \theta_n\}_i, i=1..k$  may suffer from inaccuracies with respect to the real situation. These latter may take various forms (Faulkner et al. 2017). One form of inaccuracy interests us particularly: **the fact that some variables  $\theta_{n+1}, \theta_{n+2}, \dots, \theta_{n+j}$  of the real situation may be unknown to the collective, and thus may be missing from the representation  $\{\theta_1, \theta_2, \theta_3, \dots, \theta_n\}_i, i=1..k$ . Associated with these latter are new, initially unknown states of the world, which can be called ‘the unknown’.**

It has long been acknowledged that new, initially unknown states of the world (and by implication, new, initially unknown  $\theta_i$ ) cannot be quantified by decision theory (Loch et al., 2006). They fall into Shackle’s residual hypothesis which is the only form of uncertainty that remains out of reach for decision theory (Hey, 1990 ;Le Masson et al., 2019). If one uses decision theory-based economic evaluation techniques in a state-space involving both uncertainty and unknown, one will only compute the value of uncertainty-reducing information, and one will neglect the value of information in unknown, i.e. one will not capture the economic impact of the discovery of new states of the world. This may be more or less problematic depending on whether the unknown is ‘with impact’ or ‘without impact / under control’.

#### 1.2.1. In a decision theory-based paradigm: the issue of ‘unknowns with impact’

##### Unknown with impact – definition

In what follows, we call ‘unknown with impact’ the new, initially unknown variables  $\theta_{n+1}, \theta_{n+2}, \dots, \theta_{n+j}$  that may disturb the variables  $\theta_1, \theta_2, \theta_3, \dots, \theta_n$  and the associated states of the world  $\{\theta_1, \theta_2, \theta_3, \dots, \theta_n\}_i$ , that may render them negligible or no longer relevant, or that may supersede them, entailing strong (socio-)economic impacts.

In the rest of the document, when the term ‘unknown’ is used alone, it refers to ‘unknown with impact’.

If the new  $\theta_i$  are negligible / ‘without impact’, or if the collective is able to control the new  $\theta_i$  in a fashion such that the  $\{\theta_1, \theta_2, \theta_3, \dots, \theta_n, \theta_{n+1}, \theta_{n+2}, \dots, \theta_{n+j}\}_{i, i=1..l}$  state-space is equivalent to the  $\{\theta_1, \theta_2, \theta_3, \dots, \theta_n\}_{i, i=1..k}$  state-space, one can continue to guide decision-making with an assessment of the uncertainty related to the already-identified variables<sup>5</sup>.

In contrast, if the new  $\theta_i$  that are to emerge are ‘with impact’ (e.g. if they strongly disturb the variables  $\theta_1, \theta_2, \theta_3, \dots, \theta_n$  and the associated states of the world  $\{\theta_1, \theta_2, \theta_3, \dots, \theta_n\}_i$ , if they render them negligible or no longer relevant, or if they supersede them, which can translate into strong (socio-)economic impacts), decision theory is no longer suitable to provide reliable quantitative indications that can guide decision-making, because it can only quantify the uncertainty related to already-identified variables / already identified states of the world. It will not evaluate the impact of the new, initially unknown variables, which can be misleading for action-taking in the unknown, namely for investment decision-making in the unknown.

Indeed, classical decision theory-based techniques (e.g. decision trees) applied in the unknown will assimilate the unknown to an extreme form of uncertainty. In the face of radical innovation projects that may contain unknown, they will predict a very low expected profitability (since according to decision theory, the more uncertain a project is, the lower the probability of making it profitable) and advise rational, risk-averse decision-makers against investing in such projects. In other words, they may reject promising innovation projects. In order to avoid this, there exist other techniques which evaluate and manage highly uncertain situations in a more sophisticated way, with the logic of little risky pre-investment trials: real options (McGrath, 1996), discovery-driven planning (McGrath and MacMillan, 2009), etc. Real options concede a small amount of investment<sup>6</sup> in additional information gathering (uncertainty-reducing information), so that one can, in a second time, decide more knowingly on whether or not to invest. As a financial option, the real option offers the right but not the obligation to purchase the asset. Therefore, if the asset proves interesting, one invests further. And if it turns out to be unprofitable, one has no obligation to invest and one loses nothing, except the cost of the option. Discovery-driven planning proposes to set a minimum acceptable economic return target, and to list the assumptions that need to be satisfied for the economic target to be met (McGrath and MacMillan, 2009): as new, unknown states of the world are unveiled during the discovery-driven process, the assumptions are updated.

These pre-investment trial approaches propose to manage highly uncertain situations with an adaptive process. They delay the immediate, risky investment in large-scale budgets, and avoid immediately rejecting promising projects. But if the target is in the unknown, launching pre-investment trials may be an inefficient and costly groping search, since it may take a lot of testing and time before a satisfactory result is finally achieved. Be the trials sequential or parallel, it is like taking gambles on small steps (Kokshagina, 2013). In addition, be it real options or discovery-driven planning (which can moreover be combined), the value of the new information brought by the newly discovered states of the world is not assessed in an ‘isolated’ way –it is either not assessed at all, or blended with the value of uncertainty-

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<sup>5</sup> This is what generally happens when decision theory-based techniques are applied in the real world: when decision theory under uncertainty is applied, the actors are never entirely omniscient and / or the future is never entirely predictable. So, in practice, decision theory is never applied within a perfectly fixed state-space.

<sup>6</sup> The cost of the option

reducing information. Finally, one can note that since one has no obligation to invest if the option proves unprofitable, real options encourage creating highly volatile option portfolios, with high variance, which can, with admittedly low probabilities, reach extreme values in terms of profit (McGrath, 1999). But nothing ensures that such option portfolios are robust to the emergence of unknown unknowns, unexpected events, etc.

In summary, the more sophisticated approaches which propose to evaluate and manage high uncertainty situations, remain in a decision-theory paradigm: **they only assess the economic impact of uncertainty, and not the economic impact of new, initially unknown states of the world.** Using these uncertainty-centered economic evaluation approaches to guide decision-making in the unknown may be misleading. Consequently, as one moves from uncertainty to the unknown, one ‘loses’ the decision theory-based formal framework and the computational techniques deriving from it. And situations where new  $\theta_i$  ‘with impact’ are likely to emerge are undecidable from the viewpoint of decision theory.

### 1.2.2. Design theory-based decision model: a promising basis to quantify the effects of newly designed ‘unknowns with impact’

Recent theoretical advances have demonstrated that design theory extends decision theory to the unknown (Le Masson et al., 2018)<sup>7</sup>: with a design theory-based decision model accounting for the possibility to enrich a decisional space by designing new decisions and / or new states of the world in the unknown, Le Masson et al. (2018) propose a model of decision-making in the unknown, with an associated rationality for actions in the unknown. In this design theory-based paradigm, situations that involve ‘unknown with impact’ are no longer undecidable. And according to this model, decision-makers design the new, initially unknown  $\theta_i$  – they design the unknown, and which suggests that its impact and its value are also ‘to be designed’. But thus far, these advances in design theory did not yet go as far as proposing the computational techniques that would quantify the value associated with the impact of newly designed  $\theta_i$  and provide economic-based guidance for rational action in the unknown.

#### **Theoretical motivations:**

The absence of techniques enabling to quantify the impact of new, initially unknown  $\theta_i$  is a first motivation for studying the topic of economic evaluation in the unknown: it is theoretically interesting to investigate whether in a design theory-based paradigm, the value of newly designed, initially unknown  $\theta_i$  can be quantified, in the same fashion as decision theory quantifies the value of reducing the uncertainty associated with already identified  $\theta_i$ . In addition, it is theoretically interesting to investigate how quantified unknown-related information could be integrated within operational models or tools that guide investment decision-making and help manage economic value in the unknown.

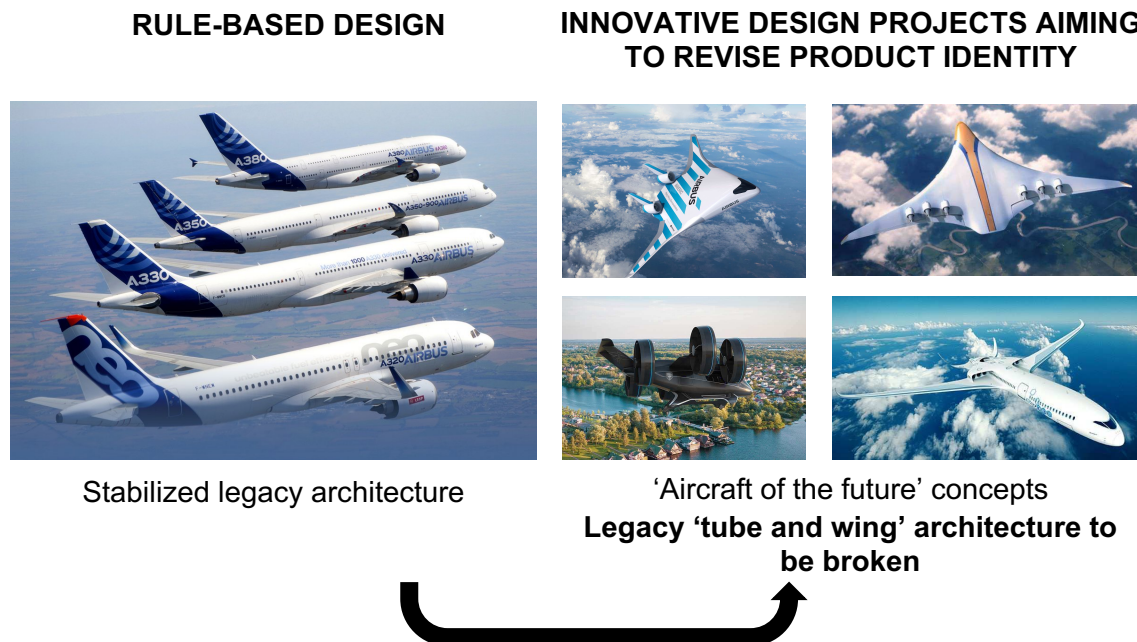
In the next subsection, we explain how being able to quantify the impact of new, initially unknown  $\theta_i$  could also have an empirical interest.

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<sup>7</sup> This model will be described in more details in our literature review (section 2.2)

1.3. Empirical motivations: the issue of ‘potentially hidden unknown with impact’  
 1.3.1. ‘Obviously visible unknown with impact’ in disruptive innovation projects

‘Unknowns with impact’ can obviously be associated with radical, disruptive innovations which break products legacy architecture in a very visible way, as Figure 2 below illustrates.



*Highly visible emergence of ‘unknowns with impact’, at the level of product architecture, but also at the level of the ecosystem of actors that interact with the object, etc.*

**Figure 2. Highly visible ‘unknowns with impact’ at the level of disruptive innovation projects**

Over the last decades, market and technological dynamics have been pressing firms to develop radical, disruptive innovations, generating a context of intensive innovations. The projects devoted to the development of these innovations are the most obvious places to find ‘unknown with impact’. In these projects, ‘unknowns with impact’ are inevitable and everywhere, at the level of the product with engineering departments being required to regenerate their set of design rules, to renew the architecture, the identity, the value space and value criteria associated with the product, etc. (Le Masson, 2001; Le Masson et al., 2014; Gillier et al., 2015), at the level of the ecosystem of actors which can be impacted by substantial changes, at the level of product marketing, etc.). And exploring and designing these unknowns with an innovative design approach is the essence of the project.

As we have seen above, no technique seems available to quantify the economic impact of the unknown, be it in the decision theory framework or in the design theory framework. But with respect to disruptive innovation projects aimed at revising the identity of a product, a certain amount is already known about the economic phenomena associated with the unknown contained in these projects, as well as regarding the value management levers in these projects.



In particular, the design function<sup>8</sup> is a design theory-based model (Le Masson, 2001; Hatchuel and Le Masson, 2006 ;Le Masson et al., 2010) which accounts for how the knowledge regeneration achieved by exploring the unknown can have a strongly positive effect on economic growth dynamics in situations of radical innovations. For instance, this model explains how Tefal<sup>9</sup> has followed a thriving growth dynamics for 20 years thanks to its ability to explore unknown knowledge bases in a systematic, repeated manner: the firm had developed the capacity to reuse on subsequent developments the ‘excess knowledge’ earned from the exploration of unknown knowledge bases on previous developments projects: this dynamic allowed Tefal to generate lineages of competences and lineages of products (with the diversification of product families) which entailed a strong innovation-driven growth dynamics.

Regarding value management, it is largely acknowledged that conventional value management approaches are unsuitable in projects that aim at revising product identity (Gillier and Hooge, 2015). Targets (market, technical, etc.) are difficult, if not impossible to forecast. Costs, benefits, Net Present Values cannot be fairly and reliably assessed, etc. The conditions for value engineering (Miles, 1961)<sup>10</sup> to be applied (which namely require a ‘stable product identity’) are not met (Gillier and Hooge, 2015). Yet, ‘value in the unknown’ is not unmanageable: recent research works propose a value management engineering involving specific tools for managing value in the unknown and guiding decisions accordingly, in the framework of ‘design regimes’ for the value of the unknown (Hooge, 2020). In this value management approach of the unknown, the unknown has value that is *a priori* not present, and calls for being designed with a rigorous managerial approach and dedicated tools, in the framework of an expanded concept of value: beyond economic value, the design of strategic value and stakeholder adhesion value in the unknown are to be managed.

These works did not go as far as proposing an economic evaluation of the unknown. In this context, a technique to quantify the impact of the unknown, and an associated economic management logic would be ‘a plus’ that could contribute, in a complementary way, to this engineering of value management in the unknown, in the same fashion as decision theory-based techniques contribute to value management under uncertainty: for the management of project portfolio in the unknown, it would offer a single criterion against which to rank investment projects (although this classification may not necessarily be the only criterion for the

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<sup>8</sup> The ‘design function’ model is a function defined over two spaces: a space of goods  $G$  and a space of knowledge  $K$ . This function accounts for the extension of these two spaces with new goods and new competences. (Le Masson, 2001; Le Masson et al., 2010). It can be represented with the relationship:

$$G \times K \rightarrow G \times K$$

$$(G_1, \dots, G_n, K_1, \dots, K_m) \xrightarrow{f} (G_1, \dots, G_n, \dots, G_{n+i}, K_1, \dots, K_m, \dots, K_{m+j})$$

where the  $G_i$  refer to the goods and the  $K_i$  refer to competences. This model generalizes the model of the production function, which in contrast covers a fixed list of goods, in a Lancasterian framework. The initial space and the final space being identical, the design function is recursive: *fof ... of* accounts for how the outputs generated at a time  $n$  can be used as resources at time  $n + 1$ . In that, contrary to the production function which computes static outputs to inputs ratios only, the design function accounts for dynamics returns.

<sup>9</sup> a French household electrical goods company known for its non-stick pans

<sup>10</sup> started in General Electric, in the 40s, during WWII, value engineering involves tool that can be applied to eliminate unnecessary costs, while still increasing product quality and reliability (Miles, 1961)

decision, it would constitute interesting additional information for decision-making); beyond the numerical result of the calculation, the use of an economic calculation technique generates discussions between actors and encourages the gathering of coherent information – which can have positive impacts in terms of coordination and building coherence in a project (Hatchuel and Moisdon, 1986 ; Hooge, 2010).

But are the projects which involve the revision of the identity of the product (in particular, a clear break in the legacy architecture of a product) the only place where ‘unknowns with impact’ can be found? For example, could there also be ‘unknown with impact’ in rule-based design-driven product evolution trajectories? In the first instance, the intuition is rather that there is ‘no unknown with impact’ in rule-based design (Figure 2 above illustrates this intuition well).

However, two phenomena taking place in contexts where product identity is not specifically revised lead to ask whether ‘hidden’ / invisible ‘unknowns with impact’ could be found in rule-based design. These phenomena are explained in detail the next subsection.

### 1.3.2. ‘Potentially hidden unknown with impact’ in rule-based design

#### **A concerning and intriguing multiplication of costly (i.e. ‘with impact’) design crises**

The recent years have been marked by several ‘design crises’, among which the Dieselgate, the explosion of the Samsung battery Note 7 and the crashes of the Boeing 737-Max.

In these three cases, the failure concerns a well-known product feature, which was supposed to be well mastered by the firm’s engineering departments:

- For decades, with each new generation of engines, Volkswagen has consistently and successfully been reducing by a few percent pollutant emissions, hence meeting the regulations in force. The American and European authorities had informed car manufacturers well ahead of time about the coming enforcement of the new regulations (which were not immensely more stringent than the previous regulations). But curiously enough, the failure at the origin of the Dieselgate concerns the engine and the pollutant emissions: the design failure occurred because Volkswagen discovered that it was unable to simultaneously reconcile customer requirements in terms of car performance (particularly engine power) and (the US) regulatory requirements in terms of reducing pollutant emissions. So it added rigging software that detected regulatory test situations, allowing the car operating in a first fashion that met regulatory requirements in test situations, and in a second fashion that met customer requirements in real driving situations.

- Samsung has been designing increasingly thinner-shelled smartphones for years. Under this trend, increasingly smaller and more powerful batteries are designed by battery suppliers and integrated in these casing. And the source of the short-circuits triggering the explosions of the smartphones turns out to be linked to battery sizing. Samsung retraced the failure mechanism that led to the short-circuits and explosions at the level of the batteries of its Galaxy Note 7. Over the successive generations, smartphones are becoming increasingly thinner-shelled. For the Galaxy Note 7, the (first) supplier in charge of the batteries designed a battery with a thinner separator between electrodes which are not supposed to touch (this

boils down to reducing design (here safety) margins, in order to reduce size. In addition, the battery involved ‘a higher energy density’ (which can ‘exacerbate the severity of a battery failure’). Finally, when these batteries were integrated into the smartphone, within the space allotted by Samsung’s smartphone design, the battery suffered from deformations at the upper corner. According to Samsung’s investigations, these deformations can lead some electrodes (supposed to be separated by the ‘thinner’ separator) to touch and generate a short-circuit.

- Boeing’s products are highly reputed, their safety is validated and endorsed by recognized and demanding aviation authorities (FAA<sup>11</sup>, EASA<sup>12</sup>). The architecture of commercial aircraft is typical of a dominant design architecture (‘tube and wing’ architecture), which is supposed to be mastered by Boeing’s engineers. Designing a stable aircraft, informing the Flight Control Systems with data coming from reliable sensors is part of the very basis of the design activities which Boeing has been successfully achieving for decades. And precisely, the failures which triggered the crashes of Lion’s Air and Ethiopian aircraft were due to design errors at these levels. With the aim of improving some features (namely engine power, fuel efficiency, noise level, weight) of its B737, Boeing has integrated an innovative engine. The diameter of this engine being larger than that of previous engine generations, and the wings of this aircraft having being particularly low-riding, Boeing repositioned the engine further forward under the wing (with respect previous aircraft). As a consequence, the aircraft tended to pitch upwards in certain situations, which could lead to a dangerous aerodynamics stall. Traditionally, when such a situation arises, the weather-vane-like sensor which detects that the angle-of-attack drifts too high (thanks to airspeed and altitude sensing) warns pilots, who then they can handle the situation (by moving the horizontal stabilizer trim upward). When Boeing discovered (quite late, during the first flight tests) that such situations would regularly arise on the B737-MAX (because of the position of the new engine), it (rapidly) designed a new system, the MCAS (Manoeuvring Characteristics Augmentation System) that would further help pilots bring the nose down. Such a system enabled to reconcile (i) the new position of the engine under the aisles and (ii) the aircraft stability requirements. If the MCAS had been introduced as a new system which pilots could decide to activate or deactivate when they want, the 737-MAX would have been considered as involving significant changes in terms of operating conditions for pilots. This would have entailed additional training (delta training) for the pilots, which is less attractive to airlines. This would have made the aircraft less saleable. So in order to reconcile (i) newlypositioned engine, (ii) aircraft safe stability conditions, (iii) no delta training, Boeing decided that the help offered by the MCAS would activate automatically (without requiring pilot action, nor without requiring to inform him), if the nose were to drift too high as the pilot flew manually, and it would deactivate automatically when the angle of attack would be sufficiently low, all this without the pilot noticing anything. Making the MCAS a ‘silent’ system was already questionable. But beyond that, in order to constitute a safe design, the triple (i), (ii) and (iii) needed a fourth dimension: (iv) a reliable Angle of Attack sensing system (or reliability in case of failure of the Angle of Attack sensor). Boeing failed to meet this fourth: in the case of Lion’s Air and Ethiopian crashes, the angle of attack sensor

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<sup>11</sup> Federal Aviation Administration (the US authorities)

<sup>12</sup> European Aviation Safety Agency

erroneously sensed that the nose was drifting too high, so the system activated automatically and pushed the nose down while it was not needed (Gates and Baker, 2019)<sup>13</sup>.

If the design issues at the origin of these three failures came from a radically novel function, or a disruptive technology, hence breaking the identity and the architecture of the car, the smartphone and the commercial aircraft, these cases would not be intriguing. They could be interpreted as an inability of the three firms to prepare the renewal of their products in a timely manner, in particular an inability to generate the necessary knowledge, to renew the design rules and the architectural knowledge, etc.

But there is seemingly no radical innovation (and no ‘unknown with impact’) in these cases. The engineering departments of Volkswagen, Samsung and Boeing were respectively mandated to design ‘a less-polluting engine’, ‘a thinner-shelled smartphone with a safe battery’ and ‘a stable commercial aircraft with a newly-positioned engine’. In these three cases, the design target was fairly identifiable, part of a product evolution dynamics which seemed to be perfectly mastered by the three highly reputed firms. Designing the incriminated features seemed to solely required the manipulation of known design variables and the application of existing design rules. In other words, design engineers were in the face classical rule-based design problems they were supposed to be perfectly equipped to solve. So the common explanations that incriminate an exhausted product architecture and an overambitious functional target are not convincing for such reputed engineering departments. In sum, one finds it difficult to discern the very rule-based design issue that explains such catastrophes. But what if the design issue was precisely not a rule-based design issue?

We can raise the questions: wouldn’t some ‘unknown with impact’ have been encountered by the engineering departments and played a role, as a hidden variable? Could the unknown (a discreet, latent, silent form of ‘unknown with impact’) explain these to date incomprehensible design crises? And if ‘unknown with impact’ turns out to be the problem, what approaches could be considered and prescribed to counteract such phenomena and their devastating socio-economic impacts<sup>14</sup>?

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<sup>13</sup> <https://www.seattletimes.com/seattle-news/times-watchdog/the-inside-story-of-mcas-how-boeings-737-max-system-gained-power-and-lost-safeguards/>

<sup>14</sup> Since the fraud was uncovered in 2015, the Dieselgate has costed Volkswagen \$30 bn (mainly to compensate American consumers) (Reuters, 2019). And the firm continues to set aside money for potential future fines, namely in Europe. Beyond these major economic impacts, this crisis also has dramatic socio-economic effects: impacts on the US supply chain, environmental and public health issues, etc. (<https://www.usinenouvelle.com/article/dieselgate-volkswagen-met-de-cote-5-5-milliards-d-euros-de-plus.N838295>)

The explosion of the batteries of the Samsung Galaxy Note 7 has entailed a loss of 20 bn€ for Samsung (due to a 10% drop in three days in its market capitalization). The recall of faulty devices, consumer compensation and lost sales cost Samsung \$4.9 bn (Rousseau, 2017)<sup>14</sup>. This crisis has cancelled the efforts of a promising development program which planned to sell 20 million units. In addition, the failure also negatively impacted Samsung reputation. (<https://www.lesechos.fr/2017/01/samsung-accuse-les-batteries-de-ses-fournisseurs-dans-le-fiasco-du-galaxy-note-7-160217>)

The effects of the unreliable MCAS system introduced by Boeing on its 737-MAX are also considerable: two crashes and 346 deaths, slowed down, and then idle production lines, Boeing’s 2019 revenues down by 24% versus 2018, a net loss (-\$636 bn) in 2019 (Boeing 2019 Annual Report, 2020), while the firm had been generated net profits since 1997, etc.

### Functional expansion phenomena: another form of unknown with impact?

As mentioned above, disruptive innovation is clearly a form of ‘unknown with impact’ (which disqualifies traditional economic value management approaches).

The conventional wisdom is that the emergence of this kind of ‘unknowns with impact’ which break the legacy identity and architecture of a product is punctual and rare along the evolution dynamics of a product with time. In other words, the conventional wisdom is that most of a product evolution dynamic is driven by the improvement and / or combination of already-existing functions, and that punctually, new disruptive functions are introduced and renew product identity (playing the role of punctual ‘unknowns with impact’). This representation reflects Lancaster's (1966) theory according to which the evolution of goods results from the combinations and recombinations of a given fixed set of functions.

But recent research works have demonstrated that in the case of some consumption goods (among which the smartphone, the vacuum cleaner, the iron, the toothbrush, etc.), the product evolution dynamics is not only driven by the improvement and / or combination of already existing functions (El Qaoumi, 2016 ; Le Masson et al., 2018): **the number of functions contained in the product increases according to a pattern which corresponds to a continuous emergence of new functions with time. Such patterns are called functional expansion (El Qaoumi, 2016).** If one considers each function as one edge of a graphic matroid, different possible evolution scenarii of the matroid rank can be associated with different regimes of functional expansion (El Qaoumi, 2016 ; Le Masson et al., 2018). One of these regimes is called 'endogenous expansion' where endogenous means that the functional change does not result from exogenous / external events (e.g. market-pull or techno-push dynamics, regulatory requirements) but from a dynamic of design that is internal to the product. Empirical tests reveal that most of the studied consumption goods (some of which the identity is a priori stable) follow this regime of endogenous expansion (El Qaoumi, 2016) the pace of which accelerates from the 1990s (Le Masson et al., 2018).

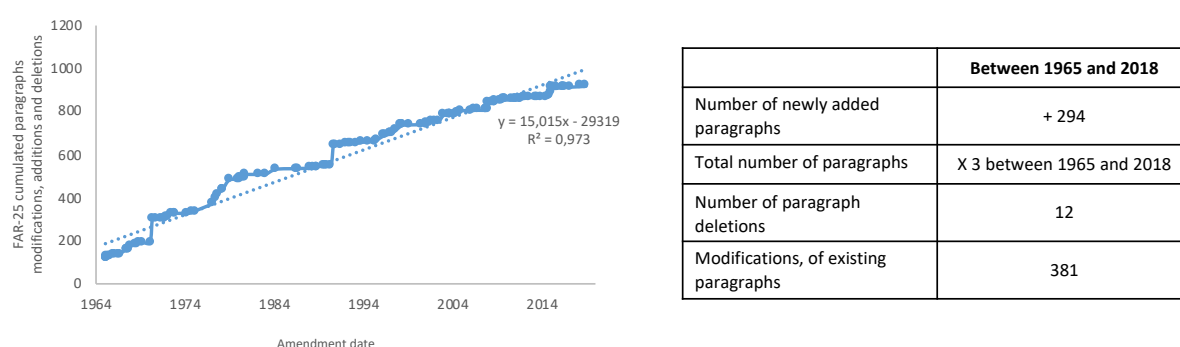
With respect to the definition of the unknown given in subsection 1.2, the new functions that emerge in the framework of an endogenous functional expansion dynamics correspond to the emergence of new, initially unknown  $\theta_i$ , i.e. to the emergence of unknown. And this phenomenon questions what ‘with impact’ is and what ‘disruption’ is in the framework of a product evolution dynamics. Indeed, the new functions part of a functional expansion phenomenon do not entail disruptions in the classical sense of the term, they do not continuously disrupt product identity. But are they still unknowns with no impact? What is the guarantee that this form of unknown does not hide more large-scale effects at the level of design activities, which themselves may entail substantial (socio-)economic impacts? Could the new functions that emerge in the framework of a functional expansion dynamics be an ‘unknown with impact’? Could there actually exist two forms of disruptions: (i) the well-known form corresponding to the new functions which obviously disrupt the identity and architecture of a product, and (ii) the ‘functional expansion’-form corresponding to new functions which preserve the identity and architecture of a product, but which have a disruptive impact in another manner?

The idea that some functional changes that are not obviously visible at the level of the product overall architecture (and identity) may actually be the source of substantial impacts has

already been mentioned in the literature related to CoPS (Complex Products and Systems). For instance, Hobday (1998) notes that ‘what appears to be incremental evolution at the system performance level can mask substantial discontinuities at the component level’. In other words, some phenomena ‘with impact’ may actually be hidden behind a seemingly stabilized product architecture. In the literature on complex systems, a paradigm of ‘continuously increasing complexity’ from one product generation to the next one has been described. According to Alderson and Doyle (2010) and Carlson and Doyle (2005), a complex system and its ‘parent’ simpler system fulfil the same basic functionalities (i.e. there is no obviously visible disruption): the evolution from the parent system to the next generation system offers an increase in the level of robustness of the basic functions (e.g. a wider window of operation, such as the possibility to operate in more extreme weathers for an aircraft). But this increase in robustness is accompanied by an increase in complexity: in particular, the new, more robust system is also more vulnerable to emergence phenomena, that is unexpected behaviors resulting from unpredicted interactions between some subsystems which were not supposed to interact –emergence phenomena (which are generally associated with rare and catastrophic events) can be seen as a form of ‘high impact’ associated with no obviously visible disruptions.

In sum, the literature on complex products reports examples of ‘not obviously visible unknowns having an impact’ which occasionally arise. But to our best knowledge, it does not mention dynamics that would involve a **continuous emergence of ‘not obviously visible unknowns with impact’**. Could functional expansion be a continuous emergence of ‘not obviously visible unknowns with impact’?

We have found that commercial aircraft (which are complex products with a stabilized architecture) were affected by functional expansion, i.e. by a continuous emergence of ‘not obviously visible unknowns’ (cf. Figure 3).



**Figure 3. Commercial aircraft functional evolution, retraced in terms of cumulative number of newly added, rewritten or deleted paragraphs in FAR-25, which is the American regulatory basis that a commercial aircraft must satisfy in order to be certified (Source: Appendix 1 of Paper 1 appended at the end of this document: using the statistical method which enables to detect the presence of functional expansion (Le Masson et al., 2019), we have found that the above pattern of increase corresponds to a dynamic of endogenous functional expansion)<sup>15</sup>**

<sup>15</sup> Remark: the curve above, which has enabled us to detect the presence of functional expansion in commercial aircraft, has been built by counting the cumulative number of newly added, rewritten or deleted paragraphs in FAR-25, which is the American regulatory basis that a commercial aircraft must

Are these not obviously visible unknowns ‘with impact’? It seems interesting to investigate this question, all the more so as if functional expansion is found to be ‘with impact’, we could ask whether there could be a link between the above-mentioned design crises and functional expansion.

A means to clarify these questions would be to test the presence of ‘unknown with impact’ in rule-based design, by measuring unknowns (e.g. measuring the economic impact of functional expansion) in rule-based design.

#### **Empirical motivations**

On the one hand, technique(s) enabling to quantify the value of new, initially unknown  $\theta_i$  (i.e. the value of information of the unknown) would be a ‘plus’ in situations involving the revision of the identity of a product, where unknowns with impact are already obviously visible (and where one already knows that their impact will be substantial). This would contribute, as a complement, to the management of the value of the unknown in these kinds of projects.

On the other hand, functional expansion suggests that ‘not obviously visible unknowns’ may be continuously emerging in a hidden way in rule-based design. It appears critical to develop techniques that enable to detect them and quantify them, in order to get a better understanding the extent of their ‘impact’ (in terms of order of magnitude). Indeed, hidden ‘unknowns with impact’ could be associated with detrimental scenarii. For instance, a decision-maker unaware of the presence of ‘not obviously visible unknowns with impact’ could launch (with a decision theory-based investment reasoning) a project that is seemingly profitable and decidable – and this project could turn out to be affected by some undesirable effects when the initially discreet unknown comes up (if it is an ‘unknown with impact’).

The other way round, if a decision-maker faces an undecidable situation (e.g. a seemingly unprofitable project) from the viewpoint of decision theory, such that no unknown with impact is obviously visible, but such that he or she thinks that there is room for designing unknowns the impact of which could enhance profitability, how to highlight them? And how to integrate them within a managerial logic where they would be treated a valuable resource? With which tools?

#### 1.4. Research questions

The identified empirical and theoretical motivations lead us to phrase two research questions.

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satisfy in order to be certified. We have built this curve thanks to the data of an historical database owned and regularly updated by a department of Airbus responsible for Airworthiness. This curve captures the functional evolution of one functional sub-space of commercial aircraft: indeed, we only count the changes that affect safety-related functions. This means that the functional expansion phenomenon observed in this case is only a lower bound of the total functional expansion that affects commercial aircraft.

**(RQ1): To what extent can the unknown that emerges in rule-based design be ‘with (economic) impact’?**

Addressing this question will contribute to the topic of *‘economic evaluation in the unknown, in rule-based design’*. In the first instance, the intuition is that there is no (or only occasional) unknown with impact in rule-based design, and that there is no need to develop techniques to evaluate the unknown in this context. But the phenomena mentioned above (rule-based design crises, functional expansion, etc.) raise the doubt. And the purpose of (RQ1) is precisely to remove doubt. *In the framework of (RQ1), we will attempt to build an instrument that allows observing the ‘discreet’ unknowns that may be present in rule-based design and measure their impact.* If the impact turns out to be non-negligible, this will mean that ‘economic evaluation in the unknown, in rule-based design’ is a genuine topic, and that in rule-based design organizations are in need for tools allowing to assess and manage the economic impact of the unknown.

**(RQ2): In the framework of projects that are undecidable from a decision theory-based viewpoint, to what extent and with what kind of tools could ‘unknowns with impact’ be the objects of a design theory-based economic value management, in particular an informed investment decision-making reasoning that is not based on a betting logic?**

Addressing this question will contribute to the topic of *‘economic evaluation in the unknown, in innovative design’*. It focuses on the projects which conventional economic calculation advises against launching (i.e. they appear either unprofitable or undecidable), but which a decision-maker decides to launch, with the purpose of adopting an innovative design approach where he or she will design unknowns with impact, whose impact can generate extra value. *In the framework of (RQ2), we will attempt to design an instrument to measure the additional value brought by the unknown. And we will focus on the possible tools to steer this value in the unknown.*

The research work has started shedding light on these questions by studying three empirical economic phenomena in the commercial aircraft industry and mobilizing of design-theory models. In the framework of this approach, we refined (RQ1) and (RQ2) in more specific sub-questions that we addressed in three papers.

Within (RQ1), we built an instrument enabling to assess whether a specific form of unknown, that is ‘functional-expansion-induced unknown’, is an ‘unknown with economic impact’. Paper 1, appended in this document, is dedicated to this investigation.

Within (RQ2), we focused on two investment cases where: (i) the presence of ‘unknown with impact’ is not obvious, (ii) an investment was realized although the projects were due to be unprofitable according to conventional economic calculation, (iii) the projects generated substantial economic returns. These cases are extremely puzzling with respect to traditional investment decision-making models. So, we built an instrument in order to detect whether or not some hidden ‘unknowns with impact’ are contained in these investment cases and could have played a role in the generation of the economic returns, and we double-check with empirical case studies. Papers 2 and 3 are dedicated to these investigations.



This document is dedicated to both reporting the results of the papers, and introducing the new insights developed regarding (RQ1) and (RQ2). The next subsection specifies the outline and the content of the thesis.

### 1.5. Outline and content of the document

This document is structured as follows: Section 2 summarizes the literature background which the three papers of the thesis rely on, which allows us to introduce their respective research questions (which are sub-questions of RQ1 and RQ2). Section 3 is dedicated to the presentation of the material and method mobilized in this research work: an intervention research carried out within Airbus, in the framework of which we used a theoretical simulation method, a statistical method and a case study method. Section 4 summarizes each individual paper. Then, Section 5 takes a step back and presents the findings obtained regarding our two research questions on economic calculation in rule-based design and innovative design. Finally, Section 6 discusses these findings and their implications (in particular their organizational implications).

## **2. Literature background: the economic impact of functional expansion ; investment decision-making models in the face of ‘unknown with impact’**

### 2.1. Functional expansion, an unknown with economic impact? [Paper 1]

The following sections suggest that one way to assess the extent to which functional expansion is an unknown ‘with impact’ could consist in trying to capture the cost impact of functional expansion (subsection 2.1.1.). But interestingly enough, no instruments seem to exist to assess the costs induced by newly added functions (subsection 2.1.1. and 2.1.2).

#### 2.1.1. The quantitative relationship between product functionality and product development effort

Extensive literature is dedicated to the fact that, in several industries, the costs related to development programs (i.e., development costs, unit production costs, and by implication, unit prices) are subject to increasing trends in the form of cost escalation (i.e., cost increase from one product generation to the subsequent generation) and cost growth<sup>16</sup> (i.e. cost overrun with respect to the initially targeted costs). This concerns, for example, military aircraft, helicopters, ships, submarines, battle tanks, and commercial aircraft. For instance, Bongers and Torres (2013) found that the costs of U.S. jet fighter aircraft have increased by 12.6% per year since 1944. Particularly in the defense sector , this cost evolution gave rise to a large number of studies that aimed to improve the understanding of the driving forces that underlie cost increases and considered leverages that could slow down these cost increases (Augustine 1983; Eskew 2000; O’Neil 2011; Arena et al. 2008; Cancian 2010; Dobson 2014; Hove and Lillekvelland 2014). In these studies, the significance and plausibility of potential

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<sup>16</sup> Cost growth generally undermines the ambitions that contemplate a slowdown of cost escalation curves with time

cost factors was investigated and discussed. Among these factors are, for example, inflation, changes at product level, requirement volatility involving programs that deviate from their initial target, management errors, poor program execution, subcontracting issues, unrealistic cost estimates, and flawed initial concept caused by over-optimism and non-progressive application of new technologies in the framework of a continuous race for superior technologies, leading to development problems.

Functional upgrades turn out to be a major cost factor. Arena et al. (2008), Bongers and Torres (2013), and Hove and Lillekvelland (2014) found a strong correlation between the upgrade of existing product characteristics (e.g., range, cruising speed, maximum speed, and whether the aircraft is carrier-based) and cost escalation. However, in these studies, the impact of newly added functions was not quantitatively discussed. Arena et al. (2008), and Hove and Lillekvelland (2014) used a regression vector that involved the same variables over the studied cost escalation period and that could not be extended to include newly introduced product characteristics. Bongers and Torres (2013) used hedonic prices in a Lancasterian framework, where the only possible product change scenarios are the optimization and/or combination of existing characteristics (i.e., a fixed-size functional space that does not allow the emergence of new product characteristics). Arena et al. (2008) explicitly mentioned the omission of newly added functions, referring to 'other elements' that could not be taken into account in their regression analysis (i.e., changes in avionics, software implementation, and product longevity) because the variables for measuring them are only available for recent systems and not applicable to older systems.

Despite this, at a qualitative level, Arena et al (2008) interviewed military aircraft programs' prime contractors regarding the topic of cost escalation: the interviewees reported that new innovative characteristics in military aircraft (e.g., stealth, software-controlled systems, and new mission equipment) are important contributors to cost escalation. Cancian (2010) also stressed that the excess cost (i.e., cost growth) incurred during the course of a military program may actually be the funding of the development of additional military capabilities that were not planned in the initial concept, and that may give extra value to the product. **These are explicit references to non-negligible costs that seem to have contributed to funding the development of new product functions. However, these facts are not addressed in quantitative terms.**

In addition, the relationship between functionality and cost is also addressed in parametric models that aim to provide estimates of design effort. Some models (Function Points (Albrecht (1979), COCOMO II (Boehm et al., 2000), cited in Fenton and Bieman (2014)) measure the functionality of a software, i.e. measure its functional size (by counting items of various types contained in the specifications), in order to provide estimates of development costs and time. In the case of hardware, Bashir and Thomson (2001) propose parametric models involving product functionality (measured with a complexity metric) and requirements severity as parameters to estimate the design effort. According to these estimation models, the addition of new functions contributes to increased design costs. However, the statistical relationships they involve do not go as far to account for *how* a continuous emergence of new functions affects design work and generates additional development costs.

In the following subsection, we aim at determining whether the literature on engineering changes and change propagation phenomena provides additional insights regarding the contribution of new product functions to costs, and regarding how to measure it.

### 2.1.2. The cost of change propagation

The literature on engineering changes distinguishes two types of engineering changes.

(1) Intentional engineering changes are developed and implemented by design engineers to answer market dynamics (e.g., customer demand for more performance), respond to regulatory requirements, or follow the emergence of new technologies. (2) While the intentional engineering changes concern targeted entities of the architecture (e.g., targeted functions, subsystems, and components), they may propagate to other entities within the architecture, and trigger additional changes (e.g., rework and redesign) in these other entities (Clarkson et al. 2004): this phenomenon is called change propagation. Intentional changes may also lead to emergence phenomena, which correspond to unanticipated interactions between entities that were not expected to affect one another. Such emergence phenomena can result in potentially undesirable effects in the product and lead to additional work (e.g., building ‘barriers’ to prevent interaction between the newly interdependent entities) (Carlson and Doyle 2005; Alderson and Doyle 2010). If emergence or propagation effects arise, their impact on a project can be dramatic (e.g., an ‘avalanche’ of propagated changes, as theorized by Eckert et al. (2004), can have catastrophic consequences in terms of costs and delays).

There exist models which compute the cost of change propagation (Georgiades et al. 2017 ; Rebentisch et al., 2017). These latter rely on Clarkson et al.’s (2004) seminal matrix-based change propagation method (CPM), which enables propagation paths to be traced within a design structure matrix (DSM). The CPM (Clarkson et al. 2004) is a probabilistic change propagation model that quantifies propagation in terms of likelihood, impact, and risk. As illustrated in Figure 4, based on DSMs, the CPM involves a propagation likelihood matrix  $\mathbf{L} = (l_{ji})_{1 \leq i, j \leq n}$ , which represents the probability  $l_{ji}$  that a change in entity  $i$  (e.g., a subsystem)<sup>17</sup> propagates to another entity  $j$  with which it is interfaced: if an interaction between  $i$  and  $j$  is highlighted in the DSM, then  $0 < l_{ji} \leq 1$ , and if there is no interaction, then  $l_{ji} = 0$ .

The likelihood matrix  $\mathbf{L}$  only covers ‘direct’ propagation paths, that is, propagation paths between interfaced entities. However, indirect propagations (with a longer-than-one propagation chain, where an entity plays the role of a bridge between two entities that are not directly interfaced) may also occur. As shown in the third matrix from the left in Figure 4, the CPM offers the ability to predict a ‘combined propagation likelihood,’ which aggregates the occurrence probabilities of all propagation paths between two components, including longer-than-one paths, using the forward CPM algorithm (Clarkson et al. 2004); forward CPM is a brute-force search algorithm that considers all possible propagation paths individually. More recently, Hamraz et al. (2013) proposed a matrix-based algorithm that, for a given integer  $k$ ,

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<sup>17</sup> Note that most studies that use DSM and CPM are based on component DSM. However, because this study specifically focuses on functional expansion and upgrades, we chose to represent functional-DSM in Figure 4 and all the DSM used in this work

computes  $cl_{ji}^{(k)}$ , which is the probability that a change in  $i$  propagates to  $j$  through propagation paths with lengths of up to  $k$ .

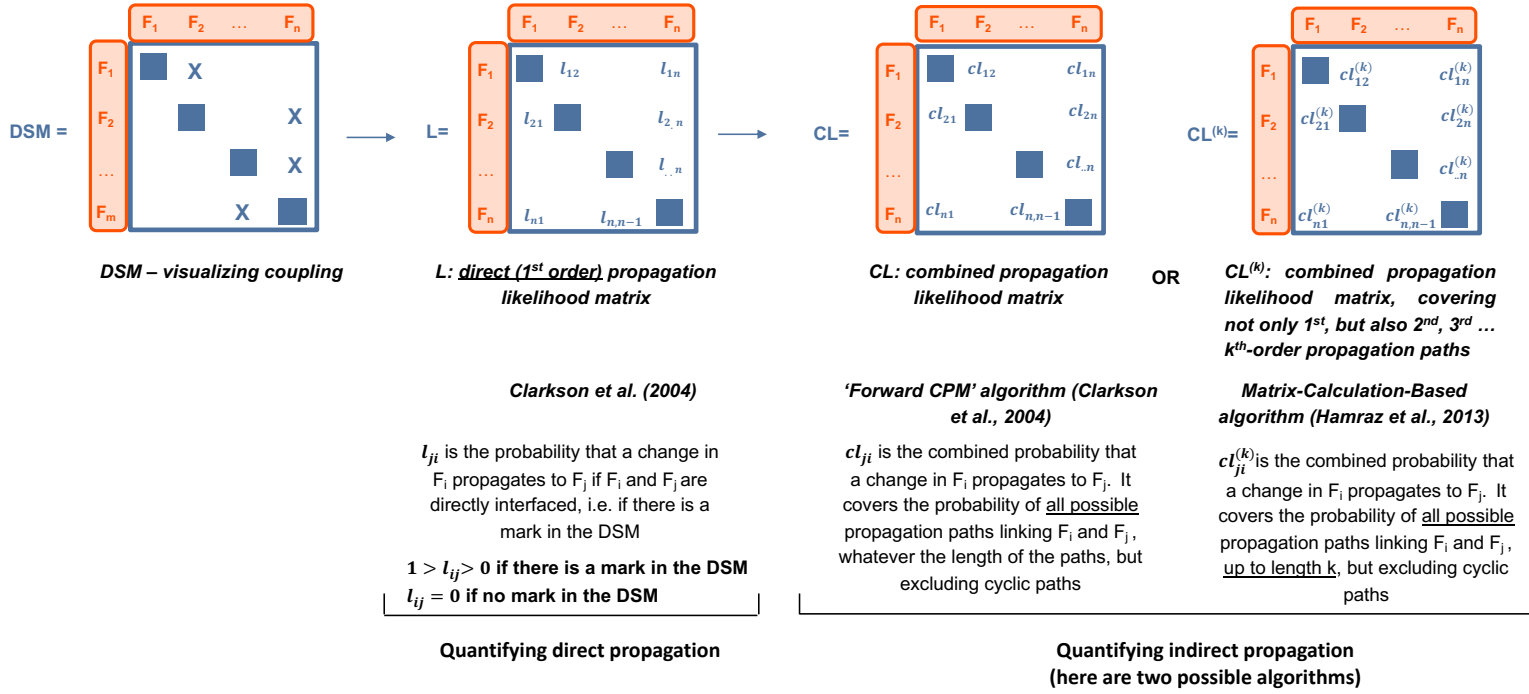
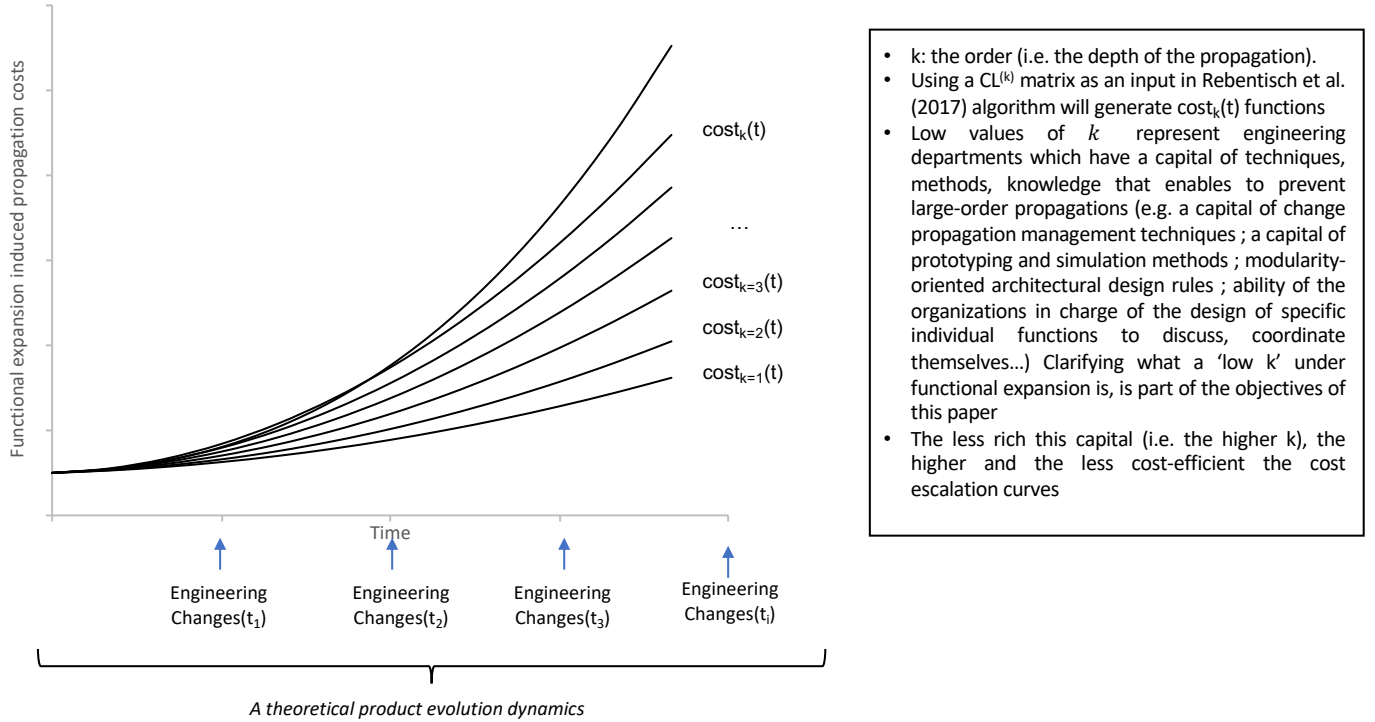


Figure 4. Direct Propagation Likelihood matrices ( $L$ ) and Combined Propagation Likelihood Matrices ( $CL$  and  $CL^{(k)}$ ) that are the elements of CPM involved in Georgiades et al. (2017) and Rebentisch et al. (2017)

By combining a  $k$ -order combined propagation likelihood matrices with a vector featuring the individual cost of each entity, one can compute the cost of an engineering change as the sum of:

- the nominal cost of the intentional change
- the cost of the propagated changes induced by the cost of the intentional change

And if we consider a theoretical sequence of intentional engineering changes, we can generate of family of theoretical attainable cost escalation curves, using the Rebentisch et al. (2017) algorithm.



**Figure 5. Possibility to generate a family of cost escalation curves, from the least cost-effective (the highest curves; high values of  $k$ ) to the most cost-effective (the lowest curves; low values of  $k$ ) using the Rebentisch et al. (2017) algorithm**

What is the nature of the changes covered by cost propagation models? Is functional expansion addressed?

The CPM (as most, if not all, propagation models) is designed to help to manage increasing complexity, that is, to help to handle products for which the number of elements (e.g., parts and components), level of interconnectedness, and vulnerability to emergence and propagation tend to increase. Such an increase in complexity results from the fact that products are required to be increasingly high-performing, and must therefore be continuously improved by engineering changes (Clarkson and Doyle 2002; Alderson and Doyle 2010; Clarkson et al. 2004; Rebentisch et al. 2017). These improvements and changes that render products increasingly complex include upgrades to existing characteristics. By contrast, it is not clear whether the addition of new, innovative entities is also covered. Indeed, the CPM and propagation models generally analyze propagation within the composite entities of a product breakdown at one moment  $t$  in the product evolution dynamics; they do not distinguish between already-existing and newly added entities. More specifically, to the best of our knowledge, they do not compare the propagation likelihood matrix at time  $t$  with the previous matrix at time  $t - 1$ , and thus do not highlight whether some of the entities contained in the matrix at time  $t$  might have been absent at time  $t - 1$ ; that is, newly added entities may very well be present in the breakdown considered by propagation models at time  $t$ , but they are neither identified as 'new' nor managed in a different manner to already-existing entities.

Thus, with cost propagation models, the literature dedicated to engineering changes offers an instrument which enables to observe and measure the economic impact (the cost impact) of functional upgrades along a product evolution dynamics. This instrument does not assess the cost impact of newly-added functions. But it can be easily extended for this purpose. Indeed, one only has to consider DSM that increase in size over time. This is what Paper 1 is devoted to, and enables us to address the three following questions (which are three sub-questions for RQ1).

**(RQ1): To what extent can the unknown that emerges in rule-based design be ‘with (economic) impact’?**

Sub-questions for (RQ1) focusing on assessing the cost impact of functional expansion

(RQ1-S1): What is the shape of ‘pure’-functional-expansion-driven development cost escalation curves? To what extent does this shape suggest that functional expansion can potentially have a large magnitude impact on development costs?

*This sub-question aims at isolating functional expansion and at determining how the different dimensions of functional expansion (the new functions themselves ; the propagation phenomena generated by the new functions) respectively contribute to cost escalation.*

(RQ1-S2): To what extent can well-proven modularity strategies slow down the cost escalation curves under functional expansion?

(RQ1-S3): With respect to functional upgrades that have already been demonstrated as a significant cost driver, under which conditions may functional expansion also be a significant cost driver (conditions under which functional expansion would be a priority in terms of cost management, ahead of controlling the costs of already-existing functions)?

With these sub-research questions, we do not aim at modelling very precise cost curves that would approximate the real cost curves of a specific product. Given the lack of knowledge regarding the cost impact of functional expansion, we rather aim at getting a first understanding of *the order of magnitude of functional expansion cost impact*. Therefore, we will try get first insights regarding the behavior of a specific type of costs, that is propagation costs, under functional expansion: we will generate cost curves within this restricted perimeter and investigate which insights these curves provide regarding (RQ1-S1), (RQ1-S2) and (RQ1-S3).

## 2.2. Investment decision-making models in the face of ‘unknown with impact’ [Papers 2 & 3]

### 2.2.1. From investment decision-making under uncertainty to investment decision-making in the unknown

Under uncertainty, i.e. in the face of identified, but uncertainly known states of the world, decision theory (Wald, 1945, 1949 ; Savage; 1954 ; Raiffa, 1968) and deriving techniques enable to reduce and eliminate uncertainty thanks to the collection of new information. In an

investment decision situation, one can use these techniques in order to select the utility-maximizing choice among: ( $d_1$ ) investing in the project, ( $d_2$ ) killing the project and ( $d_3$ ) launching an uncertainty-reducing pre-investment trial, with a trial approach (e.g. real options (McGrath, 1996), discovery-driven planning (McGrath and MacMillan, 2009)...) so that one can make a better-informed decision later on.

Decision theory-based investment decision models inform very rigorously the choice between these three alternatives, by highlighting the expected-utility-maximizing decisional way. In this context:

- The decision criteria are clear and quantifiable: one can quantify expected utility (deduced from expected profitability, expected Net Present Values, etc.). And one can quantify the value of uncertainty reduction, i.e. the gain of utility one can hope to earn if one chooses to collect additional information in order to reduce uncertainty (i.e. the pre-investment trial decisional way).
- Expected utility depends on the intensity of the uncertainty contained in the decisional situation: the more uncertain a project is, the lower the probability of making it profitable, and the more intuition-based and the riskier the decision to invest. Therefore, in decision theory based models, investments in highly uncertain projects are equivalent to risky gambles. They might generate substantial return, but with a very low probability of occurrence. Based on this decision-theory-based view, organizations are often reluctant to invest in highly uncertain projects.
- If one decides to invest in a pre-investment trial ( $d_3$ ), the perspective of making profits depends on the reliability of the 'tool' which enables to collect and 'observe' additional, uncertainty-reducing information (appropriate experimentation means, trial-and-error learning principles...). In addition, the possibility of reaping the expected profitability depends on the decision-maker's capacity to manage and organize uncertainty reduction, with the appropriate tools, processes, methods, capabilities, etc.

In summary, investment decision models based on decision theory rely on a quantified uncertainty-reduction logic. The uncertainty is very rigorously managed and controlled with a quantified technique.

But in the unknown, that is if the target associated with the investment is unknown (i.e. if one is unable to make sufficiently reliable estimates in terms of costs, benefits, technical feasibility, potential market, etc.), the third decisional way  $d_3$  encounters serious issues. Indeed, a trial-based search is likely to be vain. Be the trials sequential or parallel, it will be like taking gambles on small steps (Kokshagina, 2013): this is likely to be time and resource-intensive before. The unknown does not call for updating the state-space and its probabilities, but for designing new states of the world (Hooge et al., 2016 ; Feduzi and Runde, 2014). Gillier and Lenfle (2018) demonstrate that the unknown requires specific experimentation principles, adapted to 'expandable search' which differ from Thomke's experimentation principles. But decision theory-based models do not assess the value associated with a search in the unknown: experimentation instruments and their accuracy being given, the value of the search is the value of the update of already-existing information. The value associated with the emergence of new states of the world is not counted.

In sum, the uncertainty reduction mechanism allowed by decision theory-based models is neither helpful nor appropriate to guide decisions and manage the generation of profitability

in the unknown. The third abovementioned decisional alternative ( $d_3$ ) ('launching an uncertainty-reducing pre-investment trial so that one can make a better-informed decision later on') is no longer an alternative.

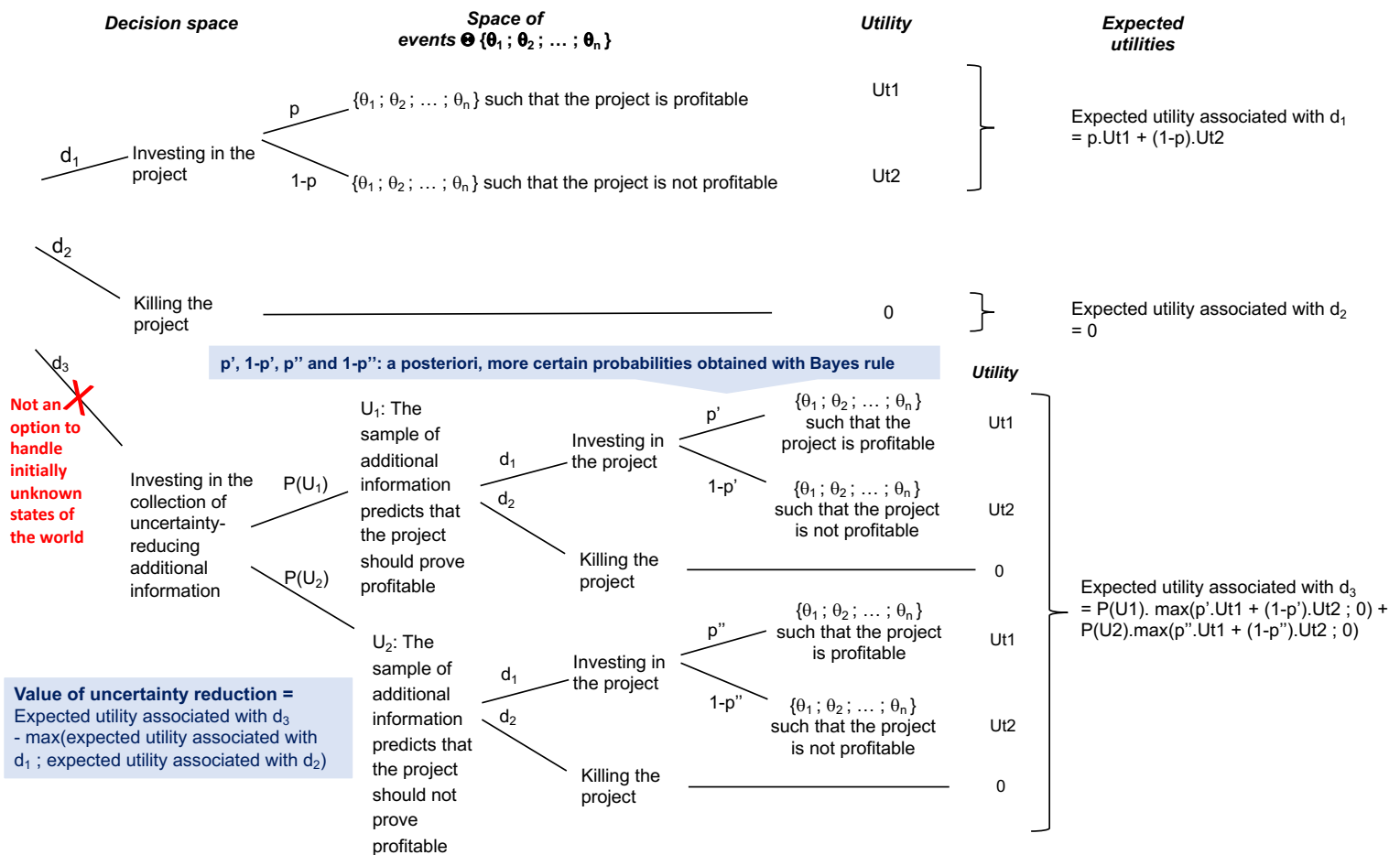


Figure 6. Decisional situation faced by a decision-maker in the unknown

If  $d_3$  is no longer a decisional alternative, decision-makers and managers are confronted to a kind of dilemma between  $d_1$  and  $d_2$  in the unknown:

- Either they invest ( $d_1$ ), but an informed way, i.e. in a 'pay and let us see what happens' philosophy, which boils down to take a very risky gamble
- Or they kill the project ( $d_2$ ), i.e. they do not run the risk of not getting a return on their investment, but they run the risk of missing promising opportunities

One can note that decision theory, as a rational theory of choice, will designate the first option as the utility-maximizing option and advise for killing the project. But both options are unsatisfactory.

In the face of this unsatisfactory dilemma, two kinds of alternative investment decision models can be considered.

### 2.2.2. First class of alternative investment decision models in the unknown: Investment decision model based on lightened economic criteria and enriched with new criteria



A first class of alternative investment decision models temporarily circumvents the profitability criterion and enrich the set of decision criteria with new criteria (quasi-quantitative, qualitative...). Within these models:

- The set of decision criteria is specifically designed for highly innovative projects<sup>18</sup>, it is not fixed<sup>19</sup> and it is made of non-monetary criteria. Strategic fit, customer relevance, communication potential, competitive potential, future business potential, etc. are examples of criteria which can be found in such sets (Dziallas, 2020 ; Martinsuo and Poskela, 2011 ; Hart et al., 2003). These criteria are informed through more or less formal means (from analysis grids to conversational mode) and may include perception-based assessments (Shenhar et al., 2001), expert evaluation (Chang et al., 2008)...
- Expected profitability is not quantified, since the profitability criterion is circumvented. However, this type of evaluation framework does not abandon profitability ambitions. The application of the set of criteria is expected to select projects that will open new business opportunities, attract new customers... for the future, and thus will pay off later on (Shenhar et al., 2001 ; Frederiksen et Knudsen, 2017).
- The possibility of making profits which such decision models will depend on whether or not the project passes the later phases of the stage-stage process, when the profitability criterion is to be reintroduced.

These alternative models propose an investment decision approach which involves a 'lightened' economic management. In this regard, Dziallas (2020) even advocates for lightened (more flexible and faster) resources allocation procedures (arguing that the delays of 'traditional' budget allocation processes may be long and may threaten innovation opportunities). The circumvention of the unknown and the use of additional decisional criteria enable to bring back the decisional situation to a situation where decision theory applies again. So the investment decision is well-informed, based on the alternative criteria. It is not gambling-based. And one can note that information which will enable to check whether the criteria are met are being collected and fuel an uncertainty reduction mechanism. In other words,  $d_3$  is restored for criteria other than the profitability criterion.

But such an investment decision model does not restore in the unknown the level of economic rigor that decision theory-based models offer under uncertainty. Indeed, the unknown related to profitability is not quantified: it is circumvented. In addition, it is not guaranteed at all that the innovation project will pass this step. In this regard, recent works show that such a move from exploration to exploitation (in the sense of March's (1991) distinction) is all but automatic, and that having an impermeable frontier between the objectives of exploration on the one side, and the objectives of exploitation activities on the other side may actually be a threat to innovation (Le Glatin, 2018 ; Le Glatin et al., 2018).

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<sup>18</sup> According to Schmidt et al. (2009), one should use different sets of evaluation criteria depending on whether one assesses incremental innovation or radical innovation projects – i.e. specific evaluation criteria should be dedicated to highly innovative projects (and hence projects which may contain unknown).

<sup>19</sup> The set of criteria may vary depending on the strategic objectives of the firm (no one-size-fits-all set of criteria) (Hart et al., 2003 ; Martinsuo and Poskela, 2011)

### 2.2.3. Second class of alternative investment decision models in the unknown: Design-theory based investment decision models

As mentioned in Section 1, it was recently demonstrated that design theory extends decision theory to the unknown (Le Masson et al., 2018): moving from a decision-theory-based reasoning to a design-based reasoning when it comes to make decisions in the unknown means that a decision problem in the unknown no longer consists in identifying the best choice among a set of possible alternatives, but in designing a new, better decision space, either by designing new decisions (better than the already existing ones) or designing new states of the world (that will lead to reconsider the preference ranking of the decisions that can be possibly taken, or that will increase / decrease the expected utility associated with all the decisional ways).

Managing an investment decision in the unknown with a design theory-based decision model would provide decision-makers with the possibility to either design a new ‘investing’ decisional way  $d_4$  which meets profitability requirements or design states of the world which render the ‘investing’ decisional way  $d_1$  profitable. This suggests that theoretically, in the frame of a design-based economic reasoning in the unknown, it is always possible to reach one’s profitability target without necessarily throwing one’s lot with chance, if one designs the judicious states of the world or the judicious decisions (i.e. if one designs the judicious ‘unknowns with impact’) to this end.

This design theory-based prediction is a complete break with the conventional wisdom according to which the expected profitability associated with investments in the unknown is extremely low and that by implication, investments in the unknown are very risky gambles<sup>20</sup>. And it is extremely counterintuitive in the first instance. A literal interpretation of it suggests an investment decision model where any investment project in the unknown can be made profitable by anyone who uses a design reasoning. While such a literal interpretation is absurd and unrealistic, we can envisage a sounder investment decision model in the unknown, where any investment could be made profitable, albeit subject to two conditions:

- (i) **the investment is allocated to a project which is suitable for the design of new decisional alternatives or new states of the world**, i.e. to a project which contains unknown with (economic) impact. This raises the question of the decision maker’s capacity to detect and highlight non-necessarily obviously visible unknowns, i.e. to detect the fact that there is room for ‘unknowns with impact’ to be designed. In this respect, research works dedicated to the design of value in the unknown highlight the importance of discussing the sources of the unknown, which need to be supported and managed with appropriate tools (Hooge, 2020 ; Hooge and Stasia, 2016)
- (ii) **the decision-maker or other actors in charge of developing the project own the indispensable methods, tools, capabilities, processes, organization... to successfully design new states of the world or new decisions**. This raises the question of the suitable techniques to explore the unknown. In this respect, works

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<sup>20</sup> This conventional wisdom is based on an assimilation of the unknown to ‘an extreme degree of uncertainty’ and relies on the prediction of decision theory according to which the more uncertain a project is, the lower its expected profitability, and the more intuition-based the decision to invest.

dedicated to the questions of building and managing value in the unknown (Hooge, 2020 ; Hooge et Stasia, 2016 ; Gillier et al., 2014), experimenting in the unknown (Gillier and Lenfle, 2018), designing generic technologies in the unknown (Hooge et al., 2016), etc. stress the importance of being capable to rigorously manage unknown exploration, thanks to dedicated organizations, managerial approaches, tools, evaluation frameworks

Enriched with these two conditions, such an investment decision model seems more realistic. In addition, in comparison with the first class of alternative investment decision models described in the previous subsection, we can make the assumption that a design-theory based investment decision model could:

- offer the possibility to reintroduce expected profitability (or utility) in the set of investment decision criteria
- could allow to assess the value of unknown exploration, i.e. the value that one can expect to generate if one chooses to explore and structure the unknown, by building new states of the world or new decisional alternatives (i.e. by building unknowns with impact), in the same fashion as decision theory-based investment decision models assess the value of uncertainty reduction (i.e. the gain of utility a decision-maker can hope to earn if s/he chooses to collect additional uncertainty-reducing information)

(the  $d_3$  decisional way would have a kind of dual  $d_3'$  in the unknown, and a decision-maker in the unknown could choose the optimal decision between  $d_1$ ,  $d_2$  and  $d_3'$  after an expected utility calculation).

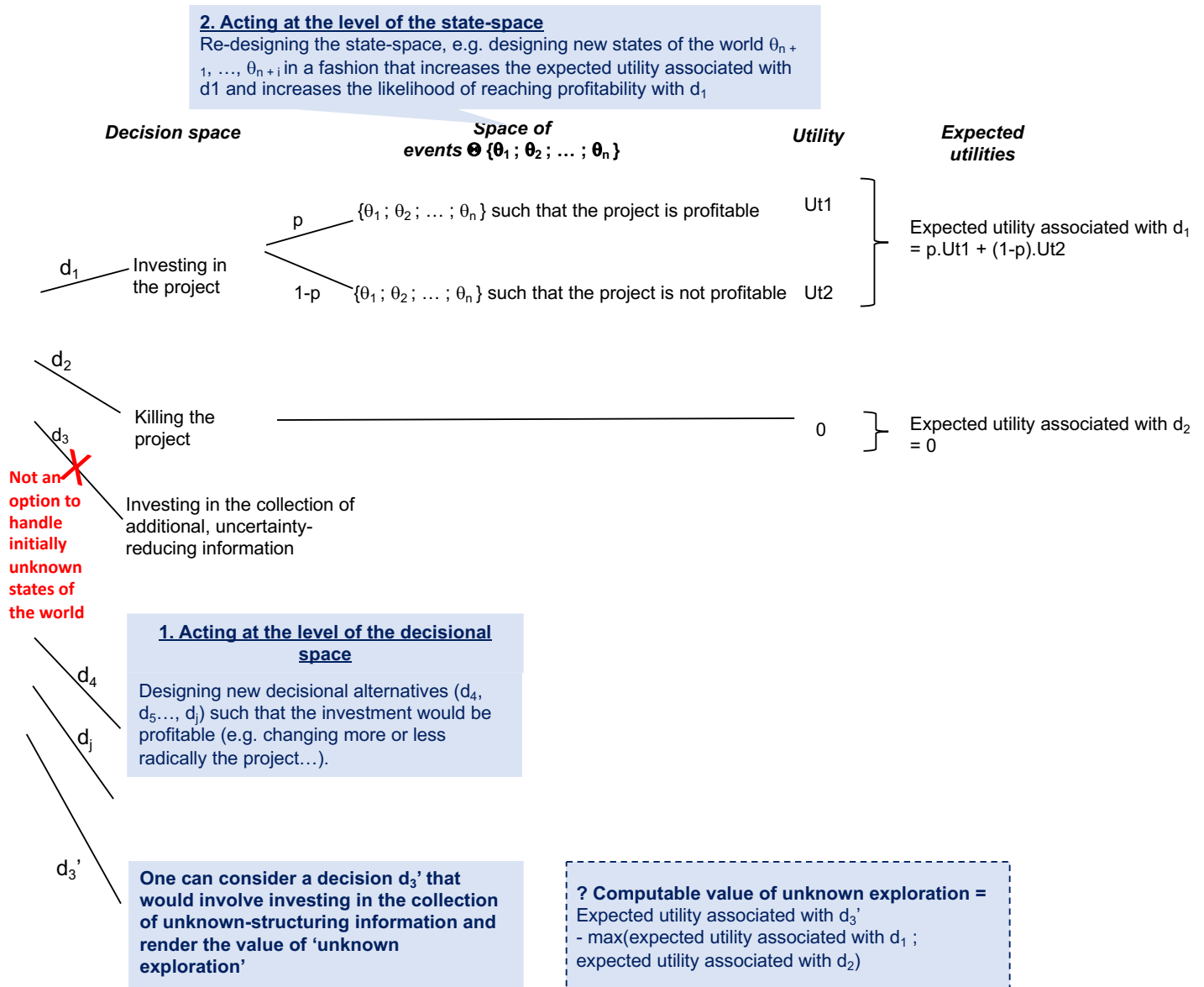


Figure 7. Alternative investment decision-making model in the unknown suggested by Le Masson and al.'s (2018) design theory-based decision-making model in the unknown

Such an alternative (to date hypothetical) investment decision model in the unknown would be particularly interesting and attractive, because it promises to generate a lot of profitability in the unknown in a rigorously managed way (from an economic management point of view, it would treat unknown exploration in a quantitative, computational way which is as rigorous as the way in which uncertainty reduction is treated by decision theory). Thus, it appears relevant to attempt to validate / invalidate this model. To that end, we need to identify real-world investment cases which we suspect fall within a design theory-based investment decision model. And we need to address the following sub-research questions, in order to determine whether the cases confirm the model.

(RQ2): In the framework of projects that are undecidable from a decision theory-based viewpoint, to what extent and with what kind of tools could 'unknowns with impact'

**be the objects of a design theory-based economic value management, in particular an informed investment decision-making reasoning that is not based on a betting logic?**

In the investment cases which we suspect fall within a design theory-based investment decision model:

RQ2-S1: Was the decision to invest [in the project] supported by the perspective of learning in the unknown? / Is the remarkable profitability [of the project] attributable to unknown exploration and ensuing impact?

RQ2-S2: How were project selection and unknown exploration carried out in the frame of the project? Do these practices confirm the assumptions which characterize our hypothetical design theory-based investment decision model in the unknown?

Papers 2 and 3 respectively study one investment case and both attempt to address S2-RQ1 and S2-RQ2.

As we will explain it in more detail in Section 3, RQ2-S1 requires to build an instrument that measures / detects the presence of unknown, while RQ2-S2 requires an empirical case study method which could double-check the answer to RQ2-S1.

### 3. Material and method

Based on our literature review and our research questions, we need material (empirical and / or theoretical), and methods to:

**(1) test whether the shape of cost escalation curves could be a relevant indicator of the impact of functional expansion** that would offer a better understanding of the characteristics of the unknowns which underlie functional expansion (unknowns with or with no impact?)

**(2) test the hypothetical design theory-based model of investment decision-making in the unknown introduced in subsection 2.2.3.** To that end, we need to design an instrument that detects the presence / absence of the unknown in investment situations, which would then allow us to test whether the situations that were found to contain unknown confirm the model.

The commercial aircraft industry and Airbus seem to offer a particularly relevant field in terms of material: this engineering intensive industry is highly concerned by the problematic of measuring and managing economic impacts ; and it is affected by a functional expansion phenomenon (Section 3.1). So this research has been lead in partnership with Airbus, in the framework of what can be seen as an intervention research method (Section 3.2). Within Airbus, we have identified anomalous phenomena which seemed hardly explainable with traditional economic calculation means, and in which we have suspected the presence of the unknown. We have studied these phenomena with different methods (theoretical simulation ; statistical method ; case study) which are explained in Section 3.3.

### 3.1. The commercial aircraft industry and its relevance to study the topic of ‘economic evaluation of the impact of the unknown’

Commercial aircraft belong to the category of Complex Products and Systems (CoPS): they are ‘high cost, high technology, software-intensive, engineering-intensive and knowledge-intensive goods made in projects and small batches’ (Hobday, 2000 ; Hobday and Rush, 1999).

Indeed, commercial aircraft count several million parts and they are made of hundreds of systems (flight control, propulsion, avionics, fuel, etc.) which are themselves individually complex. These latter must work under strong constraints (temperature, moisture, vibrations...). They must be integrated and installed in a physical space which is ‘small’ in comparison with other products (e.g. cars...) (Altfeld, 2010). They are required to work together so that the integrated aircraft fulfils its primary function, that is flying from a point A to a point B. But beyond that, aircraft manufacturers must certify that the aircraft will fly safely from point A to point B. Therefore, aircraft manufacturers have to carry out a large documentation effort, numerous, costly testing... in order to certify that newly developed products comply with high requirements in terms of safety, reliability and product integrity.

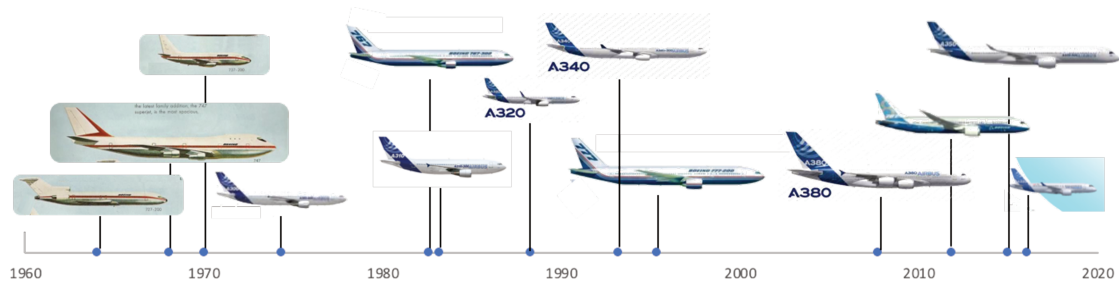
The design of these complex products requires a large workforce of engineers in program-based organizations. For instance, at the peak of the development of the A380, Airbus employed 6000 people on the program, without counting 34 000 people who were directly involved at suppliers (Altfeld, 2010). The overall development costs for these programs reach several billion dollars: for example, the development costs of the A380 are estimated at between 20 and 25 billion dollars, those of Boeing Dreamliner at around 40 billion dollars, etc. With these considerable investment volumes, launching one program has an impact on the cash reserves of the company, limiting the possibilities for launching other programs (Spitz et al., 2001), and hence leaving little room for errors in investment decision-making or in cost management. In this context, the measure of economic impacts, in the form of careful profitability analyses, costing analyses, etc. is indispensable to help rigorously manage economic performance. And commercial aircraft manufacturers are precisely massive users of economic calculation and economic management tools (well-established finance organizations, a large set of tools, indicators, methods, such as financial tools, business cases, costing models, controlling, etc.).

As with complex products in general, the cost of the early phases of commercial aircraft development programs (which involve the definition of design objectives, choice of architecture, etc.) represents only a small part of total development costs, but the decisions made during these phases are decisive for total development costs: architecting decisions in the framework of complex systems design represent less than 1% but determine 80% of the overall costs (Simmon et al., 2005, cited in Sinha, 2010). In addition to having high economic stakes, making decisions in the early phases of a complex product development project is itself ‘a complex phenomenon’ (Jankovic et al., 2009): numerous actors owning the knowledge and expertise associated with the different product (sub)systems are involved, and issues related to the ‘collaborative’ nature of the decision-making may be encountered, especially when it comes to define the project objectives, to define the project planning, etc..

For instance, the actors may contemplate incompatible local optimal, conflicting objectives. So Jankovic et al. (2010) stress that appropriate models and tools are needed to manage complex decision-making in these early phases.

In summary, this brief presentation of commercial aircraft highlights the fact that, from the aircraft manufacturers' point of view, it is very important to have models and tools to assess economic impacts and to manage decision-making.

At first glance, there is no ‘unknowns’ (in the sense of the definition given in Section 1.2) involved in the development of commercial aircraft, and therefore no unknowns to be taken into account in economic evaluation and in decision-making in the commercial aircraft industry: indeed, commercial aircraft manufacturers often evolve existing designs, by implementing engineering changes<sup>21</sup>. This results in a product evolution dynamics which is centered around a stabilized architecture, as Figure 8 below illustrates.



**Figure 8. Products developed by commercial aircraft manufacturers over the last decades**

This product evolution trajectory is driven by numerous refinements, e.g. aerodynamic improvements, fuselage reprofiling, flight control advances, weight-efficiency enhancements, etc. (van Heerden et al., 2019). Along this evolution trajectory, engineering departments apply, among other things, reuse, commonality, etc. design principles<sup>22</sup>. Interestingly, they have been present from the very beginning of the aircraft industry, being used by the Wright Brothers<sup>23</sup>.

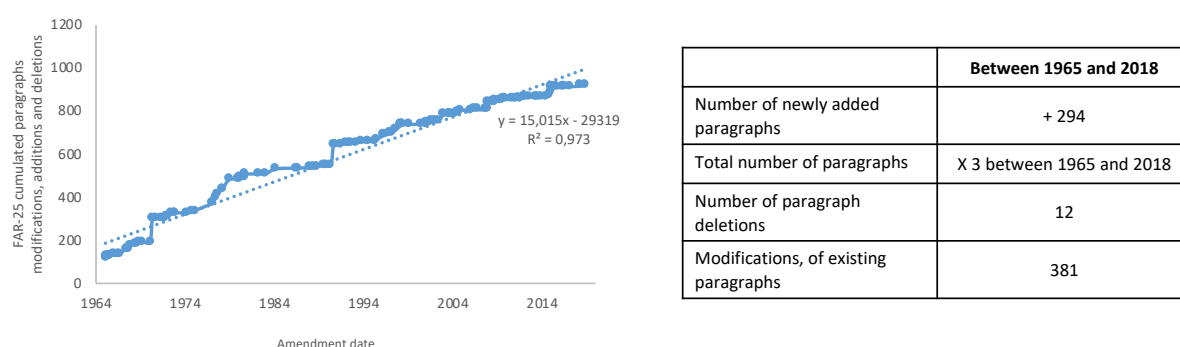
In brief, commercial aircraft are archetypal rule-based design products. And at first, it seems that either no new  $\theta_i$  is emerging, or the new  $\theta_i$  that are emerging are so controlled by engineering departments that they are ‘with no impact’.

<sup>21</sup> This is precisely related to the fact that starting a new commercial aircraft design from a clear sheet being extremely resource-intensive and risky (this is sometimes compared to betting the company) (van Heerden et al., 2019 ; Spitz, 2001)

<sup>22</sup> For example, Airbus A330/340, A380 and A350 share the same cockpit as the A320

<sup>23</sup> van Heerden et al. (2019) explain that: “the Wright Flyer III [...] reused the actual engine and most of the metal hardware from the Flyer II. [The Wright brothers] also re-adopted the wing camber of their famous 1903 design. However, the structure was stronger, the tail taller, the control surfaces featured more area, and the aircraft sat higher above the ground than its predecessors”.

But as mentioned in Section 1, behind their seemingly stabilized architecture, commercial aircraft are concerned by a continuous emergence of ‘hidden unknowns’ (in the form of new functions), in the framework of a functional expansion dynamic.



**Figure 3. Commercial aircraft functional evolution, retraced in terms of cumulative number of newly added, rewritten or deleted paragraphs in FAR-25, which is the American regulatory basis that a commercial aircraft must satisfy in order to be certified (Source: Appendix 1 of Paper 1 appended at the end of this document: using the statistical method which enables to detect the presence of functional expansion (Le Masson et al., 2019), we have found that the above pattern of increase corresponds to a dynamic of functional expansion)**

Thus, from the viewpoint of the commercial aircraft industry, it appears of utmost importance to determine whether some ‘non obviously visible unknowns’ are a variable ‘with impact’ that should be taken into account when evaluating economic aspects and taking decisions.

### 3.2. An ‘intervention research method’ within Airbus

This research work was carried out in the framework of a partnership with Airbus (a CIFRE contract<sup>24</sup> running from November 2016 to November 2019). We had the status of an Airbus employee within the department responsible for architecting and ensuring the synchronization of the different phases of the product development in the framework of the Product Development Plan, from program launch to aircraft entry into service.

Within Airbus, we have identified anomalous phenomena which seemed difficult to explain with traditional economic calculation models – and we have suspected them to involve ‘unknowns with impact’:

- a development cost escalation phenomenon affecting commercial aircraft which is puzzling for a rule-based design product. This led us to **develop an instrument for assessing the impact of functional expansion on cost escalation curves and start addressing RQ1 and its sub-questions (Paper 1)**. This phenomenon was brought to our attention at the kick-off meeting of this research work in Airbus, in October 2016, and the costing department has followed with interest the progress of our research on this topic during the PhD.

<sup>24</sup> Convention Industrielle de Formation par la REcherche (Industrial Convention of Training through Research)



- two investment cases where: (i) investments have been made although the projects were due to be unprofitable according to conventional economic calculation, (ii) the projects generated substantial economic returns. These cases are extremely puzzling with respect to traditional investment decision-making models. And managers who had been involved in these cases and / or were still involved in their follow-ups were interested in getting a model that would scientifically explain how these economic returns had been generated. In the face of these cases, **we built an instrument in order to detect whether or not some hidden ‘unknowns with impact’ are contained in these investment cases, and whether these cases could provide empirical confirmation for the design theory-based investment decision-making model introduced in section 2.2.3 (Papers 2 and 3 – one paper for one investment case).**

Building observation instruments that would isolate / highlight ‘not obviously visible unknowns’ and assess their impact was of interest both from the academic viewpoint and from Airbus viewpoint (with the above mentioned anomalies that can be seen as feelings of discomfort (Hatchuel and Molet (1986) expressed by Airbus). So we have led what can be seen as an intervention research (Hatchuel and David, 2008) within Airbus, although, beyond the steering committees that regularly gathered our academic supervisors and industrial supervisors, the ‘intervention’ side of the research is not obvious at first sight.

Indeed, the methods used to build the observation instruments and study the anomalous phenomena introduced above (which are explained in details in Section 3.3) mostly involve theoretical investigations and ex-post analyses of past projects with empirical data being collected from two main sources: an access to the internal documents related to past projects (minutes of meeting, meeting presentations, technical documents, etc.) (we would not have necessarily accessed them without a status of Airbus employee) ; and numerous and regular exchanges (interviews and informal discussions) with persons and teams related to the investigated topics.

Our implication in some projects or activities within Airbus (that were not necessarily related to cost escalation or the two investment cases mentioned above) helped us get a better understanding of the functioning of Airbus organizations. But with a few exceptions (participation in events or workshops related to our research topics, etc.), we rarely intervened on Airbus projects or activities with our researcher’s cap on.

However, the knowledge generated by our instruments which assess the impact of the unknown has not only provided new explanatory frameworks for the anomalies that concerned Airbus. This knowledge also has organizational implications. It namely highlights actions / activities devoted to ‘not obviously visible unknowns with impact’ - these actions which were thus far invisible (since both the unknown and its impact were invisible). So our research offers new representations for collective action, and it calls for transformations at the level of collective action. This corresponds to the principle of inseparability between knowledge and relationships (Hatchuel, 2001) according to which modifications at the level of knowledge necessarily entails modifications at the level of the relationships (between the actors of a collective). This can be seen as an ‘intervention’ side of our research.

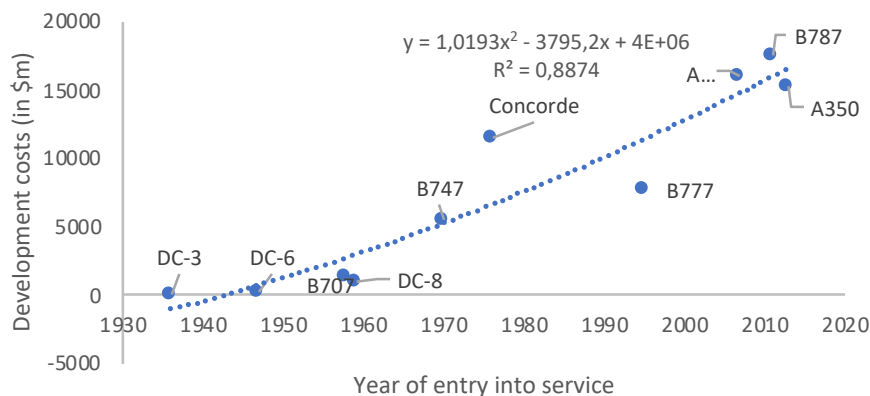
One can note that Section 4 which summarizes our three papers essentially focuses on the ‘knowledge’ generated by our research. We come back to the ‘organizational’ and relationships aspects in Section 6 (with section 6.1 dedicated to the nature of the knowledge produced during this research work and sections 6.2 to 6.4 dedicated to its organizational implications).

### 3.3. Method to address the sub-questions of (RQ1) in Paper 1

#### 3.3.1. An anomalous development cost evolution dynamics

Within a product emblematic of rule-based design, the engineering departments of the commercial aircraft industry should *a priori* have accumulated a solid capital of design knowledge, know-how, methods, design rules, etc., and should be able to use this capital with increasing efficiency, i.e. develop new product designs at increasingly lower development costs, within increasingly lower development times, thanks to learning effects.

But commercial aircraft development costs have by no means moved down a learning curve. They have steadily increased with time (see Figure 9a). And development times (from program launch to aircraft Entry Into Service) have also steadily increased (a second-degree polynomial increase, represented in Figure 9b below).



**Figure 9a. Commercial aircraft development cost escalation (in 2012 \$m) (Source: Nolte et al., 2012)**

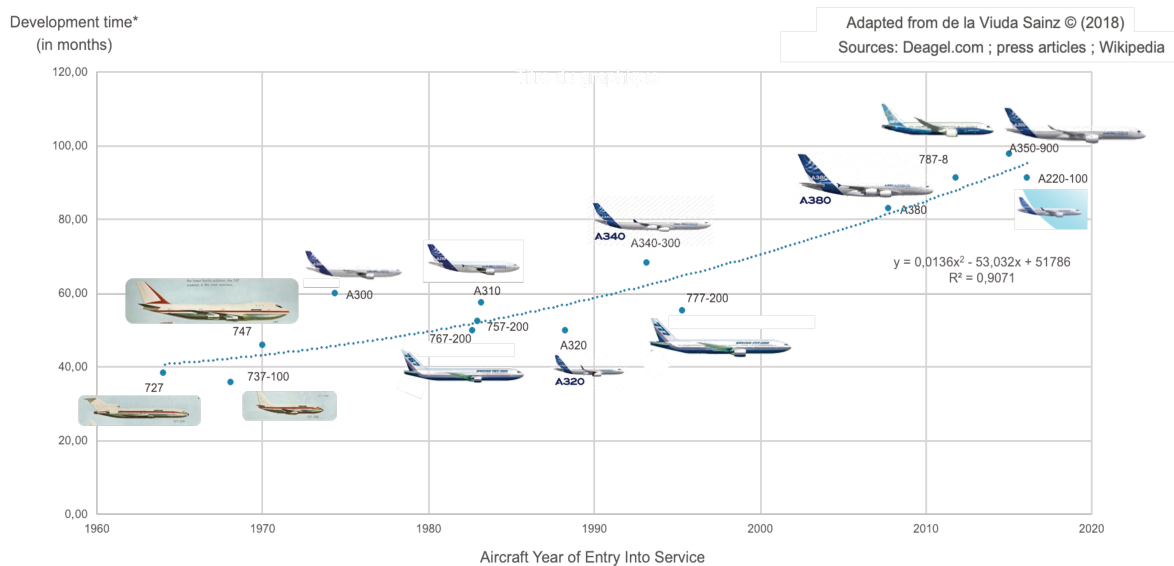


Figure 9b. Commercial aircraft development times: a polynomial increase

This cost escalation trend is a source of concern. If costs keep up increasing, the profitability of the future programs may be compromised. Indeed, the most recent products featured in Figure 8 are not only costlier and longer to develop than their predecessors: they are also characterized by major overcosts and delays with respect to their initial target. For example, the development costs of the A380 are estimated between \$20 and \$25 billion dollars<sup>25</sup>, which represents more than +90% with respect to the initially planned budget (10.5 billion dollars). In addition the program had experienced a 2-year delay. The development costs of the Boeing 787 are also twice greater than the initial target (40 bn dollars versus \$20 bn initially planned) and the program was 40 months behind schedule (Shenhar et al., 2016). Another example is that of the A220 (Bombardiers' CSeries): the program experienced +90% cost growth: \$6 bn incurred versus \$3.2 bn initially planned, and a 3-year delay. These cost deviation phenomena raise concerns because they make the attainment of investment break-even point more difficult. With the halt in production of the A380 and the final shutdown of the programme, this huge investment ticket will not have paid for itself. Some press articles also ask whether the Boeing 787 will reach profitability<sup>26</sup>. The CSeries program has sapped Bombardier's financial resources and even forced the company to cancel a business jet program, delay the development of another and sell a 30-percent stake of its train building divisions, etc<sup>27</sup>.

The intriguing, incomprehensible character of this cost escalation phenomenon leads to ask whether these increasing cost and time escalation trends would not be the manifestation of a hidden variable, which thus far has been ignored by traditional economic calculation – and more precisely, a variable that is not obviously visible in a rule-based design context: functional expansion, which, as mentioned in introduction, concerns commercial aircraft.

<sup>25</sup> Source: press article to add

<sup>26</sup>

<sup>27</sup>

Thus far, the tasks dedicated to functional expansion are not claimed by any corporate department. They may be managed by the traditional organizations (the organizations in charge of already-existing functions, the organizations in charge of product architecture and integration, etc.), in a silent, unclaimed way, without specific tools and methods, without a specific budget line, etc. that would be dedicated to functional expansion. The accountability systems or costing systems of large R&D firms do neither have an area of expenditure nor a specific budget dedicated to functional expansion. The cost of functional expansion is blended with that of other activities. To date, a manager confronted to a cost escalation curve experienced by a product affected by functional expansion is neither able to say whether or not, nor able to say with which intensity newly added functions play a role in the cost increase dynamics. And if functional expansion turns out to be a driving force in a cost escalation trend, a manager is not able to answer the question: what approaches (in terms of means of action, management, organization, method...) could be considered and prescribed to counteract such phenomena and their harmful economic impacts?

In the face of these elements, we will attempt to develop an instrument which measures the impact of functional expansion in cost escalation curves. We introduce the method we use to do so in the following subsection.

### 3.3.2. A theoretical simulation-based method to assess the cost impact of functional expansion

Our research approach to investigate the relationship between functional expansion and costs could have been empirical: collecting data about the incurred costs of past aircraft programs, and identifying in the cost structure the costs associated with newly added functions. Beyond the fact that such a study on real-world costs would have implied confidentiality issues, we have also noticed that distinguishing between functional-expansion-induced costs and upgrades-induced costs (especially distinguishing between functional-expansion-induced and upgrades-induced propagation costs) is not so obvious. In particular, given the large lack of knowledge regarding the order of magnitude of the cost impact of functional expansion, we do not know what kind of 'instruments' (especially in terms of precision) would be needed to empirically capture the costs of functional expansion.

So in the framework of this thesis, we have rather used a theoretical approach (that goes beyond the perimeter of the commercial aircraft industry) to address the question of the share of responsibility of functional expansion in a cost escalation curve. Instead of carrying out an empirical study limited to a specific product, we attempted to get a general understanding (non-specific to any product), with a simulation method. By combining and adapting models that exist in the literature, we built a simulation instrument that enables to generate cost escalation curves, and we assessed how functional expansion impacts the shape of a cost escalation curve (of a family of cost escalation curves more specifically). This instrument allowed us to capture the order of magnitude of the impact of functional expansion on a specific sub-type of development costs, that is propagation costs.

After this quantitative characterization of functional expansion in terms of cost impact, we have been interested in managerial approaches to control the studied phenomenon, i.e. to manage the impact of functional expansion. Again, we have used a theoretical, simulation-

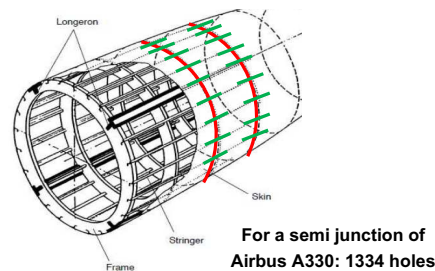
based approach: on the one hand, simulation of modularization strategies, and on the other hand, anticipation-oriented overinvestment strategies. This allows us to identify the extent to which functional expansion-induced unknown questions the economic management techniques conventionally used in rule-based design.

### 3.4. Methods to address the sub-questions of (RQ2) in Papers 2 and 3

#### 3.4.1. Two puzzling economic calculation practices in Airbus – promising candidates for offering empirical evidence for the existence of a design theory-based investment decision model

#### **First puzzling economic calculation practice: the Flextrack robot (Airbus Saint Nazaire)**

The Flextrack robot is an automated drilling system, patented by Boeing, which may be used to drill the orbital and / or longitudinal joints of an aircraft fuselage<sup>28</sup>.

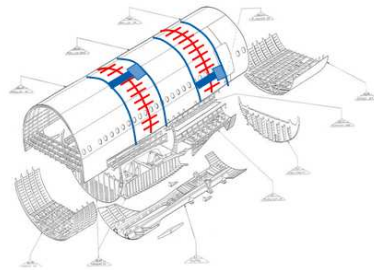


**Figure 10a. In red and green, the orbital drills on an aircraft fuselage**

In a context where the traditional technique to drill holes (manual drilling, which consists in putting grids (templates, like stencils) against the fuselage and drilling the holes with a hand drill) is well-mastered, but time-intensive, the perspective to automate drilling activities is investigated by aircraft manufacturers. In particular, Boeing has developed, patented and implemented the Flextrack drilling system. The principle consists in putting rails across (resp. along) the fuselage for an orbital drill (resp. longitudinal drill). And the Flextrack (the robot) is a platform which moves along the rails and drills the holes. Boeing has implemented the Flextrack on several of its product lines (since 2011 on 787 ; since 2013 on the 777 ; and also implemented on 767, 747-8...). And three suppliers own the license.

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<sup>28</sup> The structure of an aircraft fuselage is made of a skeleton (frames and stringers) which is recovered by a skin. The whole formed by the skeleton and the skin is assembled thanks to orbital and longitudinal rows of rivets. Installing these rivets requires making rows of orbital and longitudinal holes

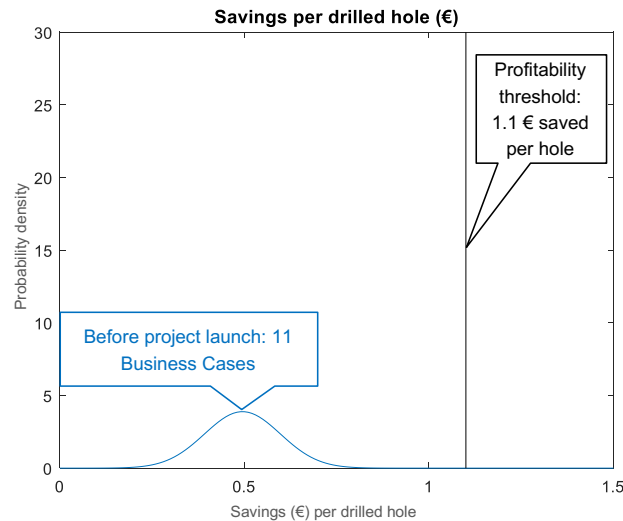


**Figure 10b. The Flextrack rails and the robot (in blue) drilling an orbital drill in red**

Between 2009 and 2013, the plant of Airbus Saint Nazaire (France) started its first activities related to the Flextrack (and to automated drilling activities), with an R&T project which aimed at developing first bricks of knowledge regarding the technology. And in 2013, the opportunity of introducing one or two robots in the production environment (on one station of the A330 line) was studied. Eleven different implementation scenarii were considered: they differed depending on whether one or two robots were to be deployed, depending on whether the robot to be introduced was a retrofit (i.e. an adaptation to the production environment) of the prototype which the R&T had used for its laboratory experimentations or a new robot purchased from a supplier, depending on whether purchasing an extra module (a sensor allowing the robot to better detect the location of the hole to be drilled) was part of the scenario or not.

In order to assess the economic interest of these scenario, the traditional economic calculation process of the plant was followed: a business case (involving a profitability estimation with an NPV calculation) was associated with each scenario. The perspective of ergonomic gains for the operators, the fact that Boeing had already deployed the robot or the fact that it could be used on other production lines was not taken into account in the calculation. In each of the studied scenario, the profitability criterion required by the plant to allow the launch an investment project (that is a less-than-two-year payback time) was not met.

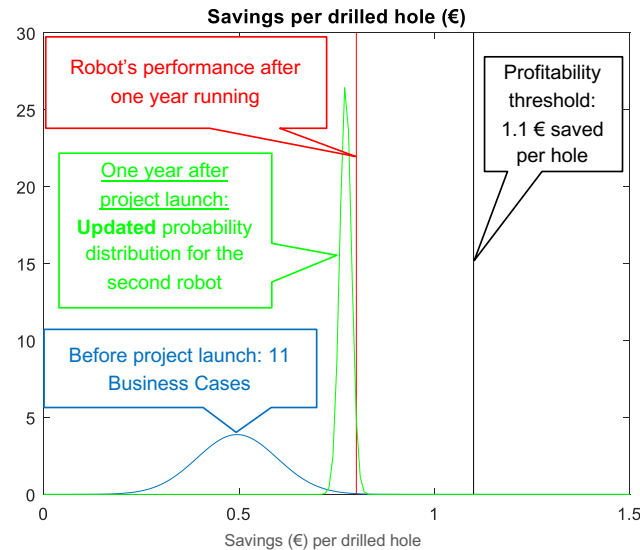
In order to represent the extent to which the Flextrack project was predicted to be unprofitable, we have computed the mean and the standard deviation of the 11 profitability estimations. This allows to plot the gaussian probability distribution featured in blue in Figure 11 below, which is centered around 0.5 € saved per drilled hole, and which is rather highly uncertain. This probability distribution is far from 1.1 euro saved per drilled hole, the threshold which should have been attained to make the project profitable. In the blue distribution, 1.1 euro saved per drilled hole is a highly uncertain value. In sum, in this investment decision situation, everything advices against launching the project, which is expected to be all but profitable.



**Figure 11. The Flextrack investment project – a seemingly all but profitable investment**

But in spite of this seemingly unfavorable investment situation, it was decided to invest in a trial involving a prototype (the robot used by the R&T for the laboratory experiments) before considering the purchase of a new robot.

One year after the beginning of the deployment of the prototype in the production environment, its performance was assessed: it was about 0.8€ saved per drilled hole (represented by the red line in the Figure below). And based on this information, ten new profitability estimations were computed in order to assess the economic interest of purchasing a new robot (with the same economic evaluation process as in the first step). Again, we computed the mean and the standard deviation of the savings per drilled hole involved in these 10 profitability estimations. This enables us to plot the green probability distribution in the Figure below. Although the profitability limit (in black) is not yet reached with the green curve, the observed profitability dynamics that had been set in motion was considered as sufficiently promising to suggest interesting spillovers (beyond the A330 production line) and to suggest the possibility of going beyond this threshold later on. In addition, in the meantime, the plant had upgraded its priorities in terms of automation and the profitability constraint for this type of project had been revised downwards. So the economic results associated with the green curve were considered as sufficiently conclusive to validate the decision to purchase a new robot.



**Figure 12. Dramatic performance shift following the deployment of the prototype**

The shift from the blue to the green probability distribution is striking: we are in the face of the same investment project, on the same production line (A330), on the same work station. And the blue and the green curve represent two very different investment projects. How can the shift from the blue to the green curve be explained?

In sum, two elements are puzzling in this case: the investment decision, and the huge returns. The fact that an investment was conceded, whereas economic calculation advised against investing can be seen as a kind of ‘deviant’ economic calculation practice. The project did not respect the traditional investment rule. We can add that although the robot was innovative, the project did not seem to involve much innovative design and unknown at first sight: indeed, the introduction of a new machine is very common in a plant.

This leads to ask the questions:

- are we in the face of a deviant, irrational (and worrying) economic calculation practice which, this time, ended well (thanks to luck), but such that nothing guarantees that it will not lead to a failure if it is reiterated?
- or are we in the face of an outstandingly performing uncertainty-reducing-learning-by-doing mechanism?
- or, contrary to appearances, would there be in this case unknown the impact of which would have generated the move from the blue to the green curve? Would the decision to invest and the generation of the huge returns be the result of an original economic reasoning in the unknown?

A theoretical interest underlies this question: could the Flextrack case provide empirical evidence for the theoretical, design-theory-based model introduced at the end of Section 2.2.3.?

At the same time, a manager of the A330 production line was interested in rigorously highlighting the mechanisms which had allowed such returns.



## **Second puzzling economic calculation practice: an Airbus innovation cluster (AIC) which manages a portfolio of incubation and acceleration projects**

In January 2019, we presented our analysis of the performance of the Flextrack case at the partners' day of the chair Design Theory and Methods for Innovation. This day was attended by the innovation leader of an innovation cluster in Airbus , who manages a portfolio of incubation and acceleration projects. He brought our attention to this very portfolio, the performance outcomes of which had some similarities with the Flextrack case. He was seeking to rigorously explain the mechanisms underlying the generation of the portfolio performance, and he asked us whether we were interested in carrying out a study on this portfolio and its economic performance.

The project portfolio in question selects difficult, technology-based problem-driven topics that traditional organizations perceive as so risky that choose not to launch them. These projects are highly uncertain. And economic calculation either renders negative results, or is simply impossible (a so high level of uncertainty that economic data cannot be quantified). The innovation cluster is very selective about the projects which join the portfolio. In 2017, 36 out of the 1500 projects which applied were selected. And in 2018, the portfolio demonstrated a remarkable profitability (x6 rate of return). In addition, among 55 projects incubated or accelerated within this portfolio between 2013 and 2018, 41 (i.e. 75%) gave rise to further investments within the very same organizations which had chosen not to launch them.

Contrary to the case of the Flextrack robot where the presence of innovative design is not obvious, innovation is at the core of the mission of the innovation cluster. But the decision to invest in these highly uncertain, seemingly unprofitable projects remains daring. And the x6 rate of return is huge and surprising for a portfolio made of projects that should not have been launched<sup>29</sup>. Indeed, according to traditional decision theory-based model, the more uncertain a project is, the lower the probability of making it profitable, so the lower its expected profitability. Put differently, the probability that an uncertainty reduction mechanism generates 'considerable' value is generally extremely low, according to decision theory.

This leads to raise the same kind of questions as in the case of the Flextrack robot: do the economic results of the portfolio result from luck? Do they result from uncertainty reduction (in this case, it should be an outstandingly performing uncertainty reduction mechanism)? Or could unknowns with impact explain these remarkable economic results?

A theoretical interest underlies this question: could this portfolio provide empirical evidence for the theoretical, design-theory-based model introduced at the end of Section 2.2.3.?

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<sup>29</sup> In Airbus, the economic performance of the portfolio, especially the x6 rate of return, also aroused surprise (sometimes doubt). So for the manager of this portfolio, our research was the opportunity to complement his 2018 profitability analysis with an 'independent' study, that would describe the mechanisms that led to generate such profits (and confirm the solidity of the economic results)

### 3.4.2. Methods used to test whether these two puzzling economic calculation practices confirm the design-theory-based model introduced at the end of Section 2.2.3.

We studied the Flextrack project first, the innovation project portfolio second. But the research questions and the broad lines of the methodological approaches are similar.

On the one hand, in both cases, we used a statistical method to determine whether the outcomes obtained in these two cases result from luck, uncertainty reduction or the unknown. In particular, we utilized Bayesian statistics to test whether the economic outcomes resulted from an outstandingly performing uncertainty-reduction-only mechanism.

On the other hand, we have carried out case studies in order to double-check the results of the statistical test and get a better understanding of the managerial practices underlying these cases. One can note that these cases are particularly appropriate for single case study (Siggelkow, 2007) research because they are anomalous, seemingly deviant phenomena within the decision-theoretic framework. In the first instance, they do not fall within an existing theoretical framework. They are unexplained. So investigating these cases would potentially provide us new with insights regarding investment and profitability within an unknown-including, design-theoretic framework. The tables below summarizes the main source of data collection in the framework of these statistical and case study methods.

Internal documents	Interviews	Observations
<p><b>Documents internal to Flextrack project:</b></p> <ul style="list-style-type: none"> <li>• Economic evaluation documents (initial business cases and associated NPV calculations, and those completed after one year)</li> <li>• The minutes and presentations associated with some preparatory meetings</li> <li>• The minutes of the kick-off meeting of the first launch of Flextrack (retrofitted prototype)</li> <li>• The minutes of the milestone review meeting – including the last milestone which provides the figures related to the performance of the robot</li> <li>• Etc.</li> </ul> <p>→ These documents provided us the figures that allowed us to describe the evolution of the performance of the Flextrack robot</p>	<p><b>Between November 2017 and June 2018:</b></p> <ul style="list-style-type: none"> <li>• 3 one-hour interviews with the manager of head of the A330 projects (+ additional informal discussions)</li> <li>• 1 one-hour interview with two members of the initial R&amp;T project, who participated in the deployment of the robot on the A330 line</li> <li>• 1 one-hour interview with the project leader of the subsequent project allowed by Flextrack (i.e. deployment of 20 robots on the A320 production line)</li> </ul> <p><b>(October 2019:</b></p> <ul style="list-style-type: none"> <li>• 1 one-hour meeting with two members of the initial R&amp;T project, who participated in the deployment of the robot on the A330 line (the same as in 2018) in order to show them the results)</li> </ul>	<p><b>December 2017:</b></p> <ul style="list-style-type: none"> <li>• Participation in the organization of a workshop the objective of which was to ‘explain’ and ‘explicit’ the mechanisms which had allowed the Flextrack project to be profitable</li> <li>• Informal discussions with workshop participants</li> </ul>

Table 1. Summary of the data sources in the case of the Flextrack project

Internal documents	Interviews	Observations
<p><b>Documents internal to the AIC:</b></p> <ul style="list-style-type: none"> <li>The excel file which records the progress of each individual project (in terms of economic value, maturity, prototyping progress, etc.) and which is continuously kept up to date by the AIC innovation leader</li> <li>The AIC evaluation of the year 2018 (critical summary of its way of working, its achievements, its economic performance, its perspectives for 2019)</li> <li>The Mission Letters for each individual project joining the portfolio</li> <li>Projects final presentations summarizing the outcomes of the incubation / acceleration process and presented to the members of the selection board</li> </ul> <p><b>AIC's documents for communication within Airbus and outside Airbus</b></p> <ul style="list-style-type: none"> <li>Brochures, communication documents (toward Airbus employees) which present the AIC and the portfolio, the incubation / acceleration, its resources, its offers, its performance</li> <li>Minutes of roundtables presenting the AIC in external events</li> </ul>	<p><b>Between mid-February and mid-June 2019:</b></p> <ul style="list-style-type: none"> <li>4 one-hour interviews with the AIC Innovation Leader (+ additional informal discussions)</li> <li>3 one-hour interviews with incubation / acceleration project owners (responsible for 3 different projects)</li> <li>2 one-hour interviews with the coaches responsible for the methodological support offered to the project (+ additional informal discussions)</li> </ul>	<p><b>Between mid-February and October 2019:</b></p> <ul style="list-style-type: none"> <li>Informal discussions with people (often the interviewees) involved in the AIC</li> </ul> <p><b>October 2019</b></p> <ul style="list-style-type: none"> <li>Attendance of an 'Innovation Event' organized by the AIC.</li> <li>Co-facilitation of a workshop dedicated to 'the creation of value with prototyping'</li> <li>Informal discussion with project members who presented the outcomes of their incubation / acceleration project in a marketplace format</li> </ul> <p><b>December 2019</b></p> <ul style="list-style-type: none"> <li>Attendance of a AIC monthly meeting where candidates pitch their ideas and where technology watch information are shared</li> </ul>

**Table 2. Summary of the data sources in the case of Airbus innovation cluster's (AIC) portfolio of incubation and acceleration projects**

#### 4. Summary of the papers

##### 4.1. Summary of Paper 1: Cost-effectiveness management under functional expansion: simulation of product development cost escalation curves and modularization strategies

Status: first version submitted to Research in Engineering Design in March 2020, revision submitted in November 2020

Previous version: Agathe Gilain, Pascal Le Masson, Benoit Weil. The hidden feat behind development cost escalation - how engineering design enables functional expansion in the aerospace industry. ICED, Aug 2019, Delft, Netherlands. (hal-02095385)

This paper is theoretical, simulation-based. It studies how functional expansion (El Qaoumi, 2016 ; Le Masson et al., 2018) 'contributes' to accelerate cost escalation curves, in an attempt to determine whether functional expansion is an 'unknown with impact'.

The literature dedicated to cost escalation identifies several factors responsible for cost increases, among which changes at product level, requirement volatility involving programs that deviate from their initial target, management errors, poor program execution, subcontracting issues, unrealistic cost estimates, and flawed initial concept caused by over-optimism and non-progressive application of new technologies in the framework of a continuous race for superior technologies, leading to development problems, etc. According to statistical studies (Arena et al., 2008 ; Bongers and Torres, 2013 ; Hove and Lillekvelland, 2014), changes at product level are a significant cost drivers. But these studies only consider one kind of product changes: upgrades of existing product functions. The addition of new functions is reported to be costly, based on qualitative investigations (interviews). But it is not assessed quantitatively.

In addition, the literature in engineering design also highlights that changes at product level (engineering changes) are costly, in particular if they propagate, that is require changes in other entities within the architecture (e.g., rework, redesign, iterations) (Clarkson et al. 2004). However, this literature does not explicitly distinguish between the cost impact of propagations due to the upgrade of an existing product function, and the cost impact of propagations due to the enrichment of the architecture with a new function.

Therefore, the objective of this first paper is to get a better understanding of the cost impact of new functions on the cost evolution dynamics associated with a product evolution trajectory. In particular, we aim at getting more insights regarding the family of cost curves which are theoretically attainable in the framework of a product evolution dynamics that includes functional expansion, i.e. a continuous emergence of new functions with time. The paper especially aims at determining the theoretically best attainable cost escalation curve (the lower curve) and the theoretically worst attainable cost curves (the highest one), in order to have a better understanding of the ‘cost space’ within which design engineers play, i.e. the cost space within which engineering departments ‘draw a curve’. And we aim at visualizing and discussing what their possibilities are in terms of navigating within the cost space (especially with a cost reduction-oriented perspective).

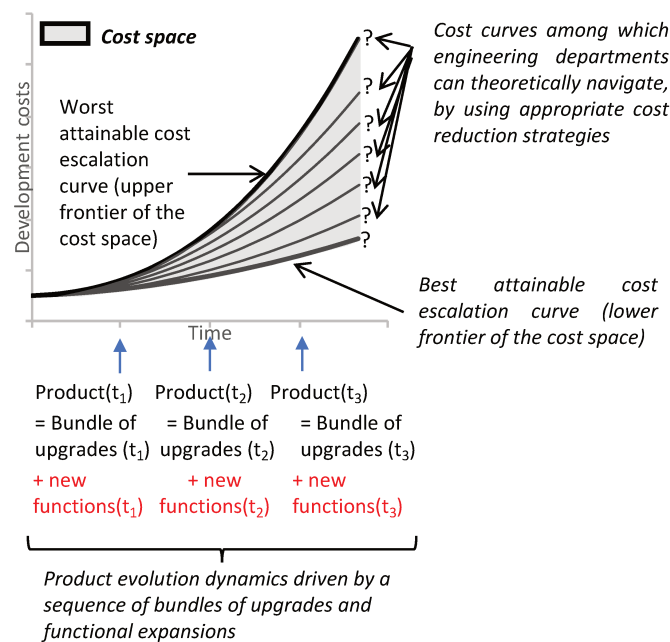


Figure 13. Cost space within which design engineers generate a development cost evolution curve in the framework of upgrade-and-functional-expansion-driven product evolution dynamics

To that end, the paper addresses the three following research questions:

(RQ1-S1): What is the shape of ‘pure’-functional-expansion-driven development cost escalation curves? To what extent does this shape suggest that functional expansion can potentially have a large magnitude impact on development costs?

*This sub-question aims at isolating functional expansion and at determining how dimension of functional expansion (the new functions themselves or the propagation phenomena generated by the new functions) contributes to cost escalation.*

(RQ1-S2): To what extent can well-proven modularity strategies slow down the cost escalation curves under functional expansion?

(RQ1-S3): With respect to functional upgrades that have already been demonstrated as a significant cost driver, under which conditions may functional expansion also be a significant cost driver (conditions under which functional expansion would be a priority in terms of cost management, ahead of controlling the costs of already-existing functions)?

We addressed each of these three questions with a simulation. In each simulation, we mobilized a cost propagation model<sup>30</sup> (Rebentisch et al., 2017). Cost propagation models are particularly appropriate to simulate families of cost escalation curves. Indeed, as mentioned in subsection 2.1.2., they are based on  $k$ -order combined propagation likelihood matrices  $CL^{(k)}$ . In these matrices,  $cl_{ij}^{(k)}$  is the probability that the implementation of an intentional engineering change in the function  $F_j$  propagates to  $F_i$  through a propagation path the length of which is equal to or less than  $k$ , and requires addition design work and costs for  $F_i$ .

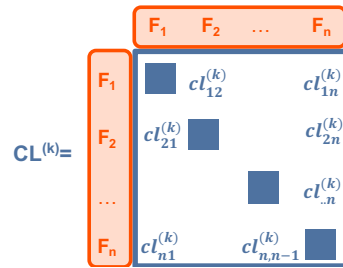


Figure 14. A  $k$ -order combined propagation likelihood matrix (Clarkson et al., 2004), used in the Rebentisch et al. (2017) cost propagation model

' $k$ ' denotes to the length of a propagation chain, i.e. to the capability of an engineering department to prevent the occurrence of longer-than- $k$  propagation chains. The integer  $k$  can be seen as an indicator of an engineering department's capital of architectural knowledge (design rules, capability of the teams in charge of the different functions to coordinate with one another,...).

If we define a theoretical product evolution dynamics, driven by both bundles of upgrades and the emergence of new functions at each time step, we can generate a cost escalation curve associated with one integer ' $k$ '. In order words, the integer ' $k$ ' involved in cost propagation models enables to discretize the cost space within which engineering departments 'play in the framework of a product evolution trajectory.

<sup>30</sup> Subsection 2.1.2 gives details about cost propagation models

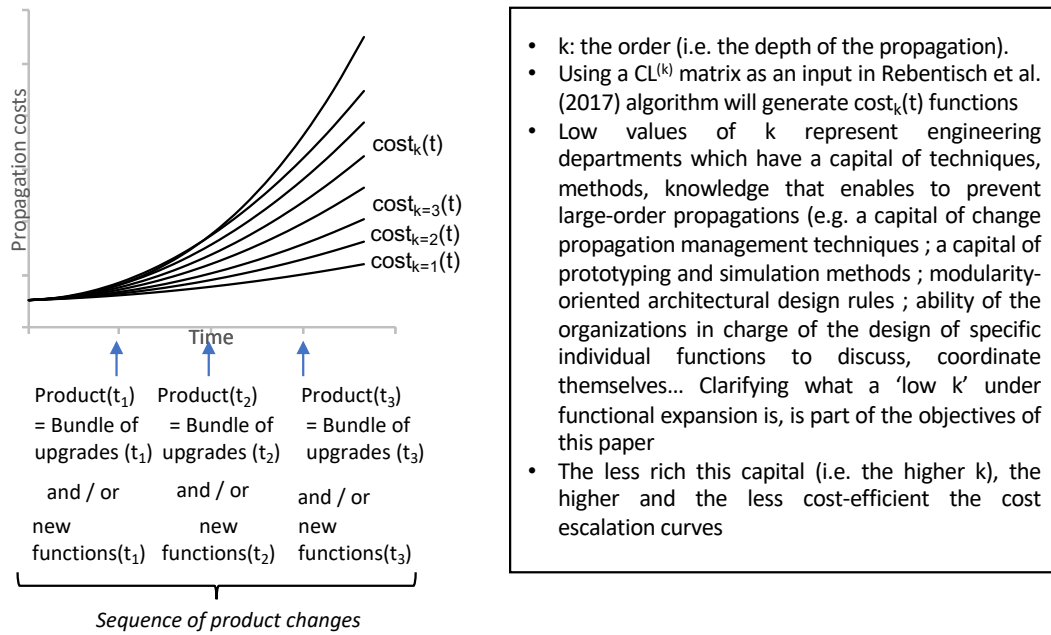


Figure 15. A family of cost curves where each cost curve corresponds to a certain capacity of the engineering department to prevent longer-than- $k$  propagation paths

Based on cost propagation models, we ran three simulations.

### Simulation 1: getting a better understanding of the shape of pure-functional-expansion-driven cost escalation curves

Simulation 1 aims at addressing RQ1-S1, i.e. at generating pure-functional-expansion-driven cost escalation curves, in order to get a better understanding of their shape. The simulation does not aim at modelling the cost behavior of a specific product, but at assessing the order of magnitude of the cost impact of functional expansion. However, to set the simulation parameters, we were inspired by the fact that in the case of aircraft, the number of regulatory paragraphs has tripled between 1965 and 2018 (details on the data in Figure 3, in Introduction). So we simulated a functional-expansion-driven product evolution dynamics such that the size of the functional space triples. In addition, computing<sup>31</sup>  $CL^{(k)}$  matrices when the functional size is large (i.e. the size of the DSM is large), is very time intensive. So this restricted the possibilities in terms of size of the functional space. We started at time  $t = 0$  with a theoretical product made of 8 functions,  $F_1, \dots, F_8$ . And we ran the simulation over 16 iterations, adding one function  $F_{8+t}$  at each time step (i.e. a 'modest' functional expansion mechanism), ending with a size-24 functional space at  $t = 16$ .

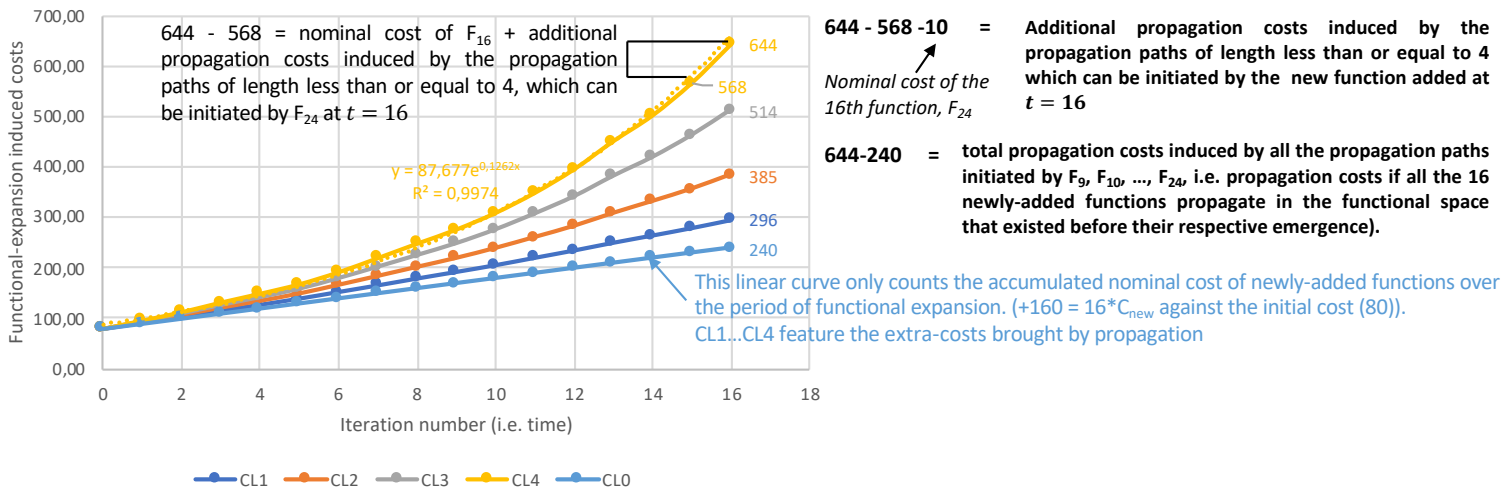
In this simulation, at each time  $t$ ,

- we counted the nominal cost  $C_{new} = 10$  of the newly-added function  $F_{8+t}$
- we counted the propagation costs induced by the addition of  $F_{8+t}$ . If a function of the already-existing-functional space ( $F_1, F_2, \dots, F_{8+t-1}$ ) is affected by a propagated change originating from  $F_{8+t}$  through a propagation path the length of which is less than or equal to  $k$ , a redesign which costs  $x\%$  of its initial cost is required. In the cost escalation curves plotted in Figure 16

<sup>31</sup> With Matlab

below, we simulated redesigns which cost 20% of a function initial costs, and we made  $k$  vary between 1 and 4, which led to generate four cost escalation curves.

Figure 16 below (explained in more details in the paper), shows that the costs induced by newly added functions may be substantial if propagation costs are not well-controlled, that is if the propagation depth of an intentional change can be as large as 4. One can note that the curves CL1, CL2, CL3 and CL4 only represent a lower bound of the possible cost impact of functional expansion. CL5, CL6... curves would be even higher<sup>32</sup>.



**Figure 16. Cost escalation curves in the frame of a pure-functional-expansion-driven product evolution dynamics**, such that one function is added at each iteration and propagates changes in the already existing functional space. A ‘CLK’ (with  $k > 0$ ) curve is a cost escalation curve computed with the combined likelihood propagation matrix  $CL^{(k)}$  – only length-less-or-equal-to- $k$  propagation paths may occur.

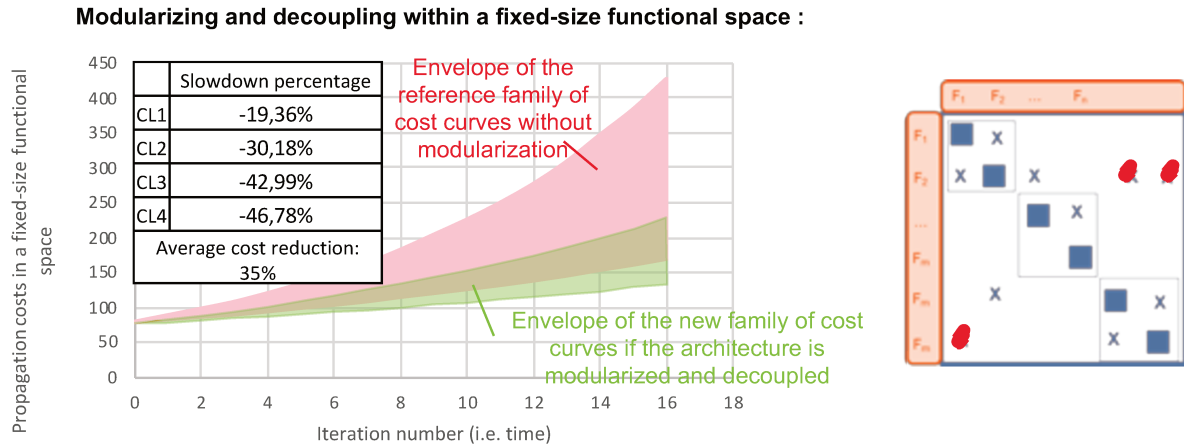
In sum, the unknown introduced by functional expansion in a product architecture may involve dramatic shocks, which themselves may induce huge propagation costs (indeed, the linear curve CL0 illustrates the nominal costs of the newly-added functions: the costs ‘above’ this curve are due to propagation). Controlling the propagation costs induced by functional expansion seems crucial, if one wants to avoid dramatic cost escalation trends.

So in Simulation 2, we investigated whether modularization, a well-proven method to prevent undesirable propagation chains and reduce costs, can slow down cost escalation under functional expansion.

**Simulation 2: is the cost-reduction power of modularization preserved under functional expansion?**

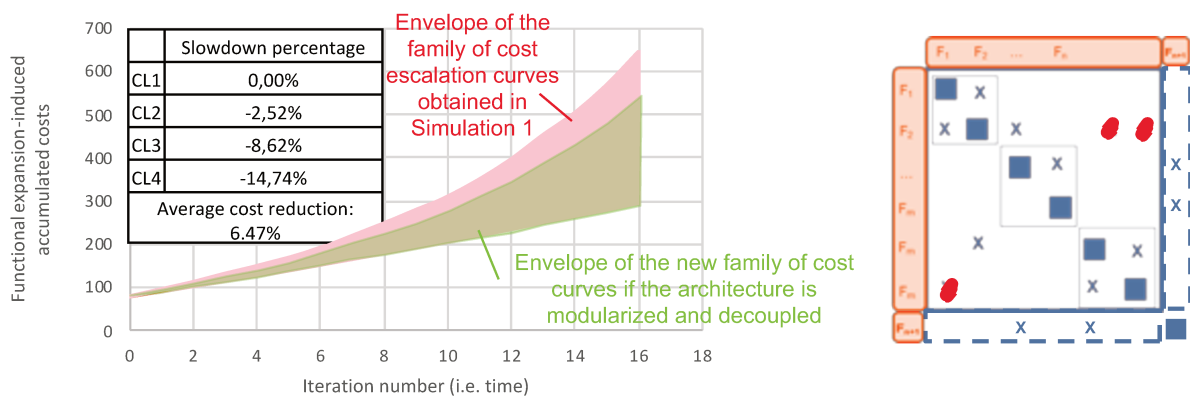
<sup>32</sup> We have not computed CL5, CL6... curves, because computing  $CL(k)$  matrices for large values of  $k$  is very long – several hours

Firstly, in Simulation 2, we ran a control test in order to ensure that our simulation of modularization strategy indeed slows down cost escalation in a fixed-size functional space, i.e. in a classic context. This test was conclusive. Figure 17 below illustrates the cost-reduction power of our modularization strategy.



**Figure 17. Modularization strategy: a control test. The cost reduction power of modularization in a fixed-size functional space**

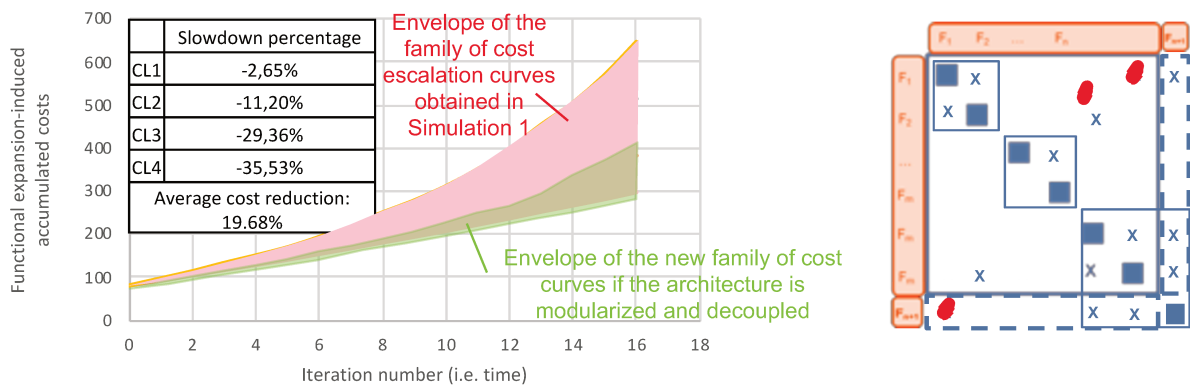
Secondly, we tested a first modularization strategy in a context of functional expansion: starting with an initially modularized architecture, and generating a pure-functional-expansion-driven product evolution dynamics. In other words, we tested whether starting with an initially modular architecture could slow down the family of cost escalation obtained in Simulation 1. The results showed that such a modularization strategy slows cost escalation down only very slightly. This is not very surprising, since the newly added functions tend to reintroduce dependencies between functions which had been initially decoupled at  $t = 0$ . In other words, functional expansion undermines the cost-reducing power of modularization.



**Figure 18. First modularization strategy simulated under functional expansion: an initial modularization of the architecture**

Then, we tested a second modularization strategy in a context of functional expansion, such that after having added one new function, we re-cluster and decouple the entire architecture before computing the costs. Figure 19 illustrates the results





**Figure 19. Trying to slow down the family of cost escalation curves obtained in simulation 1 with a ‘continuous modularization strategy’**

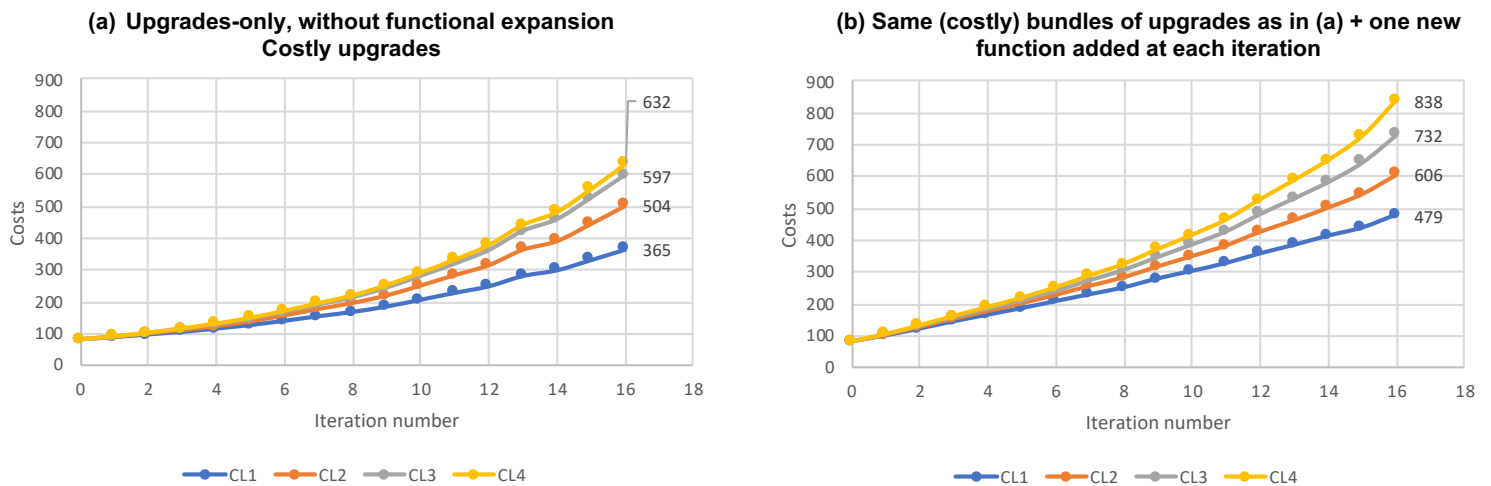
We observed that this architectural approach enables to restore the cost-reducing power of modularization, under functional expansion. This suggests that to manage the shocks that newly added functions trigger in the already-existing architecture (which are reflected in the propagation costs), design engineers need to continuously renew architectural independencies under functional expansion.

### **Simulation 3: to what extent is functional expansion a significant cost driver with respect to functional upgrades?**

In Simulations 1 and 2, we ‘isolated’ the cost effects of functional expansion to order to better understand it, that is the cost effects of all other cost factors were neutralized. In particular, the cost effects of functional upgrades, which are proven cost factors (e.g. Arena et al., 2008) were ‘neutralized’. This leads to raise the question: in a functional-expansion-and-functional-upgrade-driven cost escalation curve, what are the respective weight of functional expansion and functional upgrades? (knowing that functional upgraded are a proven cost escalation factor).

Let us consider a modest functional expansion phenomenon where one function is added at each iteration. In the first instance, adding one function at each iteration may seem anecdotal in terms of design efforts in comparison with managing the upgrade of several already-existing functions. Does Simulation 3 confirm this intuition?

In Simulation 3, we found that if the upgrade of already existing functions is costly (e.g., at each iteration, upgrading an already existing function requires a +30% increase of the nominal cost of this very function), the role of functional expansion in the cost escalation curve is limited. (Figure 20)

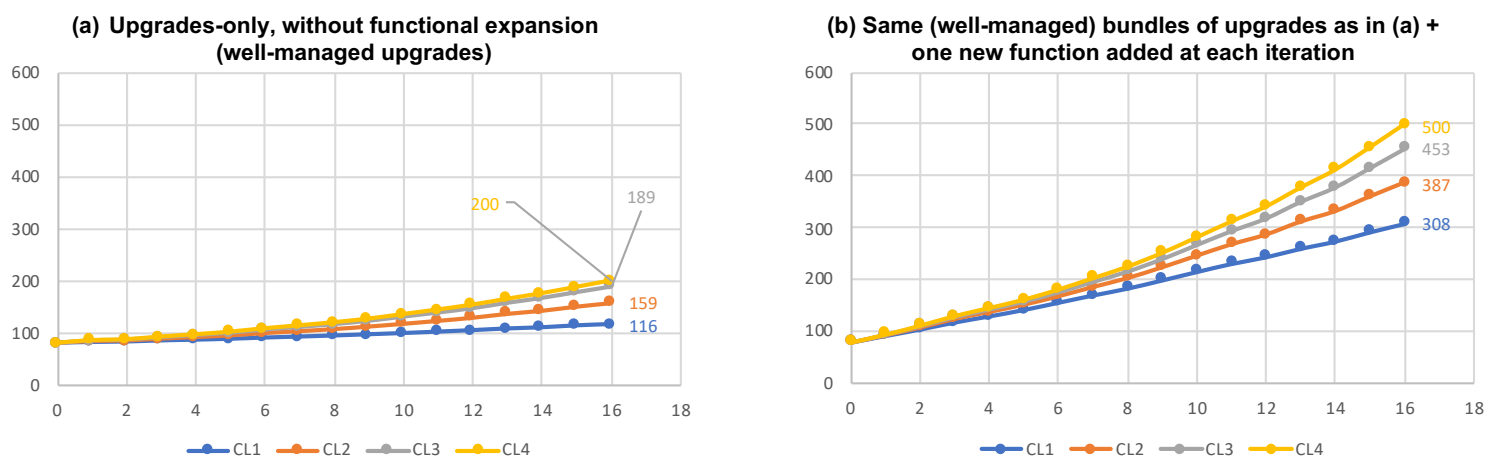


**Figure 20. Simulation of a product evolution dynamics where the upgrade of already existing functions is ill-controlled by engineering departments.**

Left: family of cost escalation curves associated with an ill-controlled, pure-upgrades-driven product evolution dynamics. Right: the same upgrades as on the left-figure have been simulated, but in addition, at each iteration, one new function was added.

Figure 20 shows that introducing new functions in a product evolution dynamics where the costs of upgrades are ill-managed has a visible, but non-spectacular impact of functional expansion on an ill-controlled upgrade-driven cost escalation curve.

But conversely, if the engineering department is capable of upgrading already-existing functions without incurring additional costs (i.e. although they are upgraded, the development costs of the already-existing function remain stable), adding one function at each iteration may have a significant impact in the cost escalation curve.



**Figure 21. Simulation of a product evolution dynamics where the upgrade of already existing functions is well-controlled by engineering departments.**

Left: family of cost escalation curves associated with an well-controlled, pure-upgrades-driven product evolution dynamics. Right: the same upgrades as on the left-figure have been simulated, but in addition, at each iteration, one new function was added.

The cost escalation curves plotted in Figure 21b may be misleading for a manager who is unaware of functional expansion and aims at reducing costs:

- this manager may be tempted to deploy a modularization strategy (e.g. invest in the deployment of a platform) – but such a strategy is likely to be disappointing, since functional expansion rapidly eliminated architectural independencies
- this manager may be tempted to pressure engineering departments to cut the costs of the individual functions. But we are in a scenario where engineering departments already master the costs of functional upgrades. So the cost reduction potential of such a strategy is limited... And as we will discuss later on, we can ask whether such decisions under functional expansion would not tend generate to crises in engineering departments, when engineers departments realize that they will not be able to reach the functional targets within the allocated budgets...

To conclude this summary of our first paper, we can highlight the following contributions.

Firstly, the results show that functional expansion requires a double-deliverable from engineering departments: (1) ensuring that the functional requirements of the individual product functions will be effectively met (classical rule-based design tasks) ; (2) continuously decoupling the expanding functional space, i.e. continuously designing independencies in the unknown (innovative-design activities, which are generally not expected in rule-based design).

Secondly, these results contribute to the literature on platforms, which has already emphasized the importance of managing and anticipating platforms obsolescence in order to avoid a violent loss of leadership (Meyer and Lehnerd, 1997, etc). This paper highlights that functional expansion may be a major obsolescence factor from the viewpoint of a platform architecture. This suggests that regarding watching for signs of platform obsolescence, design engineers should be very attentive to obsolescence that may originate from functional expansion. If an engineering department denies or ignores functional expansion and chooses traditional cost reduction strategies under functional expansion (e.g. modularization, reduction of the nominal costs of the individual functions, etc.), the results are likely to be disappointing or even worse.

Finally, our results contribute to interfaces challenges (Lakemond et al., 2013) in product development projects. Indeed, they highlight that a very specific source of interface challenge (Lakemond et al., 2013), that is functional expansion-driven product complexity (which can be distinguished from upgrades-driven complexity) requires particular early attention (anticipation?) from design engineers, project managers, etc.: if ignored, denied, ill-managed, it may alone trigger considerable costs (those which our simulation has assessed). In addition, as sources of interface challenge may combine with each other and reinforce each other (Lakemond et al., 2013), functional expansion-driven product complexity may combine with production complexity, organizational separations..., and generate even high-magnitude (economic) consequences.

All these elements clearly show that functional expansion-induced unknowns are unknowns with impact, which emphasizes the importance of organizing the management of rule-based design under functional expansion.

**Table 3. Summary of the first paper**

<b>Paper</b>	<b>Cost-effectiveness management under functional expansion: simulation of product development cost escalation curves and modularization strategies</b>
Research questions	<p>(RQ1-S1): What is the shape of functional-expansion-driven development cost escalation curves? Can functional expansion potentially have a large magnitude impact on development costs?</p> <p>(RQ1-S2): To what extent can well-proven modularity strategies slow down the cost escalation curves under functional expansion?</p> <p>(RQ1-S3): With respect to functional upgrades that have already been demonstrated as a significant cost driver, under which conditions may functional expansion also be a significant cost driver (conditions under which functional expansion would be an important object of cost management )?</p>
Literature background	<p>- The literature dedicated to cost escalation in the defense sector identified changes at product level as major cost inductors: a strong correlation between the upgrade of existing product functions and cost escalation ; and according to qualitative investigations (interviews), newly-added functions are also costly</p> <p>- Change propagation phenomena are major cost inductors</p>
Literature gap	Thus far, the cost impact of newly added functions has never been assessed. By implication, one does not know the how functional expansion influences a cost escalation curve
Research objective	Determining the magnitude of the impact of functional expansion on cost escalation. And if this impact is substantial, investigating possible cost-reduction strategies under functional expansion. In particular, investigating whether functional expansion preserves the cost-reducing power of a classic, well-known cost reduction strategy, that is modularization
Theoretical framework	Functional expansion ; cost escalation ; change propagation ; modularization
Material	Theoretical material: bricks of models that exist in the literature (cost propagation models)
Method	Simulation of families of theoretically attainable cost curves
Results	<p>(RQ1-S1): Functional expansion generates genuine ‘shocks’ in the product architecture. If these latter are out of control, their impact on costs is dramatic (exponential explosion). Thus functional-expansion-induced propagation costs might be huge.</p> <p>(RQ1-S2): Newly added functions may eliminate critical architectural independencies and render the modularized architecture obsolete, thereby undermining the cost-reduction power of modularization</p> <p>(RQ1-S3): There exists conditions under which functional-expansion-induced costs may dominate upgrade-induced cost</p>
Academic implications	<p>1/ Our results highlight that functional expansion requires a double-deliverable from engineering departments:</p> <p>- ensuring that the functional requirements of the individual product functions will be effectively met (classical rule-based design tasks)</p>

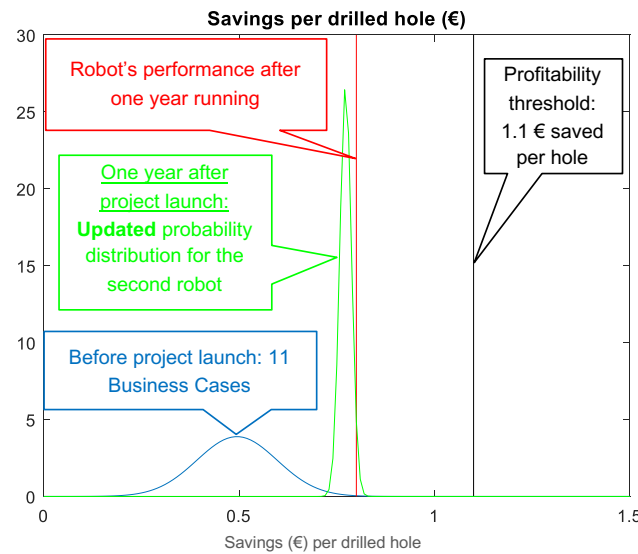
	<p>- continuously decoupling the expanding functional space, i.e. continuously designing independencies in the unknown (innovative-design activities, which are generally not expected in rule-based design).</p> <p>2/ These results contribute to the literature on platforms, which has already emphasized the importance of managing and anticipating platforms obsolescence in order to avoid a violent loss of leadership (Meyer and Lehnerd, 1997, etc). This paper highlights that functional expansion may be a major obsolescence factor from the viewpoint of a product architecture. This suggests that regarding watching for signs of platform obsolescence, design engineers should be very attentive to obsolescence that may originate from functional expansion.</p> <p>3/ If an engineering department denies or ignores functional expansion and chooses traditional cost reduction strategies under functional expansion (e.g. modularization, reduction of the nominal costs of the individual functions, etc.), the results are likely to be disappointing, or even worse.</p>
Managerial implications	<p>Given its dramatic impact on product architecture and on costs, functional expansion is a variable which should not be ignored / which should be integrated in the traditional economic models which are used in rule-based design (in particular, in the economic models which assess the feasibility (both technical and economic) of the target, in the models which help budget projects, etc.)</p> <p>From a managerial perspective, our results highlight the importance of integrating functional expansion in the models and tools that are used when it comes to budget, to assess the technical feasibility of a new rule-based design project. This would involve shifting from traditional economic models which only optimize the budget supposed to enable to reach a fairly well-known target to economic models which also plan the emergence (perhaps not major, but still significant) of unknown events within the seemingly stabilized product architecture</p>
Limits and perspectives	<p>Wouldn't recent industrial catastrophes be symptomatic of a denial of functional expansion and its impact on product architecture, which would have led to a misuse of traditional cost reduction strategies, which then would have entailed treachery?</p> <p>Simulating more sophisticated cost-reduction strategies under functional expansion (e.g. combining the 'continuous modularization' with other strategies...)</p>

#### 4.2. Summary of Paper 2: Managing Learning Curves in the Unknown: from Learning by Doing to Learning by Designing

Current version: Working paper

Previous version: Agathe Gilain, Pascal Le Masson, Benoit Weil (2018). Managing Learning Curves in the Unknown: from Learning by Doing to Learning by Designing. R&D Management Conference, Milan, July 2018.

Central in this paper is the case of the Flextrack robot (Airbus Saint Nazaire), presented in details in Section 3. It aims at determining whether this case provides empirical evidence to validate the design theory-based investment decision-making model proposed at the end of section 2.2.3.



**Figure 12. Dramatic performance shift following the deployment of the prototype**

As previously explained, given the economic calculation outcomes summarized in the blue curve, and given the profitability threshold represented by the black line, the fact that an investment was conceded to deploy a Flextrack prototype in the production line of the A330 is puzzling. The project did not respect the traditional investment rule. Was it a mistake? A risky gamble?

In addition, the shift from the blue to the green probability distribution is striking: we are in the face of the same investment project, on the same product line (A320), on the same work station. And the blue and the green curve respectively represent two very different investment projects. How can the shift from the blue to the green curve be explained?

This leads to phrase three explanatory assumptions:

H1: we are in the face of a deviant, irrational (and worrying) economic calculation practice which, in this very case, ended well (thanks to luck), but such that nothing guarantees that it will not lead to a failure if it is reiterated.

H2: we are in the face of an outstandingly performing uncertainty-reducing-learning-by-doing mechanism.

H3: or, contrary to appearances, would there be ‘unknowns with impact’ in this case? Would the decision to invest and the generation of the huge profits be the result of an original economic reasoning in the unknown that would take into account to possibility to generate unknowns with (economic) impact?

Based on these assumptions, we phrased two research questions:

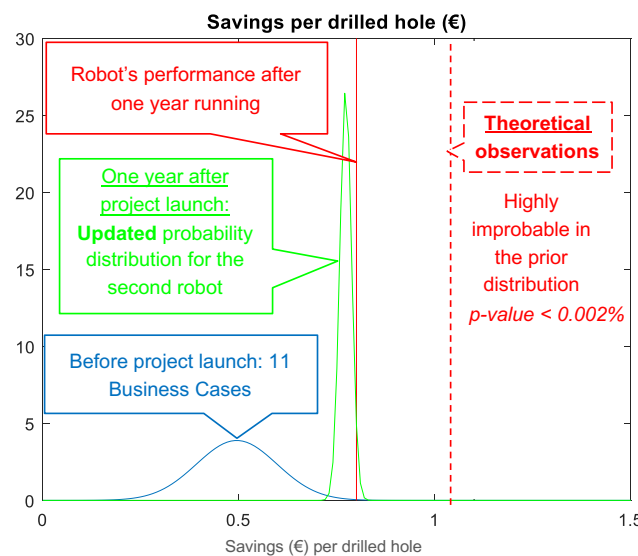
RQ2-S1: Was the decision to invest in the Flextrack project supported by the perspective of learning in the unknown? / Is the remarkable learning curve of the Flextrack robot deployed in Airbus Saint Nazaire plant attributable to unknown exploration?

RQ2-S2: How can we characterize the learning approach and the management principles (in terms of investment decision, economic steering and learning strategy) which resulted in the observed dramatic performance gains (i.e. the observed learning curve)?

### Statistical test

In order to address RQ2-S1, we carried out a statistical test in the first part of the paper. Bayesian statistics only account for the transformation of a priori knowledge into a posteriori knowledge. Since there is no information to update about initially unknown states of the world, Bayesian statistics do not manipulate the unknown. They only cover uncertainty reduction on a fixed  $\{\theta_1, \theta_2, \theta_3, \dots, \theta_n\}_{i=1..k}$  state-space. So if the move from the blue to the green curve results from an outstandingly performance uncertainty-reduction mechanism (i.e. if H2 is true), the green curve should be a Bayesian update of the blue curve, based on the new information (the learning) offered by the red curve, i.e. the learning allowed by the prototype.

In order to test whether the green curve is indeed a Bayesian update of the blue curve based on the learning brought by the red line, we have determined the theoretical “red” information which should have been observed so that the green curve is indeed a Bayesian update of the blue curve: this theoretical observation is represented by the dashed line in Figure 22. The test reveals that this theoretical observation is very far from what was observed in reality. In addition, it corresponds to a value which it is highly improbable (almost impossible) to draw in the blue distribution.



**Figure 22. Statistical test which highlights that the move from the blue to the green curve cannot be attributed to a sole mechanism of uncertainty reduction**

Thus, it is absurd that the green curve results from an update of the states of the world contained in the blue curve. This statistical test dismisses the assumption according to which the green curve results from uncertainty reduction (with a very high statistical significance:  $p\text{-value} < 0.002\%$ ). This suggests that during the move from the blue to the green curve, some new states of the world, some new  $\theta_i$  with impact may have been designed, i.e. the unknown has been explored.

The idea according to which the move from the blue to the green curve results from unknown exploration can be debated: for instance, what guarantees that the level of risk aversion and conservatism that was involved when the blue curve was computed was not higher than for the green curve? (This would suggest that the blue curve represented in Figure 22 is actually more to the left than it actually was). To what extent could this change the result of the statistical test? Such discussions highlight that for the Flextrack study, it is necessary to double-check the statistical test. In an attempt to empirically confirm the presence of the unknown, we studied the Flextrack case with a single case study approach (Siggelkow, 2007).

But beyond the debate about its capacity to really highlight the unknown in the specific case of Flextrack, the application of this statistical test provides interesting insights regarding the issue of ‘economic evaluation in the unknown in innovative design’. Indeed, formally speaking, this statistical test detects the newly designed  $\theta_i$  with impact. This means that an actor who ensures that the same level conservatism and risk aversion is applied for computing a priori and a posteriori distributions can use this test to detect the unknown. In other words, the statistical instrument used in this case to detect the unknown (without going as far as measuring it) could lead to imagine operational tools to manage the unknown in organizations.

### **Empirical investigations**

As indicated in section 3 (Table 1), these empirical investigations rely on empirical data collected from Airbus internal documents and interviews.



In order to determine whether unknown has been explored and structured during the Flextrack project, we have used the C-K theory (Hatchuel and Weil, 2002, 2009), and investigated whether we found traces of an innovative design reasoning.

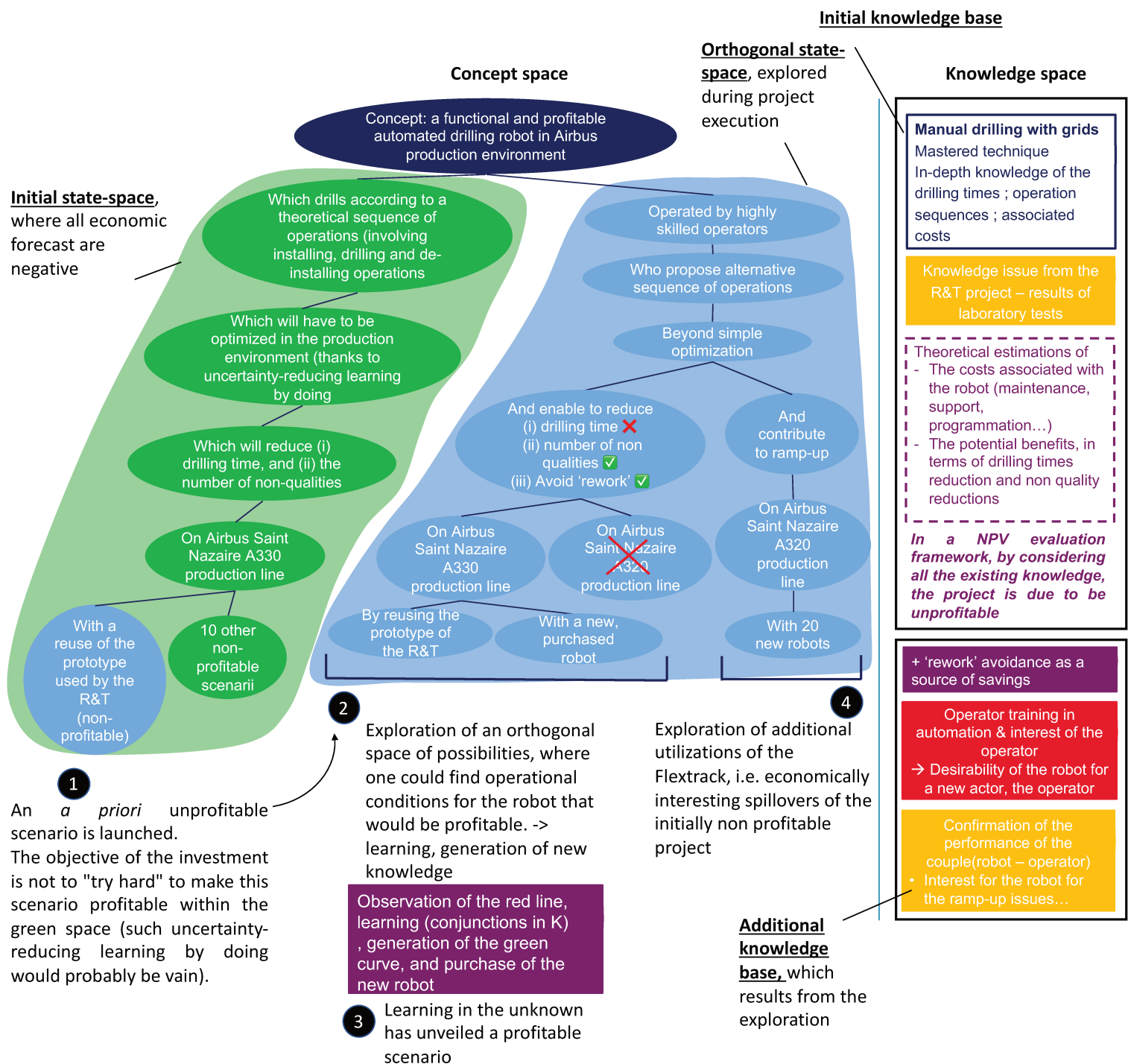


Figure 23. Representation of the Flextrack investment project with the C-K formalism – one observes the presence of an innovative design reasoning

The analysis of the Flextrack case with the C-K formalism clearly highlights that the project started within one initial concept space, associated with one initial knowledge base, and that

during project execution, an additional (orthogonal) concept space was explored, and additional knowledge bases were generated.

The initial state space was explored quite widely before project launch: study of eleven scenarii, associated profitability analyzes. Numerous meetings were dedicated to this exploration ‘on paper’, which followed the R&T project. But according to the knowledge contained in the initial knowledge base, the concepts contained in this initial concept space were due to be unprofitable. In spite of that, among the 11 concepts that had been studied, the least resource-intensive concept was selected and given funding through an unconventional funding approach: the R&T department of the plant, which was interested in seeing the follow-up of the R&T project in the plant, provided the investment, so that the plant could retrofit and deploy a prototype (which was actually the prototype that the R&T had used for its laboratory experiments during its project) within the production environment.

When deploying the prototype in the production environment, the managers in charge of the project did not ‘try hard’ to make the selected concept profitable within the initial concept space (this would have probably been vain – the profitability studies had clearly shown that this concept space was unprofitable). Rather, the managers in charge of the project moved to another concept space – more precisely, they seized the opportunity to move to another concept space, when the operators who manipulated the robot started being interested in it, and became highly skilled. The exploration of this other concept space generated a base of additional knowledge, represented in Figure 23. And in the new concept space, with the new knowledge, new conditions of use of the robots were determined, which were profitable. This led to purchase one new robot, from a supplier, as indicated at the end of the second branch of the new concept space. In addition, the convincing performance of the robot led to consider the introduction of the robot on a new, strategic production line, that of the A320 concerned with a ramp-up problematic: this can be seen as a spillover from the investment in the deployment of the prototype, which was initially due to be all but profitable.

These elements highlight that the profitability of the Flextrack robot is not due to chance. It is rather the result of an intentional and very structured exploration of the unknown.

One can stress that the performance reached by the Flextrack robot results from one very specific learning regime, which can be distinguished from the learning-by-doing learning regime generally expected when one deploys a new machine in a production environment. The classical, generally expected learning regime consists in learning by collecting a sample of information related to already identified states of the world (i.e. related to the information contained in an initially known, but uncertain knowledge base), in order to confirm or infirm an assumption. In the case of the Flextrack robot, this would that consisted in deploying the prototype in the production environment, ‘letting it run’, and after one year, collecting a sample of ‘x’ performance values, and deciding to purchase a new robot if the sample of values reveals a good performance of the robot, or deciding to kill the profit if the sample of values reveals the contrary. In this case, we are in a traditional decision theory framework. The value of learning is the value of information, obtained by sampling. Learning is Bayesian: it is an update of the uncertain knowledge contained in an initial knowledge base, an update of the

knowledge related to initially identified states of the world. This is learning by reducing uncertainty. And the generated economic value is at best, that of a Bayesian move, i.e. a moderate shift. This is the value of Learning by Doing.

But in the case of the Flextrack robot, learning did not occur within the initial concept spaces and knowledge spaces. A new concept space was explored. New states of the world were designed, which opened new sources of profitability. The created value is not the value brought by the collection of a simple sample of information. In the Flextrack case, the learning approach did not consist in deploying the robot, letting it run, and after one year, collecting a sample of information that will either recommend to kill the project or further invest in it. In the Flextrack case, learning is learning by exploring the unknown.

Finally, this case suggests that if the unknown is adequately dealt with (with a learning by designing approach, which allows the exploration new concept spaces), the unknown seems to allow remarkable value enhancement (the value of learning by designing, beyond the value of traditional, uncertainty-reducing learning by doing).

This case which involves an investment decision in the unknown, and a rigorous exploration of the unknown provides partial empirical evidence for a design theory-based investment decision-making model. In particular, we do not go as far as quantifying the value of the information collected by exploring the unknown, although we propose a first tool which formally detects the presence of the unknown. In addition, the narrative does not exhibit the methods, techniques, tools, etc. that were used to explore the unknown. These aspects are addressed in Paper 3.

**Table 4. Summary of the second paper**

<b>Paper</b>	<b>Managing Learning Curves in the Unknown: from Learning by Doing to Learning by Designing</b>
Research questions	<p>RQ2-S1: Was the decision to invest in the Flextrack project supported by the perspective of learning in the unknown? / Is the remarkable learning curve of the Flextrack robot deployed in Airbus Saint Nazaire plant attributable to unknown exploration?</p> <p>RQ2-S2: How can we characterize the learning approach and the management principles (in terms of investment decision, economic steering and learning strategy) which resulted in the observed dramatic performance gains (i.e. the observed learning curve)?</p>
Literature background	<p>Investment projects and learning mechanisms:</p> <ul style="list-style-type: none"> <li>- If a project is an investment under uncertainty, the decision to launch it can be justified by integrating uncertainty in economic calculation. In particular, uncertainty-reduction-oriented learning curves enable to do this. Indeed, the perspective of moving down a learning curve characterized by a forecast learning rate can justify the decision to launch a project that is initially non profitable (e.g. a negative NPV).</li> <li>- But if a project is an investment in the unknown, one does not have means to highlight the unknown, and to justify the interest in investing in the unknown. One does not know how to assess the value associated with the perspective of exploring and structuring the unknown</li> </ul>

Literature gap	What is a learning curve in the unknown? What managing a learning curve dynamic in the unknown would be?
Research objectives	Determining whether the Flextrack project simply involves odd, deviant practices and economic reasoning in the framework of decision theory, or whether it would not hide a more sophisticated investment reasoning in the unknown
Theoretical framework	Uncertainty-reducing learning by doing mechanisms Decision theory-based decision models Design theory (Design theory-based decision models)
Material	Empirical material: data from Airbus Saint Nazaire plant
Method	A statistical method (Bayesian statistics) to address RQ1 A empirical method (interviews, access to presentations) which complements our answer to RQ1 (double check) & addresses RQ2
Results	(RQ2-S1) The high profitability of the Flextrack robot does neither result from luck, nor from uncertainty reduction. It results from an exploration of the unknown (RQ2-S2) The exploration of the unknown was carried out with a rigorous learning approach.
Academic implications	Economic performance in the unknown can be rigorously managed, with an approach that is specific and adapted to the unknown, and that uses classical economic notions (the NPV) in a diverted way: this paper seems to have unveiled first empirical evidence for a design-theory-based investment decision model.  The results also highlight a learning dynamics, learning by designing, which can be seen as an extension of uncertainty-reducing-learning-by-doing to the unknown.
Managerial implications	Our results can contribute to change managers viewpoint on the unknown, which is generally perceived as very risky, as a threat and as something which cannot be managed economically
Perspectives	- In an economic calculation, can we estimate the value associated with the exploration of the unknown? - Can we confirm with on larger sample that the exploration of unknown can generate a lot of profitability, more than uncertainty reduction?

#### 4.3. Summary of Paper 3: Why is investing in the unknown less risky and more profitable than investing under uncertainty? Model and empirical evidence

Status: working paper to be submitted to Creativity and Innovation Management Journal

Previous version: Agathe Gilain, Pascal Le Masson, Kevin Levillain, Yannick Marin, Benoit Weil. How to enhance the profitability of your project portfolio – by reducing uncertainty or exploring the unknown?. IPDM, Jun 2019, Leicester, United Kingdom. {hal-02095624}

This paper focuses on the case of the portfolio of incubation and acceleration projects introduced in Section 3. It aims at determining whether this project portfolio provides validates the hypothetical design theory-based investment decision-making model introduced in Section 2.2.3.

As a reminder: at the entrance of the portfolio are candidate projects which traditional organizations have chosen not to launch. Among these projects, the portfolio selects the

difficult, technology-based problem-driven topics. It selects projects among these whose level of uncertainty or risk is the reason why the traditional organizations have chosen not to launch them. (Economic calculation either renders negative results or is simply impossible (a so high level of uncertainty that economic data cannot be quantified)).

For a selected project, two states of the world are possible: E1: profitable. E2: unprofitable. And the belief that E2 is the true space is very high:  $P(E2) = 0.95$  (subjective probability confirmed by the innovation leader who manages the portfolio). Then, if a project is selected, it is offered incubation or acceleration means (a budget, 100 days, a methodological support, a place, facilities...) to explore the concept it proposes. During incubation / accelerations, observations are made, which can be seen as leading an advice given by the portfolio organization on the profitability of the project. U1: profitable ; U2: unprofitable.

Based on this advice, several statuses are possible for the project which has gone through the portfolio: being stopped (inconclusive, disappointed outcomes), stored (valuable outcomes, but which do not give rise to a following project), launched by the R&T department, or implemented in a traditional organization (the very same that had perceived them as very risky and had chosen not to invest at the beginning of the process). In 2018, the portfolio demonstrated a remarkable profitability (x6 rate of return)<sup>33</sup>. In addition, among 55 projects incubated or accelerated within this portfolio between 2013 and 2018, 41 (i.e. 75%) gave rise to further investments within the very same organizations which had chosen not to launch them. These projects are launched with the belief that  $P(E1 | U1) = 0.80$  (i.e. a 80% confidence that the investment will eventually pay off, if the advice of the portfolio is a recommendation to launch the project).

As mentioned above, although innovation is the core of the mission of the innovation cluster in charge of the portfolio, the decision to invest in these highly uncertain projects that are due to be all but profitable remains daring. In addition, the generation of a x6 rate of return for a portfolio which selects highly uncertain, seemingly unprofitable projects is puzzling. Indeed, classical decision theory-based models predict that the more uncertain a project is, the lower the probability of making it profitable, so the lower its expected profitability. Put differently, the probability that an uncertainty reduction mechanism generates 'considerable' value is generally extremely low, according to decision theory.

So we can phrase the same explanatory assumptions as in the case of the Flextrack project:

H1: we are in the face of a deviant, irrational (and worrying) economic calculation practice which, in this very case, ended well (thanks to luck), but such that nothing guarantees that it will not lead to a failure if it is reiterated.

H2: we are in the face of an outstandingly performing uncertainty-reducing-learning-by-doing mechanism.

H3: or, contrary to appearances, would there be unknown in this case? Would the decision to invest and the generation of the huge profits be the result of an original economic reasoning in the unknown, taking into account to possibility to generate unknowns with (economic) impact?

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<sup>33</sup> For 1 euro invested in the portfolio, a promise of 6 euros given back to Airbus

Based on these assumptions, the paper aims to address to research questions (very similar to those addressed in the previous paper):

RQ2-S1': Are the remarkable profits of Airbus portfolio of incubation and acceleration projects attributable to unknown exploration?

RQ2-S2': How are project selection and unknown exploration carried out in the frame of Airbus portfolio? Do these practices confirm the assumptions which characterize our hypothetical design theory-based investment decision model?

We can rapidly test H1 . Did the portfolio randomly select 55 projects, among which 41 were profitable? The probability of such a scenario is  $p = \binom{55}{41} \cdot 0.05^{41} \cdot 0.95^{14} \approx 9.7 \cdot 10^{-42}$  . In other words, the scenario associated with H1 is highly unlikely. We eliminated it.

Then, in order to test H2, we tried to determine whether the information collected during incubation / acceleration only updated initial identified states of the world, which resulted in reduced uncertainty only. To that end, we represented in a decision theoretic framework the probabilistic data  $P(E1)$ ,  $P(E2)$ ,  $P(U1)$ ,  $P(U2)$ ,  $P(E1|U1)$ ,  $P(E1|U2)$ . We tested whether the probabilistic beliefs related to the profitability of the projects correspond to beliefs that have been updated according to a Bayesian mechanism. To do so, we computed the reliability of the observation instruments that should have been used to observe the profitability of the projects: and we found that if the values  $P(E1)$ ,  $P(E2)$ ,  $P(U1)$ ,  $P(U2)$ ,  $P(E1|U1)$ ,  $P(E1|U2)$  obey to a Bayesian law, the observations  $U1$  and  $U2$  should have been made with an instrument such that  $P(U2|E2) < 0.26$ : in other word, an observation which predicts profitability when a project is not profitable... which is absurd and invalidates H2. (The test is described in more details in Paper 2).

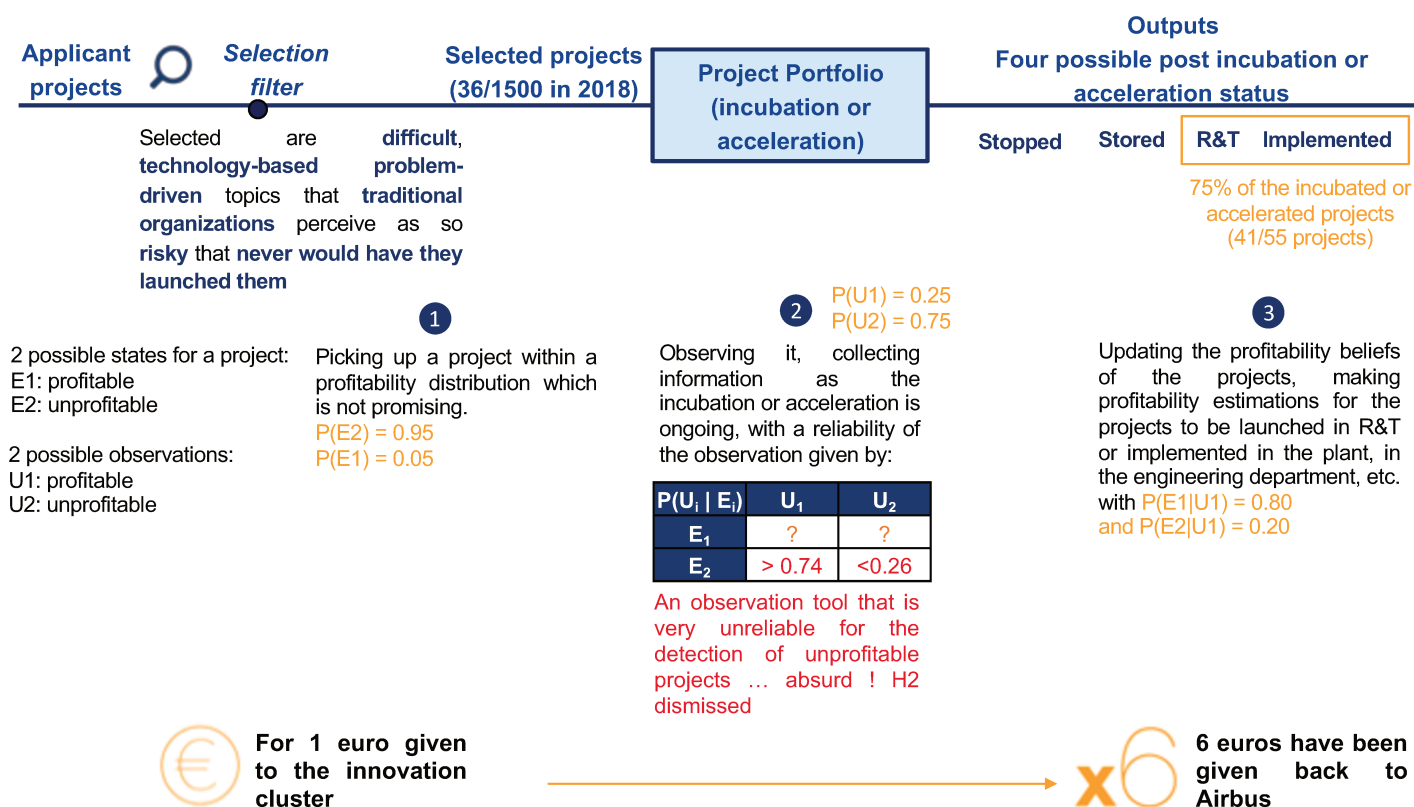


Figure 24. Representation of the probabilities that describe the portfolio in the decision theory framework

This statistical test suggests that between the arrival and exit of a project in the portfolio, the unknown was explored and structured, new states of nature were designed.

After this statistical test, Paper 3 investigates whether specific methods, tools, and managerial indicators enabling to rigorously explore the unknown are used in the framework of the management of this portfolio. To this end, the paper involves a case study (Goffin et al., 2019).

This case study revealed that the innovation cluster's investment decisions are extremely documented, informed and structured. After a first selection filter applies at the level of the applicant projects' 'home' organizations, some short-listed projects are invited to enter the second phase of the selection process which involves a pitch in front of a selection board (attended by top-managers, (e.g. head of competence centres, as potential sponsors of the project)). To prepare the pitch, the innovation cluster offers the short-listed projects the support from methodological coaches. These latter encourage the project owners to gather all the knowledge that already exists (especially within Airbus) on their topic and to make a solid project definition based on what exists in the different competence centres. This is all the more so important as one important selection criterion, called 'Not done elsewhere', requires that any engineer, any expert, any department etc., who is already working on the candidate topic within Airbus joins the application project: if he or she is not onboard, the project will not be selected to be incubated or accelerated. During the pitch preparation phase, the

candidates are also encouraged to demonstrate how the project is in line with the strategy of the Airbus, which is also a selection criterion. This phase can already be seen as the beginning of the exploration of new knowledge bases, or even new branches of concepts. During the selection process, the innovation cluster mobilizes numerous stakeholders, numerous competences (innovation catalysts, top managers and high level sponsors, methodological coaches, experts etc.) and enforces clear and strict criteria in order to detect and select projects which are genuinely innovative and which have the potential to be turned into “*real business applications*”. These selection criteria are applied in a very systematic way and select projects which all have the same very specific profile: projects rejected by traditional organizations, perceived as highly uncertain<sup>34</sup>, aligned with Airbus strategy and roadmap, mobilizing the existing Airbus knowledge related to the topic, involving prototyping perspectives, etc. All these elements support the assumption according to which the innovation cluster has developed a kind of unknown-unveiling and unknown-sensing capability, which detects projects where there is room to explore the unknown, i.e. there is room to design new states of the world.

Then, during project run, the selected projects benefit from a strong level of support: an access to a methodological package, enabling them to carry out their project method methods such as C-K theory, Agile, Design thinking, Learn UX, ... The innovation cluster ensures that all the relevant existing knowledge is available to the project team. On the one hand, this is made possible thanks to the multidisciplinary of the project (and by implication the multidisciplinary of the team). On the other hand, one person from Airbus information centre is dedicated full-time to take in charge any documentary research that the team would need in the frame of the project. Regular steering committees also enable the project to benefit from the view and the knowledge of the heads of the competence centres (which can help unblock some problems). Besides, one person from Airbus costing department provides support regarding the economic questions.

In terms of management, the innovation manager in charge of the portfolio requires a demanding reporting from the project team. In order to follow the creation of economic value, he keeps track of them with the use about more than 30 indicators (even 99 if we do not consider that some are mutually exclusive), which summarize the progress of the project, its maturity, which characterize the prototyping effort, the economic figures... The projects are well equipped, supported (in terms of methods, tools, competences, prototyping means...) and rigorously managed (a high level of reporting) to investigate a topic that calls for an exploratory approach.

In summary, we found that the portfolio is managed with a large set of methods that help select projects associated an initial knowledge base and an initial concept space such that the concepts of the conceptual space are unprofitable, but which contain unknown, i.e. room for exploring an ‘orthogonal’ concept space. So the portfolio provides funding for one of these initial unprofitable concepts. And the purpose is by no means not to try hard to make this

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<sup>34</sup> In the final steps of the selection, the innovation leader in charge of the portfolio systematically favors non-consensual projects, i.e. projects the pitch of which has both been given very good and very bad marks by the members of the selection board



very concept become profitable. The project owners are mandated to explore another, orthogonal concept space, within which new learning will be made, and conditions for profitability will be discovered. During its execution, the project can use specific methods which help explore the unknown. And the building of economic value as the project progresses is followed closely by the innovation leader.

Finally, after the case study which has confirmed the statistical test, i.e. which has confirmed the role of the unknown in the portfolio, the paper comes back on the probabilistic data, and shows that it is possible assess the value of the information that were collected by exploring the unknown. This shows that in the framework of a design theory-based investment decision model, the value of the unknown can be assessed as rigorously as the value of uncertainty can be assessed thanks to decision theory-based models.

The contributions of the paper are the following.

On a sample of 55 projects, this case provides statistical and empirical evidence for a design theory-based investment decision model, where the decision-makers' capacity to (i) detect projects that contain unknown, and (ii) deploy capabilities, methods and managerial approaches that allow the exploration of the unknown, are necessary conditions that need to be met in order to effectively generate profits. Within this model, investing in the unknown may be less risky and more profitable than investing in high, basic uncertainty: the law of decision theory according to which the more uncertain a project is, the lower the probability of making it profitable no longer applies. The huge returns of the portfolio which were incomprehensible in the decision theory-based framework become comprehensible, explained, in this design theory-based framework. And in this case, the statistical instrument which detects the unknown also quantifies the value of unknown exploration (more specifically, the value of information in the unknown). Beyond being an experimental instrument in this specific case, this could be seen as a basis for an economic calculation in innovative design.

**Table 5. Summary of the third paper**

<b>Paper</b>	<b>Why is investing in the unknown less risky and more profitable than investing under uncertainty? Model and empirical evidence</b>
Research questions	<p>RQ2-S1': Are the remarkable profits of Airbus portfolio of incubation and acceleration projects attributable to unknown exploration?</p> <p>RQ2-S2': How are project selection and unknown exploration carried out in the frame of Airbus portfolio? Do these practices confirm the assumptions which characterize our hypothetical design theory-based investment decision model?</p>
Literature background	<p>When it comes on whether or not to invest in the unknown, there is a lack of convincing economic models: the exiting economic models supposed to guide investment decision-making derive from the decision theory and confront investors to an unsatisfactory dilemma: either investing in a 'pay and let us see what happens' philosophy, which is equivalent to gambling ; or killing the project and running the risk of missing a promising opportunity. This dilemma is based on the fact that according to profitability considerations based on decision theory, investments in</p>

	<p>the unknown are predicted to have a very low expected profitability and to be very risky gambles.</p> <p>It was recently demonstrated that design theory extends decision theory to the unknown (Le Masson et al., 2018). And if applied in an investment situation in the unknown, the proposed design theory-based decision model theoretically predicts that it is always possible to reach one's profitability target without necessarily throwing one's lot with chance, if one designs the judicious states of the world or the judicious decisions to this end. On the one hand, this theoretical prediction is very counterintuitive, even absurd (it suggests that one could always design profitable opportunities in the unknown). On the other hand, it is very attractive, because it suggests that investment decision-making and profitability generation in the unknown could not only be a matter of luck, but could be rigorously (quantitatively) controlled. This is more convincing than the propositions of the decision theory-based investment decision models within which one necessarily needs to gamble if one wants to invest in the unknown.</p>
Literature gap	Design theory-based investment decision models could be a promising alternative to the unsatisfactory decision theory-based models when it comes to make investment decisions in the unknown. But to date, these models are based on theoretical advances which did not yet go as far as specifying an 'applicable' investment decision model in the real world, in which 'profitability in the unknown' could be rigorously (ideally, quantitatively) controlled.
Research objectives	<p>Investigating whether we could find empirical evidence (e.g. a case providing a large sample of projects) for the predictions of design theory regarding profitability dynamics in the unknown.</p> <p>Getting more specific insights regarding what a design theory-based investment decision model would be in the real world (in terms of methods, organization, computational approach of value, etc.)</p>
Theoretical framework	Design theory (Design theory-based decision models)
Material	Empirical material: data from Airbus innovation cluster's portfolio of incubation and acceleration projects
Method	<p>A statistical method (Bayesian statistics) to address RQ1</p> <p>A empirical method (interviews, access to presentations) which complements our answer to RQ1 (double check) &amp; addresses RQ2</p>
Results	<p>(RQ2-S1') The remarkable economic outcomes achieved on 55 projects by Airbus portfolio of incubation and acceleration projects can be attributed to the unknown. This unknown can be explicitly detected with a statistical test.</p> <p>(RQ2-S2') -Provided one is able to detect and select projects that contain unknown, and provided one is able to rigorously manage them with adapted unknown exploration methods, investing in the unknown can be very profitable and little risky</p> <p>- The value of the new information collected by exploring the unknown can be quantified, in the same fashion as decision theory-based techniques quantify the value of the new information collected by reducing uncertainty</p>
Academic implications	Our results confirm the predictions of the design theory-based decision model regarding profitability: when one shifts from basic uncertainty to the unknown, there is a rupture in terms of profitability logics. Profitability no longer follows the prediction of decision theory.

	<p>This contributes to the literature in management which had already pointed that generating a lot of profits thanks to innovation is more a question of good management and organization than a question of luck</p> <p>The fact that the value of unknown exploration can be valued confirms that economic calculation can be extended to the unknown (in the frame of a design-theory-based economic calculation)</p>
Managerial implications	<p>Our results can contribute to change managers viewpoint on the unknown, which is generally perceived as very risky.</p> <p>On the one hand, they represent call for developing capacities of detecting and rigorously managing the unknown and largely applying them, since the profits can be dramatic. However, the acquisition of design-based methods can by no means be considered by firms as a magic bullet which enables them to generate systematic returns out of the unknown. Generating a lot of profits in the unknown requires an 'compatibility' between the nature of the unknown which is identified and the efficiency (the power) of design method one owns.</p>
Perspectives	<ul style="list-style-type: none"> <li>- Further research regarding the 'compatibility' between the nature of the unknown which is identified and the efficiency (the power) of design method one owns.</li> <li>- Discussion the organizational aspects that would be associated with the deployment of 'economic calculation in the unknown' in R&amp;D firms?</li> <li>- Could the economic reasoning in the unknown help guide decision-making in the frame of threats (global warming, social crises, sanitary crises...) marked by a strong level of unknown?</li> </ul>

## 5. Findings: contributions to the topic of economic calculation in the unknown

The starting point of this research work is twofold:

- it is well-known that decision theory provides relevant techniques for economic calculation under uncertainty. These latter quantify the value of information and guide decisions under uncertainty. They are very relevant for economic calculation under uncertainty, but they ignore the value of the unknown. It is now well-proved that such techniques are unsuitable, even misleading in a situation that contains unknown. (e.g. Loch et al., 2006). If design theory extends decision theory to the unknown (Le Masson et al., 2018), it did not go as far as providing computational techniques to assess the value of information in the unknown. In other words, there seem to exist no economic calculation techniques, no economic evaluation instrument in the unknown.
- at the same time, rule-based design and innovative design activities are confronted to 'not obviously visible unknowns' for which it would be interesting to assess the impact. Several forms of evaluation and characterization could be considered, but it seems particularly relevant to carry out 'economic evaluation' for a first characterization. It will allow R&D firms to know whether they should be concerned by these invisible unknowns.

In this context, we have formulated the following research questions.

<b>Regarding the topic of 'economic evaluation in the unknown in rule-based design'</b>
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(RQ1): To what extent can the unknown that emerges in rule-based design be ‘with (economic) impact’?

Regarding the topic of ‘economic evaluation in the unknown in innovative design

(RQ2): In the framework of projects that are undecidable from a decision theory-based viewpoint, to what extent and with what kind of tools could ‘unknowns with impact’ be the objects of a design theory-based economic value management, in particular an informed investment decision-making reasoning that is not based on a betting logic?

We have started by addressing sub-research questions for these questions. This has led us to design and experiment two instruments which measure ‘not necessarily obviously visible unknowns with impact’. In rule-based design, our instrument has measured extremely costly ‘unknowns with impact’ (induced by functional expansion). And in innovative design, our instrument has highlighted ‘unknowns with impact’ which, provided they are detected by the actors and managed with appropriate design-based methods, can generate dramatic economic returns.

The following subsections summarize and further develop these ideas.

#### 5.1. Evaluating the economic impact of the unknown in rule-based design (RQ1)

##### 5.1.1. Functional expansion identified an ‘unknown with impact’, thanks to a cost-escalation-curves-simulation instrument

Within RQ1, we focused on the emergence of a specific form of unknown in rule-based design: the unknown introduced by functional expansion in an already-existing architecture. Our objective was to get a better understanding of this unknown, which is not obviously visible (not as obviously visible as a disruptive innovations are), but which emerge on a continuous basis – with an ‘instrument’ which simulates cost escalation curves, we have assessed the economic effect of this specific form of unknown that can affect rule-based design products.

We highlighted that the unknown introduced by the newly added functions can trigger major shocks in product architecture, disturbing a seemingly solid capital of rule-based design assets. If ill-mastered, these shocks may have considerable economic impact: they may generate exponential development costs escalation dynamics.

**In other words, functional expansion is an unknown with impact.**

We also found that the unknown introduced by functional expansion tends to disturb the effectiveness of the cost management techniques that are classically used in pure rule-based design (without functional expansion): functional expansion undermines the cost-reducing power of modularization strategies. Thus, designing a product architecture that is robust to the shocks which functional expansion generates in an already-existing product architecture requires from engineering departments capabilities that are specific to functional expansion. In our first paper, we have identified one kind of strategy that seems interesting to master the effects of functional expansion: re-modularizing the architecture after each phase of functional expansion, i.e. continuously designing independences, in the unknown. Such an architectural strategy restores the cost-reducing power of modularization under functional expansion.

This suggests that functional expansion in rule-based design requires a double-deliverable from engineering departments:

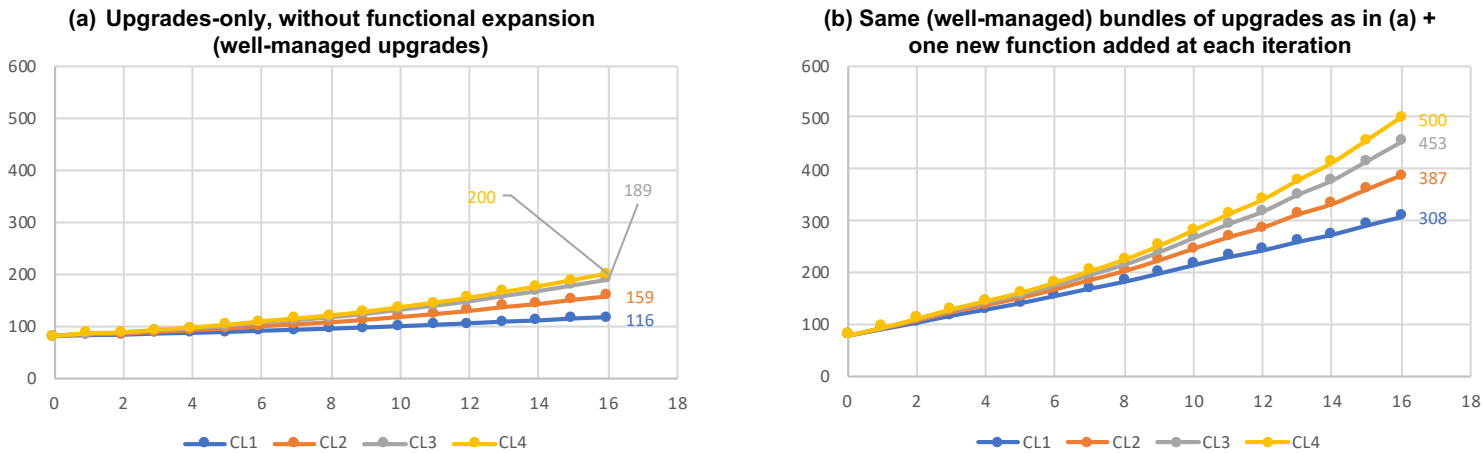
- ensuring that the functional requirements of the individual product functions will be effectively met, namely in terms of cost, quality and delay: this is the classical rule-based deliverable
- continuously decoupling the expanding functional space, i.e. continuously designing independencies in the unknown. Required by the presence of functional expansion, this deliverable is an outcome of innovative-design activities which are generally not expected in rule-based design.

The design efforts related to the first deliverable (ensuring that the functional requirements of the individual product functions are effectively met) generate economic returns which will be immediately visible: they are carried out during the development program responsible for designing a new product generation, and they pay off as soon as this newly developed product is released on the market (with the revenues being determined by the extent to which the cost-quality-lead-time objectives are met (namely from the viewpoint of customers)). In contrast, the design efforts related to the second deliverable (continuously renewing independencies in an expanding functional space) generate economic returns which are less visible, and will pay off in a longer run. Indeed, in the first steps of a functional expansion dynamics, only a small number of functions have started accumulating: neglecting the decoupling of the functional space might seem to have no serious impact, perhaps only a small number of additional propagation paths to manage, negligible excess costs, etc. But as new functions accumulate in the functional space, there is a combinatorial explosion of the number of possible propagation paths, and the costs for managing the propagation paths induced by the new functions increase exponentially. Therefore, although its effects are less visible, and although it seems more ‘prevention-related’, this design effort is indispensable. As we will discuss in more details in Section 6.2, identifying the ‘places’ in the architecture where this kind of design effort is needed and implementing a continuous decoupling effort pose significant decision-making and organizational challenges.

Beyond the fact that according to our simulations of propagation costs, neglecting the renewal of architectural independencies in an expanding functional space may generate exponential cost explosions, we can also ask whether the denial (or the ignorance) of a functional expansion phenomenon might not be a possible explanation for a scenario of industrial catastrophe.

#### 5.1.2. New explanations for recent design crises offered by economic calculation in the unknown?

In Figure 21 (reminded below), we noted that if an engineering department is already proficient at mastering the cost of upgrades, the cost-reduction potential associated with a strategy that consists of cutting the cost of already-existing functions is very low. What would happen if a cost manager is misled by the steady functional-expansion-driven cost escalation curve generated by this engineering department (e.g. Figure 21b.) and decides to pressure the organizations responsible for the development of individual functions to cut their costs?



**Figure 21. Simulation of a product evolution dynamics where the upgrade of already existing functions is well-controlled by engineering departments.**

Left: family of cost escalation curves associated with an well-controlled, pure-upgrades-driven product evolution dynamics. Right: the same upgrades as on the left-figure have been simulated, but in addition, at each iteration, one new function was added.

A first (harmless) possibility is that this classic cost-cut strategy will yield little cost reduction and be disappointing. However, a second, more detrimental, possibility is that the cost cuts will prevent the engineering department from performing critical activities dedicated to observing the signs of obsolescence of the architecture. The engineering department will lack the necessary resources to prepare the renewal of the architectural design rules accordingly. And if a product is under functional expansion, the potential sources of architectural obsolescence are numerous: each newly added function may destroy a critical independency and require the restructuring of the architecture. Thus, cutting the cost of an engineering department in an attempt to reduce the nominal development costs of the functions may have a detrimental effect under functional expansion: depriving the engineering department of the necessary resources to observe and manage architectural obsolescence sources that would come from functional expansion. The non-detection of architectural obsolescence may have catastrophic impacts: launching the next development project based on a flawed architecture that will turn out not to allow functional requirements to be met, i.e. launching the next development project based on ‘false assumptions of feasibility’. When it is discovered that the assumptions of feasibility are false, this generates a crisis situation in the development project. And two options are possible: (1) acknowledging the fact that the project is in the face of ‘concept’ or ‘scope deviation’ (Ingvarsson et al., 2015), which will require the intervention of task forces, and entail changes in project resources, planning... or (2) denying the deviation, using somehow the flawed architecture, i.e. cheating with the risk that unanticipated propagation phenomena happen as the product is in service. We can ask whether the combination of an inappropriate cost-reduction strategy and an unnoticed functional-expansion-induced architectural obsolescence (caused by lack of engineering resources) which was then denied could explain design catastrophes, such as the case of the Boeing 737-MAX, Dieselgate, Samsung Galaxy Note 7 (which are all three incomprehensible in a pure rule-based design framework).

### 5.1.3. Dynamics returns and the benefits of a specific class of anticipative-learning-oriented overinvestments in rule-based design under functional expansion

The performance logic associated with pure rule-based design involves resources optimization, minimization of knowledge production... In rule-based design under functional expansion, does this logic still apply? Or could anticipative investments (i.e. investing in the generation of excess knowledge, ahead of time, that will be used later on) enhance economic performance along a product evolution dynamic under functional expansion? In order to study this question, we have run a simulation aiming at determining the conditions under which anticipative-learning-oriented overinvestments could pay off in rule-based design under functional expansion.

Our simulation relies on a model which seeks to establish a relationship between investment in knowledge creation (horizontal axis in Figure 24) and its effects in terms of product transformation (vertical axis in Figure 24). A technological frontier (the orange line) is defined, above which functionalities are not attainable. And we are interested in possible investment strategies to move from a certain functional level  $FL_0$  to a superior level  $FL_1$ . A minimum investment is necessary to move from  $FL_0$  to  $FL_1$  : one cannot switch freely (i.e. vertically) from  $FL_0$  to  $FL_1$ . This minimum investment is represented by the feasibility constraint (dotted line, left). An 'obvious' investment strategy at first sight is to reach  $FL_1$  with this minimum investment (red arrow: the ratio  $\frac{\Delta FL}{\Delta K}$  is maximized): this is the investment strategy in classical rule-based design. But we can also study over-investment strategies, in a logic of anticipated knowledge production (green arrow). This deteriorates the ratio  $\frac{\Delta FL}{\Delta K}$ . We define a threshold beyond which this deterioration of the ratio  $\frac{\Delta FL}{\Delta K}$  is no longer acceptable (criterion of minimum profitability delimited by the dotted line on the right. The authorized investments are those made in the cone delimited by the two constraints / the two dotted lines.

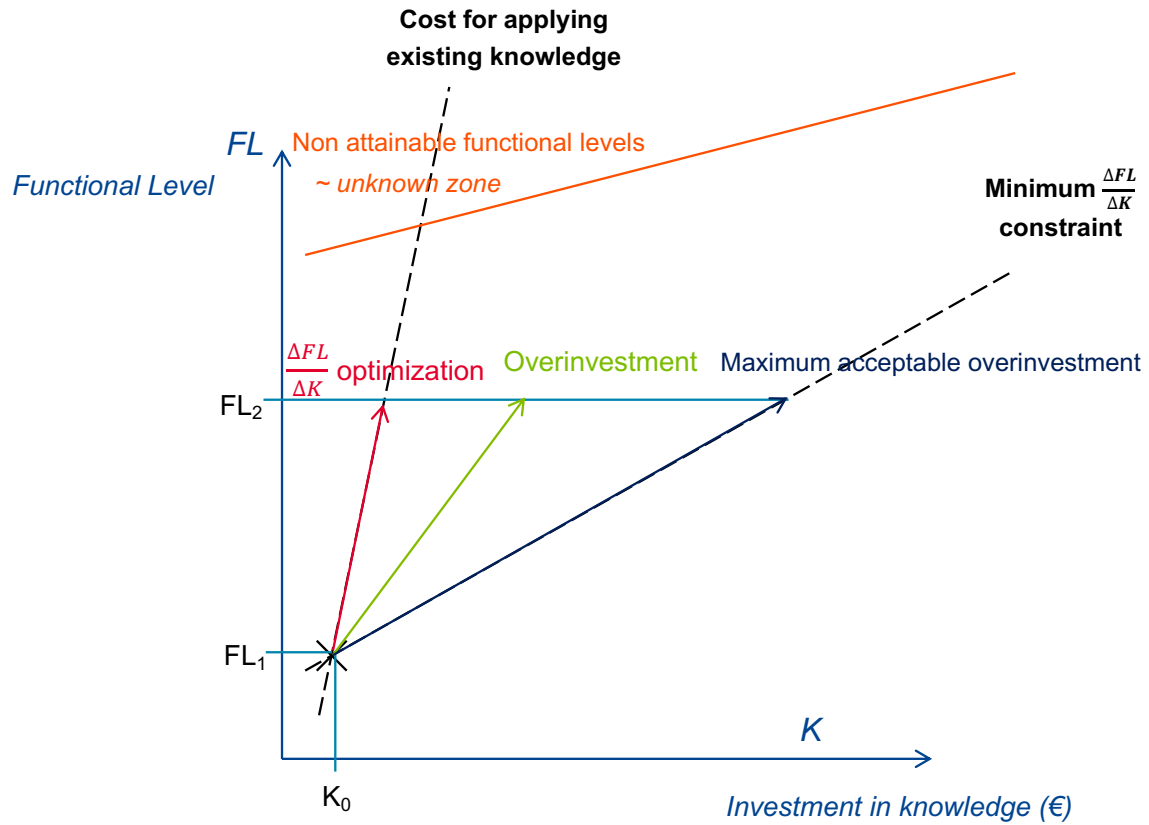


Figure 24. model of the relationship between investment in knowledge creation and its effects in terms of product transformation

We have attempted to identify the conditions under which it is more interesting to overinvest than to minimize the investment.

To this end, we have simulated different investment strategies deployed to reach a sequence of functional level (illustrated by the two schemes in Figure 25)

(a) The first strategy consists in choosing the minimum investment for each functional increment

(b) The second strategy is to over-invest. We tested cases where overinvestment was rewarded by shifting the technological frontier and cases where the technological frontier remained unchanged.



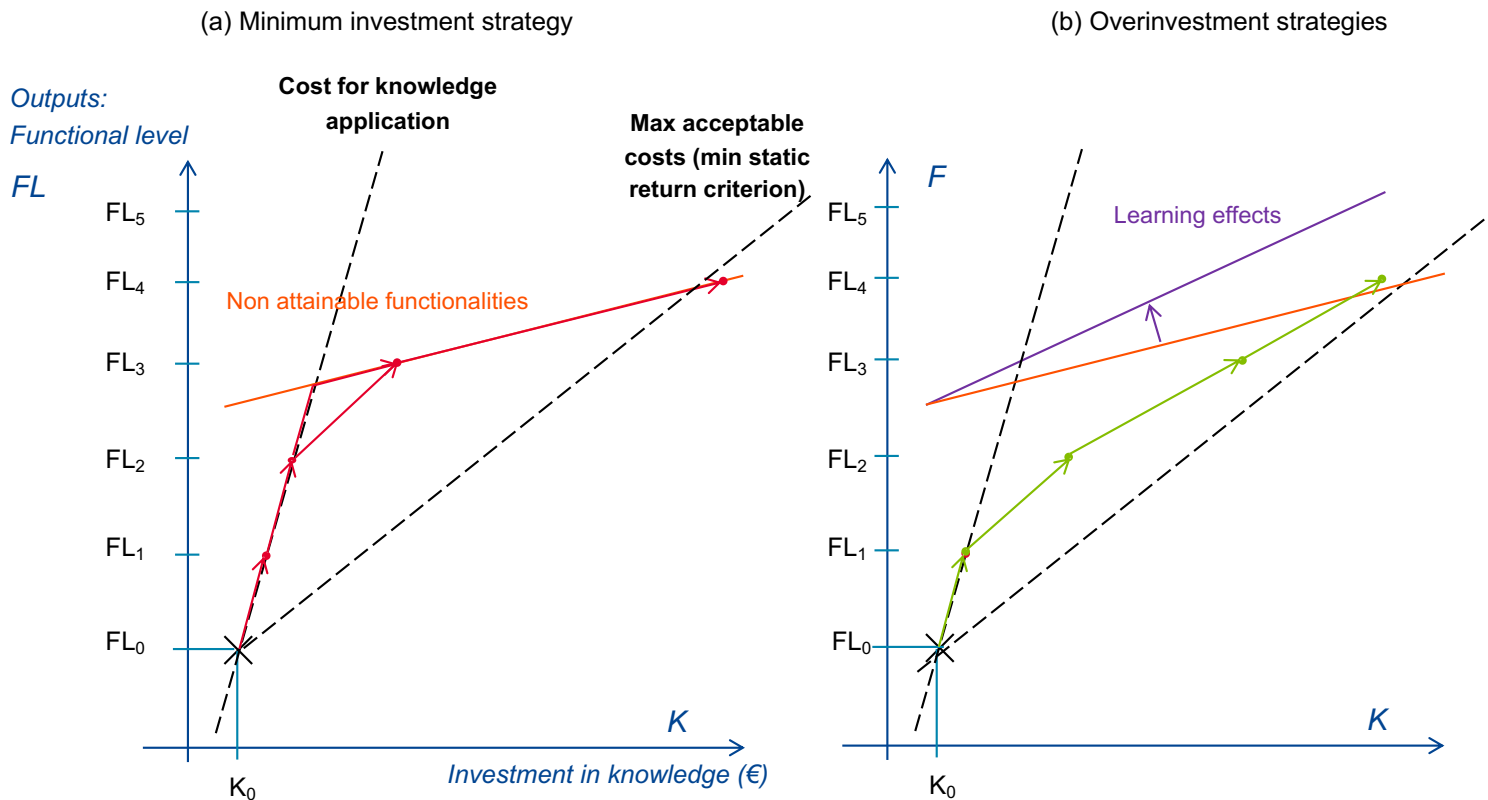


Figure 25. Simulation: comparison of different investment strategies

The main results of the simulation are the following:

(1) if the overinvestment is not rewarded by a shift of the technological frontier, profitability is either deteriorated (one invests more to reach the same functional level), or unchanged (this corresponds to a load smoothing scenario: one overinvests at an earlier step, in order to reduce a substantial investment at a subsequent step)

(2) the sole class of overinvestments that enhance profitability are those which shift the technological frontier. And the cases in which the overinvestment strategy is significantly more profitable are those within which the technological frontier is very horizontal with respect to the feasibility constraint, i.e. the case in which a large investment effort is required to achieve even a small functional increment. It is then interesting to over-invest in order to move the frontier and thus open up new technological possibilities. The zone of unreachable functionality can be seen as corresponding to an "unknown" zone: thus, these simulation results suggest that an overinvestment is economically interesting if it allows to explore the unknown, i.e. if it corresponds to a decision to explore the unknown.

This suggests that in rule-based design under functional expansion, overinvestment strategy related to the first deliverable (ensuring that the functional requirements of the individual product functions are effectively met) will not contribute to enhance economic performance. For the traditional rule-based design deliverable of rule-based design under functional expansion, the rule-based design performance logic still applies: exploiting at best existing knowledge bases, avoiding unnecessary costs. In contrast, anticipation-oriented

overinvestment strategies could be interesting for the second deliverable, that is the renewal of architectural independencies in the unknown...

5.1.4. Conclusion: Functional expansion – an ‘unknown with impact’ which disturbs robustness in rule-based design and calls for transformations at the level of rule-based design organizations

In summary, our results reveal that the unknown introduced by functional expansion plays a non-negligible role in rule-based design. It is an explanatory variable for phenomena that appear impossible, incomprehensible in pure rule-based design. Namely, it offers a new explanation regarding why commercial aircraft development costs do not move down a learning curve. It also offers an explanation for recent rule-based design crises.

The explanatory mechanism is the following: the continuous generativity that functional expansion introduces in rule-based design destabilizes the practices, the rules, the principles... which traditionally ensure robustness in pure-rule-based design. Under functional expansion, applying Suh’s (2005) information and independence axioms will not be sufficient to ensure a robust design. Managing the convergence of product development projects toward cost, quality and time objectives is insufficient to ensure reliable project deliverables: one can note that the products in cause in the recent design crises (Dieselgate, Boeing 737, Samsung Galaxy Note 7...) were, for a time, before the design flaws were discovered, evaluated as performing in terms of cost, quality and development lead time<sup>35</sup>.

Among what could restore robustness under functional expansion, we have identified (1) a capability that seems key to keep the economic impacts of functional expansion under control (continuously renewing architectural independencies in an expanding functional space), and (2) an investment strategy that could help enhance economic trajectories with time (overinvesting in anticipative learning for the design of architectural independencies in the unknown). But we have not discussed the kind of organizational structure that would be required for the exercise of a continuous remodularization capability and / or the deployment of overinvestment strategies in the unknown. Nor have we talked about the organizational transformations which this would entail within the current organizations of an R&D firm. It is already possible to say that deploying a continuous remodularization capability and / or overinvestment strategies in the unknown would imply non negligible transformations in D(evelopment) organizations (engineering department, product development organizations...) which traditionally rely on rigid and disciplined development logics (e.g. stage-gate processes to manage convergence to Cost, Quality Time targets), on resources optimization... Which actors could take the responsibility for the design of independencies in the unknown which will not immediately pay off? Which actors could take responsibility for overinvestments which deoptimize immediate economic returns? Etc. Such practices which consist in incorporating a small dose of flexibility within the rigorous, highly controlled product development logics of rule-based design, are unnatural from the viewpoint of pure rule-based design. So we will further discuss these questions in section 6.

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<sup>35</sup> They are not known for excessive cost growth with respect to their initial budget. They passed the tests which controlled their technical performance. Etc.

## 5.2. Evaluating the economic impact of the unknown in innovative design (RQ2)

### 5.2.1. In a design theory framework, rigorously managing the economic performance of projects that conventional economic calculation advises against launching is possible, and can be very profitable...

As mentioned earlier, in a decision theory framework, innovative design projects that contain unknown prove undecidable and unmanageable. Any possibility of management stops at the investment calculation stage, since investment calculation proves either impossible (lack of data) or advises against launching such risky project. If, in spite of the advice of decision theory-based profitability calculations, the project is launched, decision theory does not offer any approach to economically manage the execution of the project and its value. But if one leaves the decision theory framework, and moves to design theory, rigorous managerial approaches enabling to build value in the unknown can be considered (Hooze, 2020). Falling into the avenue opened by these works, our research works exhibited empirical evidence which show that in a design theory framework, rigorously managing the profitability of innovative design projects that conventional economic calculation advises against launching is possible:

- (1) In this design theory-based investment framework, quantifying the economic value generated by exploring the unknown is possible. In that, one can extend uncertainty-reduction-value calculation to the unknown.
- (2) In addition, in this framework, an indispensable condition for effectively generating profits by exploring the unknown is the deployment of managerial tools that will be used to mobilized to select projects that contain unknown, and to closely monitor unknown exploration during project execution.

This calls for two remarks: in the design theory framework, profitability has a behavior in its own right, which differs from the behavior of profitability under (high) uncertainty. In this framework, luck is not the sole explanation for high gains in the unknown. Contrary to high gains under high uncertainty which can only be explained by luck, high gains in the unknown can be explained by a rigorous exploration of the unknown, based on the exercise of design-methods that enable to build new states of the world, which may bring new sources of profits.

But we can also highlight that acquiring the indispensable methods that enable to effectively ‘systematically’ generate a lot of profits in the unknown (e.g. profits of the same order as those of Airbus innovation cluster) is difficult. We develop this point in the next subsection.

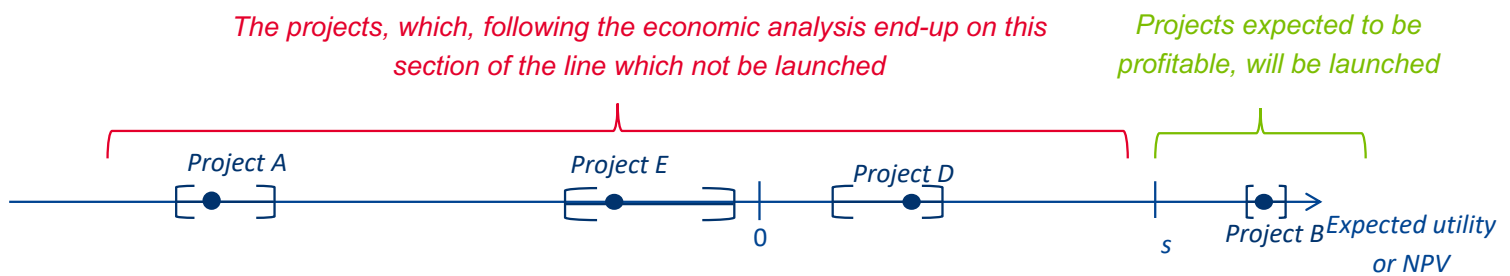
### 5.2.2. ... but systematically generating a lot of profits in the unknown requires to meet demanding and difficult conditions

Articulating (i) a capability to detect not necessarily obviously visible unknowns with impact, and (ii) a capability to explore these unknowns with design-based methods in order to design high profits in the unknown is possible, but difficult.

To develop this point, we can rapidly come back to the way one represents uncertainty in an investment decision under uncertainty only. Under uncertainty, one generally computes an economic indicator (e.g. a NPV), which can be affected by a variance representing uncertainty.

The comparison between the outcome of this economic calculation and a threshold generally guides investment decision-making. In other words, from a perspective that only takes uncertainty into account, if the project is not affected by the unknown, the manager's objective is to situate it on a horizontal axis, with a level of confidence that depends on the degree of uncertainty affecting the project. (Figure 26 below)

**(a) Economic decision-making under uncertainty**



**Figure 26. Investment decision-making under uncertainty only**

If the project does not contain any unknowns, its positioning can be considered reliable on this axis. In contrast, if a project contains unknown, its position on the horizontal axis can be disturbed. To take this into account, we introduce a representation that involves a second axis, 'unknown potential', distinguishing between a positive unknown potential (corresponding to initially unknown  $\theta_i$  which, if they emerge (e.g. if designed), can have a positive economic impact) and a negative unknown potential (corresponding to initially unknown  $\theta_i$  which, if they emerge can have negative, undesirable economic impacts, and thus which call for being controlled or avoided) (Figure 27 below).

(b) Economic decision-making under uncertainty and in the unknown

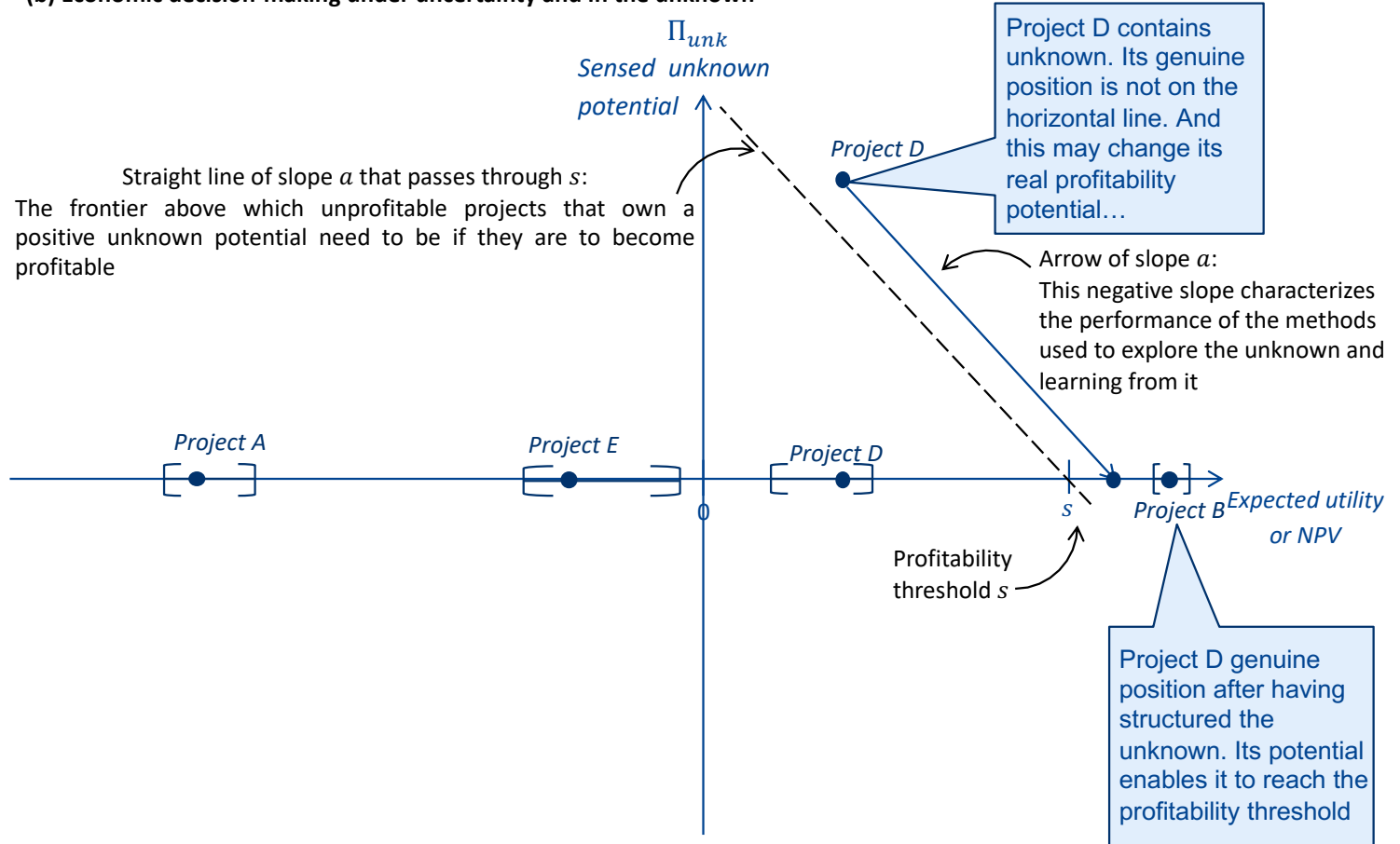


Figure 27. Introduction of a vertical axis to represent the unknown in investment decision-making

What happens to project D in Figure 25 is what has happened to the Flextrack project and to the 75% of the projects incubated or accelerated by Airbus innovation cluster portfolio: the project which was due to be unprofitable according to classical economic calculation becomes profitable and generates profits (in the frame of subsequent projects).

But the conditions for an initially unprofitable project to experience a trajectory similar to that of project D (i.e. become profitable) are demanding and difficult to meet.

In the scheme of Figure 24, moving from the unprofitability zone to the profitability zone depends on the combination of two things:

- the slope  $a$  of the vector between the initial position of project D, with the unknown being taken into account, and the genuine position of project D after unknown exploration. This slope can be seen as an indicator of the capability of the actors of explore and structure the unknown. The larger this slope (i.e. the closer it is to zero), the more performing the capability to explore the unknown
- the height of the initial position of project D: if the performance  $a$  of the unknown exploration methods is fixed, an unprofitable project needs to have an unknown potential which is high enough, if it is to become profitable. The necessary height is determined by the dashed straight line in Figure 25. Given  $a$ , the unprofitable projects need to be above this line if they are to become profitable after unknown exploration

Detecting a project the unknown potential of which is high enough, given one's capability to explore the unknown is not obvious. In Figure 26 below, we have delimited several areas. The area 5 is the area within which unprofitable projects need to be located if they are to become profitable after unknown exploration. But other profitability dynamics can occur in the unknown. The projects contained in area 4 (e.g. project E) have a positive potential of unknown, but which is insufficient to reach profitability, given  $a$ . We can also highlight that some projects might also be affected by a negative unknown potential: if one invests in the projects in areas 2 or 3, undesirable events may affect profitability negatively. Especially for the projects of area 2 (e.g. project B), which are the projects launched by traditional organizations, this might be a major problem: this can result in economic failures (less-than-expected profits), which may seem incomprehensible, as long as one is unaware of the unknown.

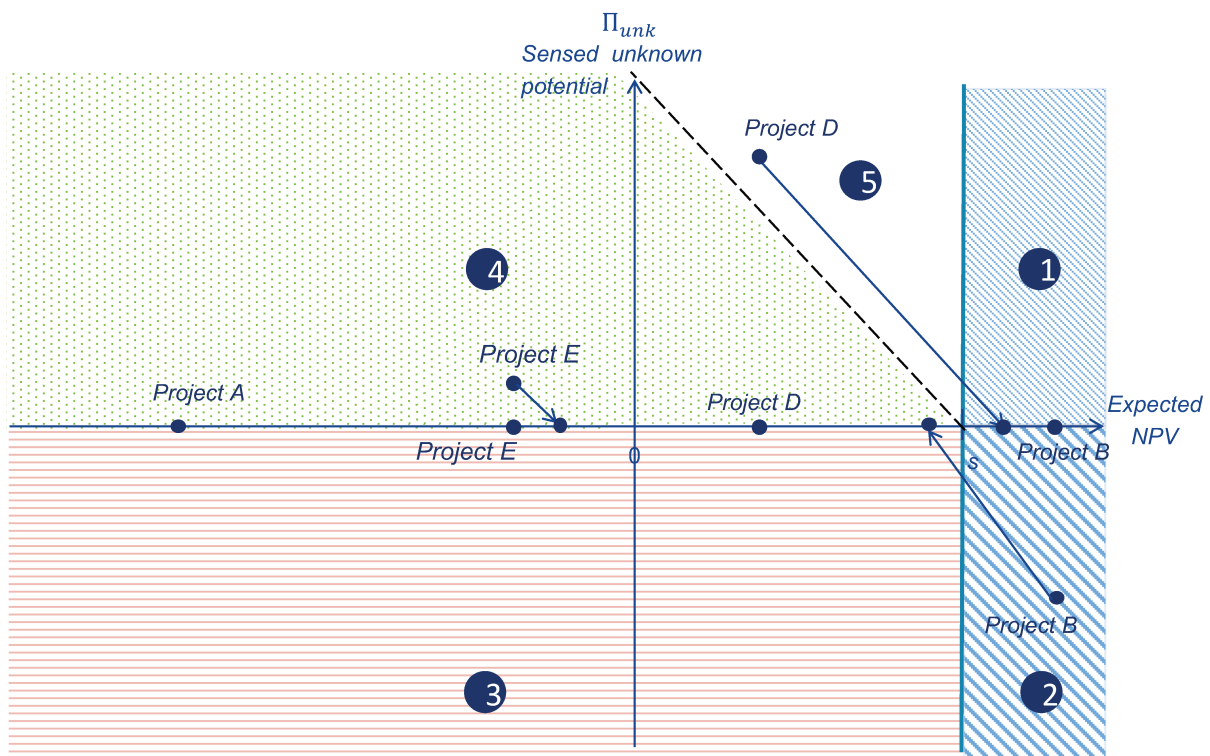


Figure 28. Demanding 'conditions' for turning initially unprofitable projects into profitable projects in the unknown

Our studies of the Flextrack project and Airbus innovation cluster's project portfolio also suggest that it is possible to intentionally select projects that are in area 5. In particular, it seems that with its criteria, Airbus innovation clusters has deployed a way to systematically select projects that are in the 'area 5' associated with the performance of its design methods. But Figure 26 also emphasizes that the conditions under which an initially unprofitable project can become profitable after unknown exploration are quite restrictive, and difficult to meet.

### 5.2.3. Conclusion: about the quantifiability and manageability of the profitability in the unknown

In summary, with two ‘exemplary cases’, our research works have highlighted that in innovative design, the value of information in the unknown is quantifiable, and that it can be rigorously managed and generate a lot of profits, provided one owns appropriate design methods. In subsection 5.2.2. we have highlighted that the conditions which are required to generate high profitability in the unknown are demanding and difficult to reach.

One can add that we have demonstrated the economic ‘quantifiability’ and the ‘manageability’ of the unknown in cases (Flextrack and Airbus innovation cluster) which have a specificity. Airbus innovation clusters explicitly claims to address projects which all have the same profile and face the same kind of issues: projects perceived as highly risky and uncertain, non-consensual projects<sup>36</sup>, aligned with Airbus strategy and roadmap, mobilizing the existing Airbus knowledge related to the topic, involving prototyping plans, etc., and rejected by the traditional organizations. In other words, Airbus innovation cluster achieves a generative exploration of the unknown which generates a lot of profits under ‘restricted’ conditions: it only addresses a very specific class of problem, that is the fact that promising innovation projects are rejected by traditional organizations, because traditional economic calculation predicts non profitability<sup>37</sup>.

These restricted conditions are a limitation of our findings. We will further discuss this in Section 6.3 which is devoted to what can be inferred from these findings for the organization of a contemporary Innovation Department.

## 6. Discussion

### 6.1. Nature of the knowledge produced by our intervention

Within two design theory-based theoretical frameworks (functional expansion ; design theory based-decision model) which endogenize the unknown but do not quantify its impact, this research work has developed computational techniques (referred to as ‘instruments’ in this document) which, from a formal viewpoint, allow to isolate and evaluate the economic impact of the unknown (i.e. of new, initially unknown  $\theta_i$  with respect to the definition of the unknown given in section 1.2).

The development of these techniques has been motivated by the objective of investigating whether the unknown played a role as a ‘hidden’ variable in the framework of rule-based design and innovative design phenomena identified in the commercial aircraft industry, and which appeared anomalous when analyzed with traditional decision theory-based economic models.

Our economic evaluation instruments which were designed specifically for the analysis of these anomalous phenomena highlighted that (1) the economic impact of the unknown is

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<sup>36</sup> In the final steps of the selection, the innovation leader in charge of the portfolio systematically favors non-consensual projects, i.e. projects the pitch of which has both been given very good and very bad marks by the members of the selection board

<sup>37</sup> The Flextrack project addressed a problem of the same class

quantifiable, and (2) these economic impacts can be large: high economic impacts generated by hidden unknown (on the one hand high costs; on the other hand, huge returns, provided appropriate design methods suitable to efficiently explore the unknown are deployed). Once the (huge) economic impact of the hidden unknown is revealed, our phenomena seem much less intriguing.

- A second degree polynomial increase for the development costs of commercial aircraft no longer seems unexplainable when one knows that commercial aircraft are affected by functional expansion, and that functional expansion can lead to exponential cost dynamics.
- The transformation of a project that was due to be unprofitable into a highly profitable project seems much less intriguing when one knows that investors in the unknown, if they are equipped with adapted design methods to detect and explore the unknown, can redesign their decisional space by designing an economic target and by designing the new decisions or the states of the world that will allow to reach the target.

In other words, by proposing economic calculation instruments that endogenize the unknown, this research work has produced new interpretation frameworks for phenomena that seemed incomprehensible.

The fact that the economic impact of 'hidden unknowns' can be large suggests that there is a strong contemporary need for economic computational and managerial means detecting and managing 'hidden unknowns with impact'. This emphasizes the importance of further investigating how the economic evaluation of design in the unknown could be tooled more broadly and systematically in order to manage hidden unknowns, and in particular avoid their detrimental effects and exploit their promising economic potential. And this emphasizes the importance of further investigating how these results call for organizational transformations.

During our intervention, the interaction between our evaluation instruments and Airbus organizations was restricted to an application to analyze past projects and to presentations in meetings, namely in steering committee – no attempt was made to implement the tool. However, this form of interaction generates some impacts in Airbus organizations (which our papers do not discuss).

Between hidden unknowns and their impact highlighted by our instruments, there are some activities / actions which were hidden as long as the impact of the associated unknown had not been quantified: either hidden activities in the sense that these activities are effectively carried out, but in an unclaimed, invisible way (involving invisible relationships between actors) ; or hidden activities in the sense that the activities are not carried out and should be – which calls for building new relationships between actors. So our research offers new representations for collective action, and it calls for transformations at the level of collective action. This corresponds to the principle of inseparability between knowledge and relationships (Hatchuel, 2001) according to which modifications at the level of knowledge necessarily generates modifications at the level of the relationships between the actors of an organization. In particular, the importance of a continuous renewal of architectural interdependencies under functional expansion (Paper 1) suggests a need for organizational relationships dedicated to decoupling tasks. These relationships and the associated activities are thus far hidden. Some relationships may exist, with the associated activities effectively carried out, but in an unclaimed, invisible way. Others may be neglected, ignored, and the



dependency that has not been decoupled may lead to costly design issues (propagation effects). The perspective of characterizing and ‘generating’ these relationships (where they are needed, how to tool them, etc.) in the framework of a future research work with Airbus has been mentioned.

In terms of impact, the results of Paper 1 aroused interest and numerous discussions in Airbus, namely regarding the extent to which a slowdown of the cost escalation curves experienced by commercial aircraft manufacturers is feasible and realistic, regarding the costing of integration task, regarding the possible strategies to master development costs, etc. Following the last steering committee of this thesis work, in October 2019, where the department in charge of Airbus Product Development Plan, the Costing department and the R&T department were represented, we were asked to present the results to several teams in charge of activities or projects where functional expansion could be influential. The point was to raise awareness about functional expansion (i.e. diffuse the new vision offered by models which have made hidden unknowns visible), namely within the Costing community and within teams working on the product development logic (current and that ‘of the future’). Moreover, the results regarding economic calculation in rule-based design and in innovative design were both presented to the stakeholders of the innovation cluster’s project portfolio analyzed in Paper 3. For the presentation, we were asked to insist on the role played by the exploration methods and the close managerial control / close monitoring in the generation of the huge economic returns.

In what follows, we further discuss the limitations of our results and their organizational impacts.

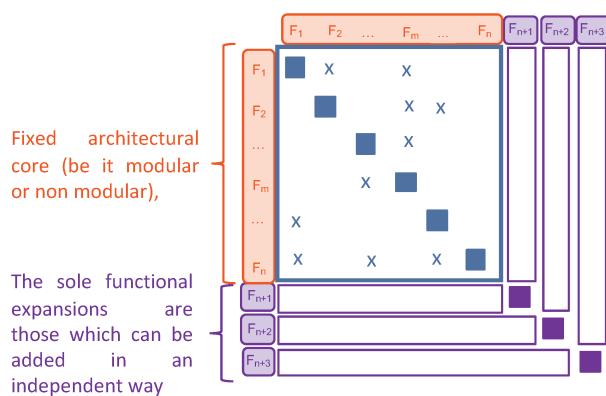
## 6.2. Limitations: Rule-based design under functional expansion: which organizational impacts for rule-based design organizations (engineering departments and in product development organizations)?

As mentioned in Section 5.1, the unknown introduced by functional expansion disturbs the traditional ways to ensure robustness in rule-based design. Our investigations have led us to identify two strategies that could contribute to robustness in rule-based design under functional expansion: (1) continuously renewing architectural independencies, and (2) overinvesting in anticipative learning in the unknown. From a pure rule-based design viewpoint oriented toward immediate profit optimization, minimization of knowledge production, reuse of a stable set of design rules, etc. such strategies are very unnatural and are likely to require substantial organizational transformations. This leads to ask the question: wouldn’t there exist alternative strategies which could help efficiently manage functional expansion in rule-based design, without requiring major organizational changes in engineering departments and in product development organizations? Two such strategies can actually be identified: we discuss them in subsections 6.2.1 and 6.2.2, and we explain that they are little convincing to help efficiently deal with functional expansion in rule-based design.

### 6.2.1. Strict control of the flow of new functions: restricting functional expansion to modular functional expansions

There exists a strategy which we have thus far not discussed in this document, which would require minor (even no) changes at the level of product development organizations, and which could offer resilience in the face of functional expansion: this is a strategy which would allow the integration of a new function within an already-existing architecture only if this new function can be introduced in a modular, independent way. Under this condition, the unknown introduced by functional expansion would be confined within the new function. The strength of this model is that thanks to independence, there would be no propagation-induced unknown, so no impact on architectural design rules. The robustness of rule-based design would not be disturbed, Suh's axioms could apply as when there is no functional expansion at all. Figure 29 below specifies in more details the characteristics of this model.

At first sight, this condition would make sense from the viewpoint of Development organizations – this would correspond to a sequential model where innovation activities develop new functions in a first time. And in a second time, development organizations decide on whether or not they accept to integrate the innovative new functions, based on a criterion of 'functional independence'.



<b>Nature of the new functions</b>	New peripheral modules, which are not integrated with the core. They are architecturally independent from the core thanks to design standards (parallel with APIs in the case of software development)
<b>Architectural design work, location of the innovation effort</b>	The architectural core itself is stable (rule-based design). The innovation effort is concentrated on the boundaries of the architectural core (on their renewal when needed).
<b>Value associated with product changes</b>	Value of the newly added functions
<b>Unknown issues</b>	The unknown-related issues are mastered. The unknown is confined to the newly added functions. The issue of functional-expansion-driven propagated changes is bypassed.

One can make a parallel with a real-world model, that is the model such that some entities of the architectural cores are 'open', i.e. allow the plug of peripheral modules (e.g. add-on applications, common in software development...) (Brunswick et Schecter, 2019 ; Yoo et al., 2010)

Figure 29. Allowing modular functional expansions only

Although this model may seem attractive because it represents little organizational changes, and although it is close to some existing real-world architecture management models, especially 'open platform' models in the software industry, it is not convincing to manage complex rule-based design products under functional expansion (such as commercial aircraft) for at least two reasons.

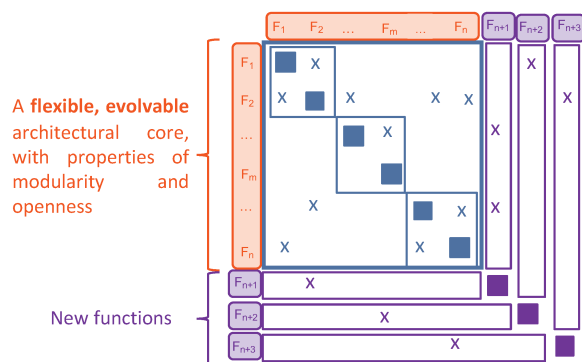
Firstly, a phenomenon of functional expansion solely involving the emergence 'peripheral' functions is not representative of the phenomenon of functional expansion experienced by rule-based design products: for example, in the case of commercial aircraft, many of the newly added functions are integrated within the core architecture. They are strongly related to and integrated with core functions of the product: some crucial functional expansions are even safety-related – they interact with and enrich, or complement the primary functions of the product (e.g. doors functions, flight systems innovations, etc.). They are by no means only peripheral modules. Secondly, rejecting functions that would need to be integrated within

the core architecture of the product may reduce the attractiveness of the new product to consumers who expect new functions (although it is also true that the results of our first paper suggest that the value of adding a new function should be carefully studied, since its impact in the already existing architecture is not anecdotal).

### 6.2.2. Exploiting the flexibility of a given modular architectural core

In Paper 1, with a theoretical simulation, we have found that a modular architectural core is not robust to the propagation phenomena induced by a continuous emergence of new functions. In this subsection, we quickly come back to the model of the ‘modular architectural core’, in order to better highlight what distinguishes it from the model which our research works suggest for the management of rule-based design under functional expansion.

As explained in more details in Figure 28 below, the model of the ‘modular architectural core’ facilitates the modification of entities within existing modules. It allows to manage the uncertainty associated with the modification of these very entities (MacCormack et al., 2001 ; Schapiro and Henry, 2006). Eventually, it can be resilient in the face of an occasional emergence of a new function. But it does not manage the architectural impact of these new functions<sup>38</sup> as an issue in its own right. It neglects the unknown. And according to our simulation (Simulation 2, Paper 1), if the emergence of new functions is continuous, such a model which neglects the unknown is not robust: cost can increase dramatically.



<b>Nature of the new functions</b>	New peripheral modules or New functions integrated with the architectural core
<b>Architectural design work, location of the innovation effort</b>	Innovation work: <ul style="list-style-type: none"> <li>- The boundaries of the architectural core, so that new functions can be plugged</li> <li>- The evolutions of the architectural core due to the emergence of new market information or technological information (either during the course of one development project, or between two product generations). (market / technological uncertainty driven evolutions)</li> <li>- <i>+ The evolutions of the architectural core due to propagated changes initiated by the newly added functions. (unknown-driven evolutions)</i></li> </ul>
<b>Value associated with product changes</b>	<ul style="list-style-type: none"> <li>- Value of the newly added functions</li> <li>- Value offered by the flexibility of the architectural core, that enables to evolve the core following the emergence of new market or technological information: this is the value of the reduction of market and technological uncertainty (equivalent to the value of a real option), achievable thanks to the modularity of the core (Baldwin and Clark, 2006)</li> <li>- <i>+ Value of unknown driven evolutions: uncovered by this model</i></li> </ul>
<b>Unknown issues</b>	Some evolutions of the architectural core are driven by the unknown, but these latter are ignored by this model

- Without what is written in italic in the table, this model corresponds to flexible (or agile) development models (e.g. MacCormack et al., 2001 ; Schapiro and Henry, 2012): such a model is particular adapted to reduce market and / or technology related uncertainty, in unstable environments.
- If the emergence of new functions is rare, this model will probably still be appropriate (the unknown can be neglected)
- However, the theoretical results of our first paper suggest that if the emergence of new functions is frequent, continuous, this model will become useless to master the evolutions of the architecture – the propagations within the architectural core will become out of control

<sup>38</sup> (which is an unknown-related issue)

Figure 30. The model of the ‘modular architectural core’

Since incorporating modularity and flexibility in a given architectural core does not allow to efficiently manage functional expansion, we investigated another approach, which according to our simulation (Paper 1, Simulation 2b) could help master functional expansion: incorporating flexibility in a changing, continuously extending core, with a continuous re-modularization approach.

### 6.2.3. Exploiting the flexibility of a continuously changing architectural core: the continuous re-modularization approach and its organizational implications for the distribution and coordination of design work

Figure 31 below illustrates the architectural strategy which we have simulated in Paper 1 (Simulation 2c) and which seems promising to manage rule-based design under functional expansion.

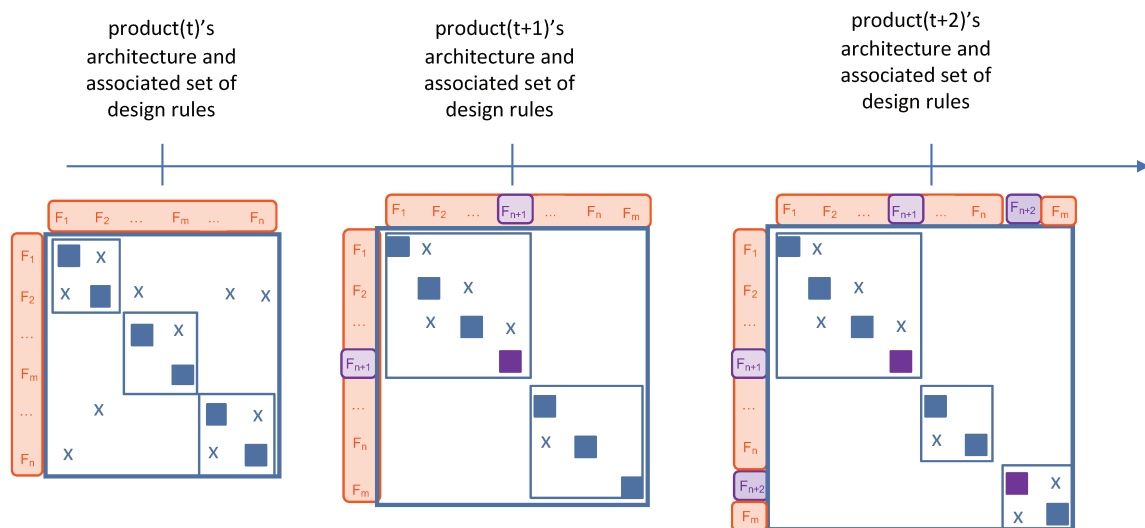
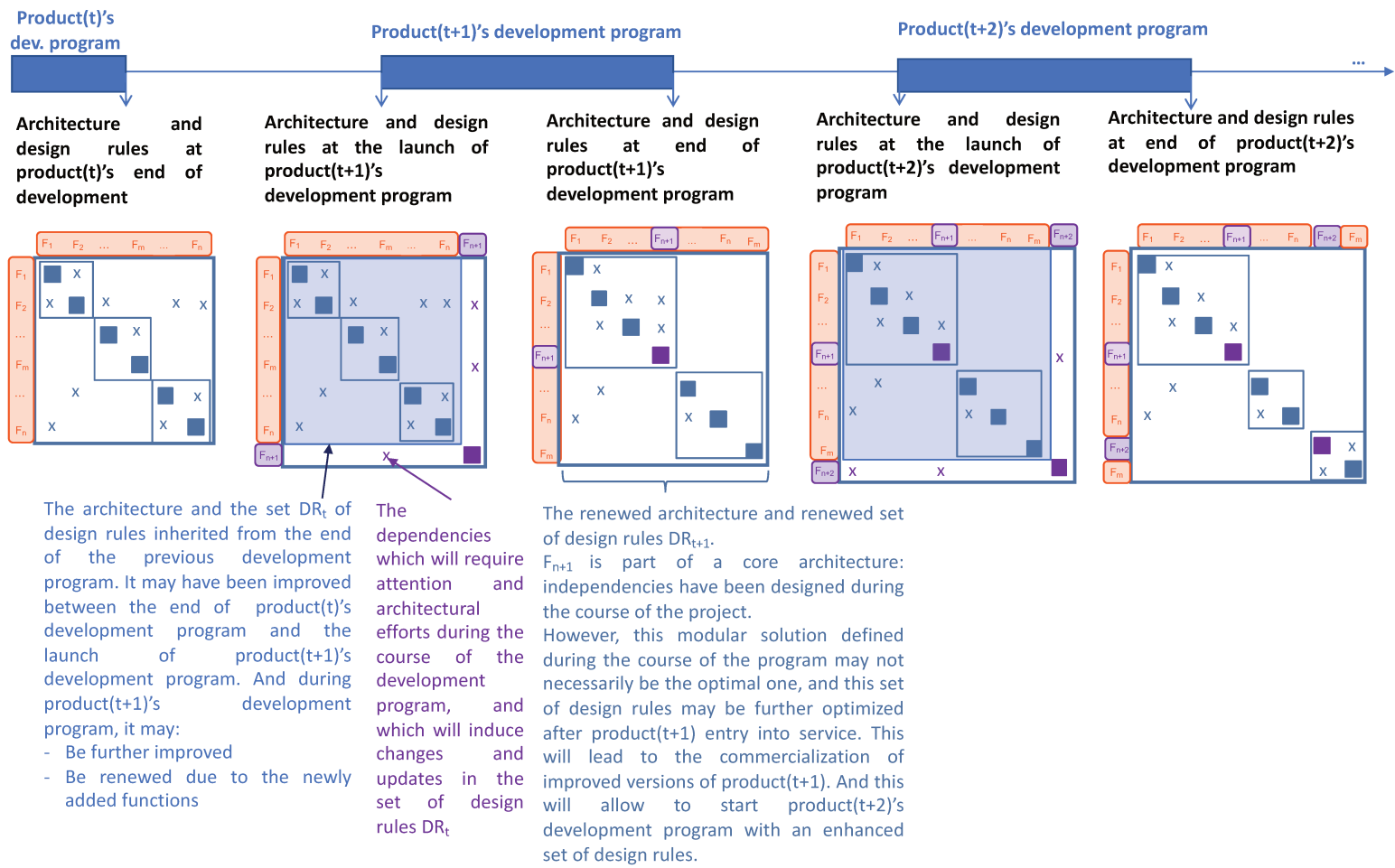


Figure 31. Simulated continuous re-modularization strategy

However, it is important to note that Figure 31 represents a very simplified design rules renewal model: indeed, the fully modularized expanded architecture represented for product(t+i)'s development program will not necessarily be known at the outset of product(t+i)'s development program.

Figure 30 below represents a more realistic continuous re-modularization model which takes into account the fact that the renewal of the architectural rules may start being anticipated before the launch of product(t+i)'s development program, but may also take place during product(t+i)'s development program.



**Figure 32. More realistic continuous re-modularization model**

In Figure 32, the whole architecture is restructured on a continuous basis, so the distinction between the architectural core and the new functions is no longer relevant. This can be seen as a continuous sequence of Henderson and Clark's (1990) 'architectural innovations'. Although architectural innovations are generally considered as occurring on a punctual basis, previous works have already emphasized that architectural obsolescence and renewal require continuous attention. Meyer and his colleagues (Meyer and Lehnerd 1997; Meyer et al. 1997; Meyer and Mugge 2001) stress that design engineers need to continuously exert preparatory architectural 'renewal' efforts, in order to be able to achieve platform renewal in a timely manner when market-pull or techno-push factors render obsolete the current product platform. Functional expansion can be seen as a source of architectural obsolescence that is even more intense than the market-pull or techno-push factors which render platforms obsolete: it continuously generates architectural obsolescence and continuously calls for renewing the architecture.

As explained in Section 4, continuously renewing architectural independences as potentially harmful couplings are discovered seems important in order to avoid a combinatorial explosion of propagation paths. But it raises significant organizational issues and questions (which this thesis has not addressed).

The renewal of architectural independencies may take place *during the course of the project* in order to ‘decouple’ entities which, if they remain coupled, could trigger undesirable phenomena, not necessarily for the product under development, but perhaps for the next development projects (indeed, the risk of undesirable phenomena appearing on the next developments is multiplied by the (accumulation of the) functional expansions emerging on the next generation products). So, the model represented in Figure 32 involves interactions between teams or actors during the course of the project, in order to ‘decouple’ or ‘prepare the decoupling’ of entities the interaction of which may have undesirable effects. This corresponds to a dynamic relationship between the emergence of knowledge (here, knowledge regarding the identification of potentially detrimental couplings), product architecture and organization which has already been underlined by previous research works (Lakemond and Magnusson, 2017). This also suggests that under functional expansion, forms of collaborative decision-making and associated models and tools (Jankovic et al., 2010) may be required beyond the early phases of a product development project. Similarly interaction management models (e.g. Stal-Le Cardinal and Marle, 2006) which are generally supposed to structure projects during their early phases, by defining the interactions within Product Breakdown Structures, Work Breakdown Structures may, in a context of functional expansion, be needed (under adapted forms) in later phases, and this regularly.

One can note that the continuous re-modularization model and its dynamic set of design rules represent significant difficulties for the coordination of design work between different project teams, since the design rules may change between the descent and ascent of the V-cycle. This means that if the actors of the design do not coordinate and communicate on the updates of the design rules, product sub-assemblies (systems, subsystems, components...) may be developed with different the design rules, leading to incompatible sub-assemblies and integration problems during the later phases of the development process. This suggests that functional expansion in rule-based design involves a kind of innovation which does not lend itself well to collaborations with external partners, unless appropriate coordination means are available. This leads to questions regarding what appropriate coordination tools (architecture modelling tools, digital modelling tools, decision-making tools ...) under functional expansion could be, in order to manage continuously evolving design rules. This also leads to ask which actors, which managerial figures, which decision-maker figures, could take on the role of coordinating different design teams who work based on unstable sets of design rules. This discussion suggests that under functional expansion, opting for a tiered outsourcing strategy and delegating the integration of sub-assemblies to Tier-1 suppliers may be very risky – a close sharing of the design rules changes will be required between the final integrator which outsources and its Tier-1 suppliers, in order to avoid integration issues. In the case of Boeing’s B787 program which (for the first time) involved a tiered outsourcing strategy<sup>39</sup> (Denning, 2013), the dramatic integration issues (which caused major delays and overcosts, which led Boeing to buy one of the Tier-1 suppliers) are namely attributed to a lack of know-how of the Tier-1 suppliers, to a web-based communication tool (Exostar) which, reportedly, did not allow a satisfying collaboration between the design teams, etc. But one can ask whether an instable basis of design rules due to functional expansion would not be an

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<sup>39</sup> [http://comlabgames.com/45-971/instructor/02\\_outsourcing/boeing/What%20Went%20Wrong%20At%20Boeing.pdf](http://comlabgames.com/45-971/instructor/02_outsourcing/boeing/What%20Went%20Wrong%20At%20Boeing.pdf)

additional explanation to the problems experienced by the suppliers of the Boeing 787 program.

In conclusion, this discussion has further lengthened the list of the potential large-scale and undesirable effects of ignored / denied functional expansion in rule-based design. And it has underlined the organizational challenges raised by the ‘unknown with impact’ which functional expansion is. (Re)gaining control over functional expansion seems of utmost importance – and the solutions to do so seem to require a genuine rationalization of Development organizations: new investment logics, new coordination tools, new architectural activities and architectural roles, new decision-making activities, etc., which would also have an impact at the level of engineering education.

### 6.3. Limitations of the findings regarding ‘economic management in the unknown’

#### 6.3.1. The unknown, a manageable economic variable according to our results on ‘economic evaluations in innovative design’: but which organizational suggestions for contemporary Innovation Departments?

Our research dedicated to economic calculation in the unknown in innovative design aimed at studying potential tools which could provide an economic performance management approach for projects declared as unprofitable, and hence unmanageable by traditional decision theory-based economic calculation techniques. This led us to primarily focus on the managerial approaches and on the methods used within Airbus innovation cluster and in the case of the Flextrack project.

In this subsection, beyond the methods and managerial approaches, we briefly come back to Airbus innovation cluster and to the Flextrack project with a focus on their organizational setting.

One first remark is that neither Airbus innovation cluster nor the Flextrack project falls into a setting of organizational ambidexterity (in the sense of March (1991)). The innovation cluster’s project portfolio is strongly related with Airbus traditional organizations: it has deployed a network of local innovation correspondents within the competences centres, it selects ideas that come from the traditional organizations (engineering, manufacturing, customer services...), the heads of the competence centers are members of the project selection board, and members of the projects steering committees. The innovation leader in charge of the portfolio ensures that the incubated / accelerated projects can as much as possible access all Airbus-existing knowledge which could contribute to the project. As for the Flextrack project, it took place within a very traditional organization (the plant). At the first sight, the project was a very common manufacturing plant problem which involved no innovative design and no unknown. Throughout its duration, it was managed by the operational objectives of the plant (recurring costs, ramp-up issues, etc.).

In this regard, our results are consistent with previous research work (Le Glatin, 2018) which emphasize the important of conditioning the objectives exploration with the knowledge of exploitation.



All these elements (the position of the Flextrack project, the relationship of the incubation / acceleration portfolio with the traditional organizations) stress that the knowledge heritage of the traditional organizations is by no means a hindrance for carrying out economic reasonings in the unknown that generate a lot of profitability. Quite the contrary, in both cases, the initial knowledge base, and the access to the corporate knowledge plays an important role in the performance of the exploration of the unknown.

In order to better understand the organizational forms associated with these innovative design regime which generates a lot of profitability, we could have tried to get a better understanding of the role of the innovation cluster's Innovation Leader: as mentioned earlier, he closely monitors the progress of the projects. Doing so, his aim is to follow (and ensure) value building. His feedback probably plays a role in triggering the exploration of a space of concepts which departs from the initial, unprofitable concept. We could have further investigated whether his role is close to that of a creative leader (Ezzat et al., 2017).

Finally, as mentioned in Paper 3, prototyping activities have a major place in Airbus innovation cluster. A project needs to propose prototyping tracks if it is to enter the project portfolio. The innovation leader in charge of the portfolio studies very closely the prototyping activities of the incubated / accelerated projects: he has built about twenty indicators to characterize the prototype(s) built by the team during the course of the project – and based on these prototypes, he infers a 'prototyping performance' of the project. During our research, we noted that prototypes which turned out to invalidate the explored concept can be marked as 'performing' by the innovation leader – we could have further investigated these aspects.

#### 6.3.2. Restricted deployment situations for the identified economic evaluation and management techniques in the unknown?

Our research work in 'innovative design' revealed that the value associated with unknown exploration can be assessed and can be large, provided it is managed with appropriate design-based methods – however, we demonstrated this result for projects which meet very specific conditions, in the framework of the Flextrack projects and the projects of Airbus innovation cluster. In all these cases, the economic issue is similar and clear: it is about solving a seemingly impossible economic equation in the framework of a problem which has been provided by a traditional organization. The projects that enter Airbus innovation cluster's portfolio are aligned with Airbus strategy and roadmaps. And they are to be solved in a short-medium term, with objectives of 'disruptive business applications'. And Airbus innovation cluster does not initiate new projects.

For these reasons, we are in the face of an innovative design regime, with a slightly 'inferior' level of generativity. This leads to ask the extent to which the model identified in these specific Airbus cases could be extended for enhanced generativity, while keeping the same kind of profitability regime and the same kind of economic calculation in the unknown. In particular, this leads to ask whether the calculation of the value of the unknown and the associated management logic which work in the abovementioned specific cases would be applicable for innovative design activities in a less constrained way. Could the value of the unknown be computed with this very calculation in the case of innovative design activities which aim at making innovation fields emerge? Could it apply for more prospective innovative



design activities? Could it apply when it comes to guide decision-making for major themes (e.g. energy transition)?

The other way round, another limitation of our research is that it did not go as far as identifying how functional expansion-induced unknown with impact could make the object of an economic management. But we could ask whether the economic evaluation and management techniques identified in the Flextrack case and in Airbus innovation cluster could be deployable to help manage the unknown in rule-based design. This is what we discuss in the next subsection.

#### 6.4. Conclusion and opening

##### 6.4.1. Summaries of (R1) A rule-based design regime under functional expansion and (R2) A 'highly framed' innovative design regime

This thesis work dedicated to economic calculation in the unknown has studied two phenomena coming from the commercial aircraft industry.

The first phenomenon is the development cost escalation phenomenon affecting the commercial aircraft industry, which is incomprehensible from in the performance analysis framework of rule-based design (unless engineering departments are very under-performing). In the face of this phenomenon, we built an instrument that measures the economic impact of functional-expansion-induced unknown. And we used the instrument as an analytical tool in an attempt to get a better understanding cost escalation dynamics in rule-based design. This revealed that the unknown could be an explanatory variable for increasing development costs in rule based design. And it enabled to investigate and study the approaches which could be considered and prescribed to manage this unknown and its large-scale impacts.

The findings and the above discussions provide us elements to start characterizing, in terms of reasoning, performance logics and organizational forms (Le Masson et al., 2017) a rule-based design regime under functional expansion. This regime (R1) is a rule-based design regime which is deeply destabilized by a slightly superior, continuous and non-negligible dose of generativity. It is summarized in Table 4 below.

The second phenomenon is related to puzzling, seemingly deviant economic calculation practices, which, contrary to the predictions of traditional economic calculation techniques, turn out to generate huge returns. Mobilizing design-theory models, we built an instrument which detects whether some unknown has been structured during the course of a project, and isolates and quantifies the value of unknown exploration. From this instrument, one could derive a tool and identify associated management logics enabling to design the economic value of projects which, according to traditional economic calculation techniques should not be launched and hence unmanageable.

The findings and discussions associated with unknown detection and assessment techniques provide us elements to characterize a new design regime: a highly-framed but highly profitable innovative design regime (R2), equivalent to an innovative design regime with a slightly inferior level of generativity. It is summarized in Table 4 below.

As indicated in Table 4. below, R1 and R2 include both elements of rule-based design (in particular discipline-oriented elements) and elements of innovative design (in particular generativity-oriented elements).

**Table 4. toward the characterization of R1. A rule-based design regime under functional expansion and R2. A ‘highly framed’ innovative design regime**

	Outcome of the work dedicated to economic calculation in the unknown, in rule-based design	Outcome of the work dedicated to economic calculation in the unknown, in innovative design
	<b>R1</b> Rule-based design under functional expansion = rule-based design regime with a slightly superior, continuous and non negligible dose of generativity	<b>R2</b> Highly framed innovative design regime in place in the frame of Airbus innovation cluster = an innovative design regime with a slightly inferior level of generativity
<b>Design reasoning</b>	Within a stable dominant design architecture which is only ‘apparent’. The seemingly stable, already-existing architecture may be subject to genuine shocks triggered by the newly added functions  → Engineering departments’ twofold deliverable: 1. Ensuring that the functional requirements of the individual product functions are effectively met  2. Renewing architectural independencies, especially architectural independencies in the unknown	A search for business-application-oriented disruptive solutions to address problems experienced by the traditional organizations Product identity is not necessarily to be destabilized  Deliverables: New knowledge and concepts, not to initiate new projects within new innovation fields, but to provide successful operational solution which will meet the performance objectives of the traditional organizations
<b>Logics of performance</b>	Regarding the first deliverable: Optimizing resources: achieving the improvements at product level while minimizing knowledge production (the production of excess knowledge)  Regarding the second deliverable: producing excess knowledge (i.e. overinvesting) may be economically interesting to renew architectural independencies the unknown.	(Over)Investment in projects which are due to be unprofitable and should not be launched according to traditional economic calculation  The project objective is to meet the operational objective of the traditional organization, more than destabilizing product identify
<b>Management of economic performance</b>	For the first deliverable: Management by objectives, the performance objectives being: - Cost quality lead time - Static returns: revenues / resources ratio  For the second deliverable: From management by objectives to value management (management of the value of the unknown) (Hooge, 2010 2020) → Which performance objectives and criteria to manage the economic performance of the renewal of architectural independencies?	Value needs to be designed, which requires appropriate managerial approaches (Hooge, 2010, 2020)  + The economic value associated with unknown exploration can be quantified and managed. It can be very large provided appropriate methods and capabilities to detect and explore the unknown (and to design profitability in the unknown). → This result has been demonstrated and is valid under ‘restricted conditions’
<b>Forms of collective organization</b>	The forms of collective organization of pure rule-based design are inappropriate to manage the dynamic set of design rules. Keeping the organizational setting of rule-based design (e.g. cost cuts of the individual functions ; tiered outsourcing...) may entail catastrophic consequences. This emphasizes a need for new organizational settings, in particular for collaborative tools adapted to a context of ‘dynamic design rules’, to coordinate different design teams under functional expansion...	A very strict organization which relies on the knowledge heritage of the exploitation. A steering committee with a high level of sponsorship A place (e.g. Airbus Protospace), with prototyping resources, a training, a strong innovation leadership...

#### 6.4.2. How could a coupling between (R1) and (R2) help them address their respective challenges?

The discussion in the previous subsections clearly highlight that both R1 and R2 face issues.

Regarding R1: ill-controlled functional expansion turns out to have large scale, detrimental (socio)economic impacts. Thus, for product development organizations, it is crucial to (re)gain control over functional expansion. It is not impossible that spontaneously, without claiming it, some product development organizations have already started developing ways to manage functional expansion, in particular, to coordinate themselves in a context where design rules are instable<sup>40</sup>. But such implicit, unbudgeted ways of managing functional expansion could be vulnerable to cost cuts measured deployed by a management who ignores or denies functional expansion. And nothing guarantees that spontaneous ways of managing functional expansion will be sufficient if the pace of functional expansion accelerates. So there is a challenge of developing organizational forms which explicitly take on functional expansion. Our research works suggest that the nature of the organizational means required to master functional expansion substantially differs from the organizational forms in place in rule-based design: reinforcing existing rule-based design organizational forms in an attempt to master functional expansion is likely to have undesirable effects. In other words, the slight but non-negligible dose of generativity induced by functional expansion confronts traditional Development organizations to a crisis calling for a necessary rationalization step (new coordination logics, new investment logics, new decision-making logics, new tools, new roles...) of rule-based design.

Regarding R2: the strength of R2 is that it can potentially generate a lot of profits out of the unknown (more than uncertainty reduction), in a less risky way than if one engages in high uncertainty without unknown. However, the conditions under which such design regimes may take place are very demanding (difficult to reach) and restrictive. Does this mean that the occurrence of such a design regime, and the huge performance it may generate, are condemned to remain rare?

These issues related to R1 and R2 would call for future research. However, we can conclude on the following opening: to what extent could a coupling between R1 and R2 allow them to address their respective abovementioned issues? In particular, to what extent could the highly framed (but highly profitable) innovative design regime R2 help address the crisis faced by the rule based design regime under functional expansion? Indeed, the discipline (close monitoring...) and the high profits of R2 are perfectly compatible with a design regime which is mainly rule-based. R2 could bring methods, organizational ways, rigor to R1 in building independencies in the unknown and in continuously renewing design rules. This could restore the robustness of rule-based design, which is threatened in rule-based design under functional

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<sup>40</sup> According to the public data we collected, development costs and development times seem to have been experiencing a polynomial increase ( $x^2$ ) in the commercial aircraft industry (Figure 1b. and Figure 9). This could suggest that in the commercial aircraft industry, engineering departments already developed ways to manage the unknown introduced by functional expansion, but in an invisible, unclaimed way, without any dedicated organization, tools, budget, etc.

expansion (because of functional expansion). The other way round, such a coupling between R1 and R2 would offer R2 an area where it could express its generativity more intensely (initiating projects, operating in the unknown of products' architectural design space...).

Such a model is of course not evident to set up. It would probably be very demanding in terms of unknown management, especially in terms unknown exploration methods.

But this would not be the first time that innovative design feeds rule-based design in order to help it renew its design rules – quite the contrary, such a coupling has existed and played a critical role at the very origin of engineering departments (Le Masson and Weil, 2010). Could it be an opportunity for their survival in the face of functional expansion?

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## **8. Appendices**

Paper 1: Cost-effectiveness management under functional expansion: simulation of product development cost escalation curves and modularization strategies

Paper 2: Managing Learning Curves In The Unknown – From Learning By Doing To Learning By Designing

Paper 3: Why is investing in the unknown less risky and more profitable than investing under uncertainty? Model and empirical evidence

## Résumé

L'émergence régulière de nouvelles fonctions dans les produits est un phénomène largement connu qui peut être mesuré à l'aide de la notion d'expansion fonctionnelle. Cependant, la façon dont les bureaux d'étude exécutent les tâches de conception liées à l'expansion fonctionnelle, ainsi que les coûts associés à ces tâches et les outils qui permettent de les gérer sont mal connus. En particulier, alors que l'incrémentation des fonctions existantes est déjà reconnu comme un inducteur de coûts majeur, l'influence sur les coûts de développement des fonctions nouvelles n'a pas été quantifiée. Dans cette étude, nous avons simulé les tendances d'évolution des coûts de développement associées à la dynamique d'évolution des produits induite par l'expansion fonctionnelle. Tout d'abord, nous avons simulé les coûts d'une dynamique d'évolution produit induite par l'expansion fonctionnelle pure et nous avons constaté que les coûts de propagation induits par l'expansion fonctionnelle pouvaient être colossaux. Ensuite, nous avons simulé des stratégies de modularisation pour vérifier si les techniques de modularisation traditionnelles réduisent les coûts de propagation induits par l'expansion fonctionnelle. Cette simulation a révélé que les fonctions nouvellement ajoutées peuvent éliminer les indépendances architecturales critiques et rendre l'architecture modularisée obsolète, éliminant ainsi le potentiel de réduction des coûts de la modularisation. Nous avons ensuite simulé une stratégie de modularisation alternative qui est spécifique à l'expansion fonctionnelle et qui restaure la propriété de réduction des coûts de la modularité : une stratégie de re-modularisation continue. Enfin, nous avons simulé une dynamique d'évolution des produits impliquant incrémentation et expansion fonctionnelle afin de déterminer les conditions dans lesquelles les coûts induits par l'expansion fonctionnelle peuvent dominer les coûts induits par l'incrémentation de fonctions existantes, et vice versa. Ces résultats encouragent à discuter non seulement sur le potentiel de réduction des coûts mais aussi les risques de conception associés à une (mauvaise) utilisation de trois techniques de réduction des coûts dans le cadre d'une expansion fonctionnelle : réduction des coûts nominaux des fonctions, modularisation classique et modularisation continue.

### **Cost-effectiveness management under functional expansion: simulation of product development cost escalation curves and modularization strategies**

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## **Abstract**

The regular emergence of new functions in products is a well-known phenomenon that can be measured using the notion of functional expansion. However, we know little about how engineering departments execute functional-expansion-related design tasks, in particular, the development costs induced by functional expansion and the appropriate cost management techniques under functional expansion. The upgrade of already-existing functions has been found to be a significant cost factor. However, the influence of newly added functions on development costs has not been quantified. In this study, we simulated the development cost evolution trends associated with functional-expansion-driven product evolution dynamics. First, we simulated the costs of pure functional-expansion-driven product evolution dynamics and found that functional-expansion-induced propagation costs

might be huge. Second, we simulated modularization strategies to test whether traditional modularization techniques reduce functional-expansion-induced propagation costs. This simulation revealed that newly added functions may eliminate critical architectural independencies and render the modularized architecture obsolete, thereby undermining the cost-reduction power of modularization. Thus, we simulated an alternative modularization strategy that is specific to functional expansion and restores the cost-reduction property of modularity: a continuous re-modularization strategy. Finally, we simulated upgrade-and-functional-expansion-driven product evolution dynamics to determine the conditions under which functional-expansion-induced costs may dominate upgrade-induced cost, and vice versa. These findings encourage the discussion of not only cost-reduction potential but also the design risks associated with the (mis)use of three cost-reduction techniques under functional expansion: cutting functions' nominal costs, classic modularization, and continuous modularization.

Keywords: functional expansion; cost escalation; modularization; cost reduction; architectural independencies

## 1. Introduction

In a competitive context marked by the influence of, for example, market-based dynamics (e.g., customer demand for improved performance, higher quality, more customization, and lower prices), technology and innovation-based dynamics (e.g., new computer-aided design capabilities, new technologies, and new innovative materials), and regulatory-based dynamics, firms are strongly encouraged to enrich their products with innovative features. Design engineers develop and implement changes at the product level that can take the form of functional optimizations or of upgrades that improve existing product characteristics, or can take a more disruptive form, that is, an enrichment of the product's functional space with the addition of innovative characteristics. Innovative changes in the form of functional upgrades or newly added functions drive product evolution trajectories. Additionally, they may entail significant modifications and disruptions within product architectures. Hobday (1998) stated that even incremental changes (i.e., upgrades) implemented in a product whose architecture seems stable may hide disruptive changes at the level of subsystems or components. In recent research, the magnitude of the emergence of new functions with time in the case of consumption goods has been measured and found it to be very strong (El Qaoumi 2016; Le Masson et al. 2019). More specifically, the measures highlight consumption goods, some of which present a seemingly stabilized architecture, that prove to be continuously enriched with new functions. The continuous enrichment of products with new functions is called *functional expansion* (El Qaoumi 2016; Le Masson et al. 2019). An example of this phenomenon is the vacuum cleaner was enriched with as many new functions as the mobile phone, albeit over a time period that was three times longer.

If a product is under functional expansion, it can reasonably be assumed that functional expansion results in additional functional-expansion-related design tasks for engineering departments. But how much additional design work is induced by functional expansion? What are the associated costs?

Extensive literature is dedicated to the quantitative relationship between product functionality and product development effort (in particular product development costs).

In several industries, the costs related to development programs (i.e., development costs, unit production costs, and by implication, unit prices) are subject to increasing trends in the form of cost escalation (i.e., cost increase from one product generation to the subsequent generation) and cost growth (i.e., cost overrun with respect to the initially targeted costs, which generally undermine the ambitions that contemplate a slowdown of cost escalation curves with time). This concerns, for example, military aircraft, helicopters, ships, submarines, battle tanks, and commercial aircraft. For instance, Bongers and Torres (2013) found that the costs of U.S. jet fighter aircraft have increased by 12.6% per year since 1944. Particularly in the defense sector, this cost evolution gave rise to a large number of studies that aimed to improve the understanding of the driving forces that underlie cost increases and consider leverages that could slow down these cost increases (Augustine 1983; Eskew 2000; O'Neil 2011; Arena et al. 2008; Cancian 2010; Dobson 2014; Hove and Lillekvelland 2014). In these studies, the significance and plausibility of potential cost factors was investigated and discussed. Among these factors are, for example, inflation, changes at product level, product complexity, requirement volatility involving programs that deviate from their initial target, management errors, poor program execution, subcontracting issues, unrealistic cost estimates, and flawed initial concept caused by over-optimism and non-progressive application of new technologies in the framework of a continuous race for superior technologies, leading to development problems. Functional upgrades are a major cost factor. Arena et al. (2008), Bongers and Torres (2013), and Hove and Lillekvelland (2014) found a strong correlation between the upgrade of existing product characteristics (e.g., range, cruising speed, maximum speed, and whether the aircraft is carrier-based) and cost escalation. However, in these studies, the impact of newly added functions was not quantitatively discussed. Arena et al. (2008), and Hove and Lillekvelland (2014) used a regression vector that involved the same variables over the studied cost escalation period and that could not be extended to include newly introduced product characteristics. Bongers and Torres (2013) used hedonic prices in a Lancasterian framework, where the only possible product change scenarios are the optimization and/or combination of existing characteristics (i.e., a *fixed-size* functional space that does not allow the emergence of new product characteristics). Arena et al. (2008) explicitly mentioned the omission of newly

added functions, referring to ‘other elements’ that could not be taken into account in their regression analysis (i.e., changes in avionics, software implementation, and product longevity) because the variables for measuring them are only available for recent systems and not applicable to older systems.

Despite this, at a qualitative level, Arena et al (2008) interviewed military aircraft programs’ prime contractors regarding the topic of cost escalation: the interviewees reported that new innovative characteristics in military aircraft (e.g., stealth, software-controlled systems, and new mission equipment) are important contributors to cost escalation. Cancian (2010) also stressed that the excess cost (i.e., cost growth) incurred during the course of a military program may actually be the funding of the development of additional military capabilities that were not planned in the initial concept, and that may give extra value to the product. These are explicit references to non-negligible costs that seem to have contributed to funding the development of new product functions. However, these facts are not addressed in quantitative terms.

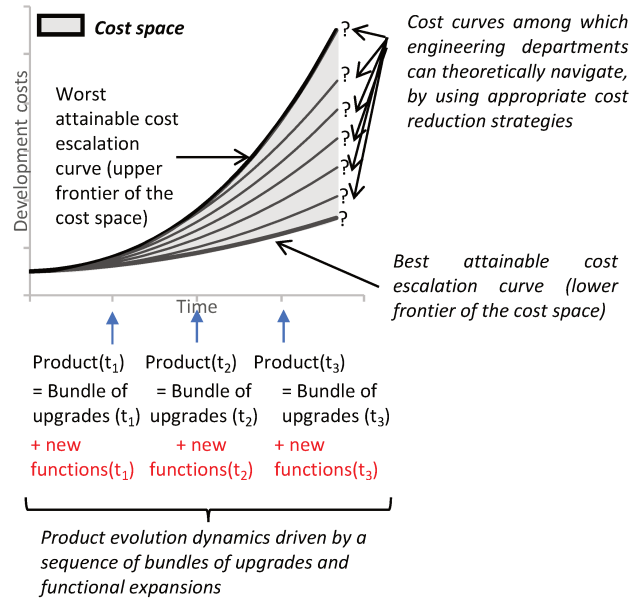
The relationship between functionality and cost is also addressed in parametric models that aim to provide estimates of design effort. Some models (Function Points (Albrecht (1979), COCOMO II (Boehm et al., 2000), cited in Fenton and Bieman (2014)) measure the functionality of a software, i.e. measure its functional size (by counting items of various types contained in the specifications), in order to provide estimates of development costs and time. In the case of hardware, Bashir and Thomson (2001) propose parametric models involving product functionality (measured with a complexity metric) and requirements severity as parameters to estimate the design effort. According to these estimation models, the addition of new functions contributes to increased design costs. However, the statistical relationships they involve do not go as far to account for *how* a continuous emergence of new functions affects design work and generates additional development costs.

This lack of insights regarding the relationship between functional expansion and development costs is problematic, particularly in industries whose products are both seemingly stabilized and under functional expansion, and which are subject to cost escalation (see the example of the commercial aircraft industry in Appendix 1). Indeed, in these industries, engineering departments do not claim responsibility for activities dedicated to functional expansion. The claimed development process is a stage-gate-like process that is seemingly dedicated to a product whose already-existing functions will be improved. This suggests that the design tasks related to functional expansion are performed by traditional organizations (e.g., those responsible for already-existing functions and those responsible for product architecture and integration), in a silent, unclaimed manner, without, for example, specific tools and methods, and a specific budget line dedicated to functional expansion.

And the cost manager confronted by the cost escalation curve experienced by the product under functional expansion but the architecture of which is seemingly stabilized, can neither say whether nor with what intensity newly added functions play a role in the cost increase dynamics. The cost manager does not know what a well-managed functional expansion phenomenon is supposed to cost. Additionally, this manager does not know how far the extra costs associated with ill-managed functional expansion can extend.

From a theoretical point of view, this raises a series of questions. If we consider the ‘spectrum’ of development cost evolution curves that an engineering department can theoretically attain depending on how proficient it is at mastering costs in the framework of upgrade-and-functional-expansion-driven product evolution dynamics (see Figure 1):

- (1) What is the contribution of functional expansion to a development cost evolution curve? What is the nature of the cost increase induced by functional expansion? What is the shape of the cost space associated with a functional expansion dynamics?
- (2) In terms of managerial practices, what strategies can be used to slow down cost escalation and move to a lower, more cost-effective cost evolution curve? Are the traditional strategies that apply without functional expansion still valid and efficient? In particular, it is widely acknowledged that the design of modular architectures (i.e., architectures whose entities (e.g., components or functions) are arranged into dense modules (also called clusters or chunks) that are as decoupled from one another as possible (i.e., inter-module interactions are weak) (Ulrich 1994; Browning 1998; Koh et al. 2015)) can facilitate the implementation of engineering changes and control the destabilization that changes may trigger in the product structure. Modular architectures facilitate the upgrading of existing modules by limiting the perimeter of the design work to a few modules. Additionally, they also facilitate the addition of new modules, that is, ‘product extension’ (Ulrich 1994) or augmentation (Baldwin and Clark 2006), also referred to as ‘add-ons’ (Ulrich 1995). It is largely acknowledged that this can contribute to reducing the development costs and time of next-generation products. Are these economic properties of modularization preserved under functional expansion or are they undermined by functional expansion, which would suggest that functional expansion calls for the acquisition of a specific capital of, for example, architectural methods, strategies, knowledge, and design rules?
- (3) In comparison with other proven cost factors, in particular functional upgrades, is functional expansion a significant cost driver that should be taken into account as an important object of cost management?



**Figure 1. Cost space within which design engineers generate a development cost evolution curve in the framework of upgrade-and-functional-expansion-driven product evolution dynamics**

The literature dedicated to cost escalation and the literature dedicated to cost estimates do not seem to address the three questions mentioned above. In an attempt to generate first insights in this regard, in this paper, we adopt the following research approach.

First, we attempt to isolate functional-expansion-driven cost evolution dynamics by simulating the theoretical ‘cost space’ (i.e., the surface delimited by the best attainable cost escalation curve, which is the lowest curve, and the worst attainable cost escalation curve, which is the highest curve, see Figure 1) associated with functional-expansion-driven product evolution dynamics (Simulation 1). In Simulation 1, we aim to obtain insights into the shapes of cost escalation curves that engineering departments can possibly generate under functional expansion (i.e., we aim to obtain insights regarding question (1)). Second, we run a second simulation (Simulation 2) to investigate whether and under which conditions traditional and well-proven modularization strategies contribute to slowing down cost escalation curves under functional expansion. This should provide us with insights regarding question (2).

Finally, with a third simulation (Simulation 3), we generate upgrade-and-functional-expansion-driven cost escalation curves, in order to explore the conditions under which the cost effects of upgrades may dominate the cost effects of functional expansion, and vice versa. This should provide us with insights into the extent to which functional expansion may be a significant cost driver, and hence should be regarded as an object of ‘cost management’ that should be taken into account (question (3)).



This paper is structured as follows; In Section 2, we examine the literature on models that could provide the equation that underlies a product-change-driven cost escalation curve, which could enable us to build Simulations 1, 2, and 3. In Section 3, we introduce the simulation model that we have chosen. In Section 4, we present the results of the simulation, which reveal that functional expansion significantly accelerates cost escalation dynamics. Additionally, it provides some insights into the conditions under which modularization strategies can slow down cost escalation curves in the context of functional expansion. We discuss these results in Section 5.

## **2. Review of the literature: review of cost model equations that could help to simulate groups of functional-expansion-driven cost escalation curves and functional-expansion-and-upgrade-driven cost escalation curves, and modularization strategies**

In this section, we examine the literature on models that meet the following three requirements:

- (i) enable the isolation of the (minimum) impact of functional expansion on development costs (to be able to build Simulation 1);
- (ii) enable the simulation of the modularization of product architectures (to be able to build Simulation 2);  
and
- (iii) enable the assessment of the extent to which functional expansion might be a significant cost driver, with respect to functional upgrades that have already been demonstrated as a major cost driver (Simulation 3).

### ***2.1. Isolating the cost impact of functional expansion***

What is the nature of the costs induced by functional expansion? What are the types of other cost effects that we have to ‘neutralize’ if we want to isolate functional expansion?

To answer these questions, we can rely on the literature on engineering changes, which distinguishes two types of engineering changes. (1) Intentional engineering changes are developed and implemented by design engineers to answer market dynamics (e.g., customer demand for more performance), respond to regulatory requirements, or follow the emergence of new technologies. Functional upgrades and functional expansion can be seen as two specific forms of intentional engineering changes. (2) While the intentional engineering changes concern targeted entities of the architecture (e.g., targeted functions, subsystems, and components), they may propagate to other entities within the architecture, and trigger additional changes (e.g., rework and redesign) in these other entities (Clarkson et al. 2004): this phenomenon is called change propagation. Intentional changes may also lead to

emergence phenomena, which correspond to unanticipated interactions between entities that were not expected to affect one another. Such emergence phenomena can result in potentially undesirable effects in the product and lead to additional work (e.g., building ‘barriers’ to prevent interaction between the newly interdependent entities) (Carlson and Doyle 2005; Alderson and Doyle 2010). If emergence or propagation effects arise, their impact on a project can be dramatic (e.g., an ‘avalanche’ of propagated changes, as theorized by Eckert et al. (2004), can have catastrophic consequences in terms of costs and delays).

Functional expansion can be seen as a specific type of engineering change. Additionally, the costs it induces could be broken down in the following manner:

- the nominal cost of the new function that is intentionally developed and implemented; and
- the cost induced by the changes propagated by the new function in the already-existing architecture.

If we want to isolate these costs caused by functional expansion, we have to neutralize the costs induced by another, more well-known and mastered form of engineering change: functional upgrades. Similar to the costs induced by functional expansion, the costs of functional upgrades can be broken down into

- the nominal cost of the intentional upgrade; and
- the cost induced by the changes propagated by the upgrade in the architecture.

We can easily set a standard nominal cost corresponding to the development effort of a new function. Additionally, we can easily define a trivial mathematical function to compute the nominal development costs over an expanding functional space.

Less obvious is how to assess the extent to which the newly introduced function disturbs the already-existing architecture, and generate propagation costs. In the next section, we investigate how cost propagation models handle functional-expansion-induced propagation costs.

## ***2.2. Cost propagation models: appropriateness to compute functional-expansion-induced propagation costs***

Cost propagation models account for how “costs propagate” as changes propagate (Georgiades et al. 2017). To model how costs propagate among the entities contained in a product, Georgiades et al. (2017) and Rebentisch et al. (2017) combined two key elements:

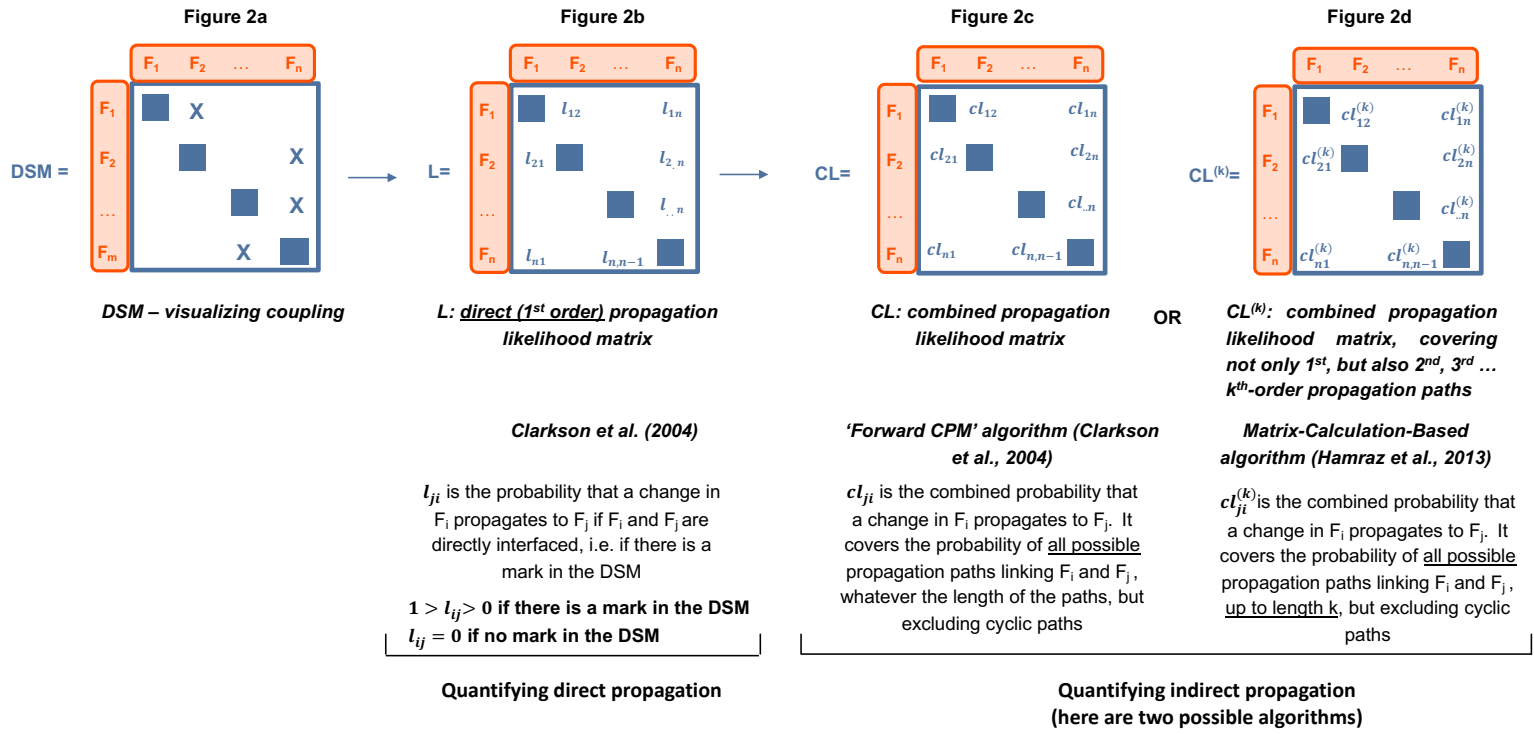
- a *change propagation model*: both Georgiades et al. (2017) and Rebentisch et al. (2017) used Clarkson et al.'s (2004) seminal matrix-based change propagation method (CPM), which enables propagation paths to be traced within a design structure matrix (DSM); and
- a *record of the individual costs of the entities* among which changes (and costs) propagate: Georgiades et al. (2017) used a *vector* and Rebentisch et al. used a *matrix* (2017).

The CPM (Clarkson et al. 2004) is a probabilistic change propagation model that quantifies propagation in terms of likelihood, impact, and risk. As illustrated in Figure 2, based on DSMs, the CPM involves a propagation likelihood matrix  $\mathbf{L} = (\mathbf{L}_{ji})_{1 \leq i, j \leq n}$ , which represents the probability  $l_{ji}$  that a change in entity  $i$  (e.g., a subsystem)<sup>41</sup> propagates to another entity  $j$  with which it is interfaced: if an interaction between  $i$  and  $j$  is highlighted in the DSM, then  $0 < l_{ji} \leq 1$ , and if there is no interaction, then  $l_{ji} = 0$ . Additionally, the CPM involves a propagation impact matrix  $\mathbf{I} = (\mathbf{I}_{ij})_{1 \leq i, j \leq n}$  such that  $I_{ji}$  is the proportion of  $j$  that needs to be redesigned if it is affected by a propagated change originating from  $i$ . Based on the likelihood matrix and the impact matrix, the CPM involves the calculation of a risk propagation matrix  $\mathbf{R}$ . Georgiades et al. (2017) and Rebentisch et al. (2017) only used the likelihood matrix of the CPM in their cost propagation models.

The likelihood matrix  $\mathbf{L}$  only covers ‘direct’ propagation paths, that is, propagation paths between interfaced entities. However, indirect propagations (with a longer-than-one propagation chain, where an entity plays the role of a bridge between two entities that are not directly interfaced) may also occur. As shown in in Figure 2c, the CPM offers the ability to predict a ‘combined propagation likelihood,’ which aggregates the occurrence probabilities of all propagation paths between two components, including longer-than-one paths, using the forward CPM algorithm (Clarkson et al. 2004); forward CPM is a brute-force search algorithm that considers all possible propagation paths individually. More recently, Hamraz et al. (2013) proposed a matrix-based algorithm that, for a given integer  $k$ , computes  $cl_{ji}^{(k)}$ , which is the probability that a change in  $i$  propagates to  $j$  through propagation paths with lengths of up to  $k$ .

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<sup>41</sup> Note that most studies that use DSM and CPM are based on component DSM. However, because this study specifically focuses on functional expansion and upgrades, we chose to represent functional-DSM in Figure 2 and all the figures in this paper.



**Figure 2. Direct Propagation Likelihood matrices ( $L$ ) and Combined Propagation Likelihood Matrices ( $CL$  and  $CL^{(k)}$ ) that are the elements of CPM involved in Georgiades et al. (2017) and Rebentisch et al. (2017)**

By combining CPM likelihood matrices with a vector featuring the individual cost of each entity, Georgiades et al. (2017) considered each possible (non-cyclic) propagation path in turn and associated it with a cost. For each pair of entities  $(i, j)$ , they computed aggregated propagation costs generated by the combination of all the propagation paths that may connect  $i$  and  $j$ . To summarize, this method provides a comprehensive view of all the propagation paths and their associated costs. When design engineers implement design changes that involve initial changes in some specific components, they can check such a visualization to observe the likely impacts of the change in terms of propagated costs. Rebentisch et al. (2017) used the articulation of the CPM and a matrix featuring the individual cost of each entity to assess the cost impacts of several specific design change alternatives, and compare them. This enables the selection of the most cost-effective alternative. They modeled a ‘design change alternative’ by highlighting in a dedicated matrix the entities to be changed in the framework of a particular alternative. Then, articulating this matrix with (normally distributed) combined propagation likelihoods and with the individual cost of each entity, Rebentisch et al. (2017) modeled the successive steps of a propagation chain, which converge under a less-than-one learning factor that is multiplied by the propagation probability at each step, and therefore eventually decrease to zero. Associated with this sequence of change propagations, the final result given by the model is a cost probability distribution that enables an assessment of the cost-effective nature of the initial intentional change alternative.

What is the nature of the changes covered by cost propagation models? Can they address functional expansion?

The CPM (as most, if not all, propagation models) is designed to help to manage increasing complexity, that is, to help to handle products for which the number of elements (e.g., parts and components), level of interconnectedness, and vulnerability to emergence and propagation tend to increase. Such an increase in complexity results from the fact that products are required to be increasingly high-performing, and must therefore be continuously improved by engineering changes (Clarkson and Doyle 2002; Alderson and Doyle 2010; Clarkson et al. 2004; Rebentisch et al. 2017). These improvements and changes that render products increasingly complex include upgrades to existing characteristics. By contrast, it is not clear whether the addition of new, innovative entities is also covered.

Indeed, the CPM and propagation models generally analyze propagation within the composite entities of a product breakdown at one moment  $t$  in the product evolution dynamics; they do not distinguish between already-existing and newly added entities. More specifically, to the best of our knowledge, they do not compare the propagation likelihood matrix at time  $t$  with the previous matrix at time  $t - 1$ , and thus do not highlight whether some of the entities contained in the matrix at time  $t$  might have been absent at time  $t - 1$ ; that is, newly added entities may very well be present in the breakdown considered by propagation models at time  $t$ , but they are neither identified as ‘new’ nor managed in a different manner to already-existing entities. In both the successive propagation steps of Rebentisch et al. (2017) and the propagation visualization provided by Georgiades et al. (2017), the size of the propagation likelihood matrix remains unchanged.

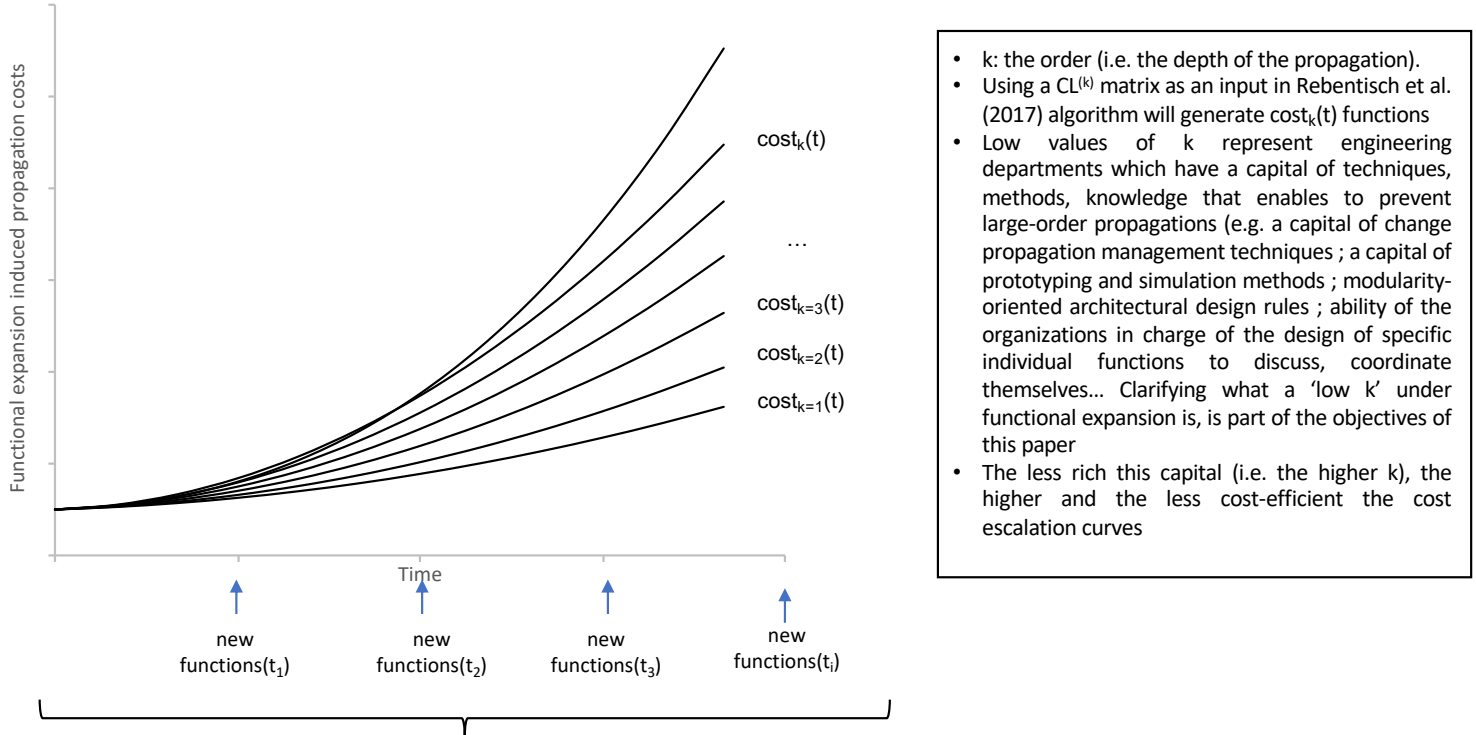
Clearly, cost propagation models do not directly provide the equation that underlies the evolution of functional-expansion-induced propagation costs with time.

First, Georgiades et al. (2017) did not consider any specific change scenarios, but provided a visualization of all possible change scenarios at a specific moment in time. Rebentisch et al. (2017) studied one moment in the product evolution dynamics at which different change alternatives were considered and compared. However, we can easily specify a sequence of bundles of engineering changes that define the move from one generation of a product to its successor (cf. Figure 3) and compute the associated *sequence of ‘propagated costs’* using the principle involved in the models of Rebentisch et al. (2017) and Georgiades et al. (2017). If we define bundles of engineering changes that involve augmenting the DSM a new row and a new column (i.e., augmenting the size of the DSM), this results in a simulation of the emergence of a new architectural entity. Additionally, if we apply the formula involved in change propagation models in the enlarged architecture, we capture propagation costs under functional expansion.

Thus, using cost propagation models, we can simulate the propagation costs caused by upgrades and the propagation costs caused by functional expansion. As illustrated in Figure 3, the integer parameter  $k$  in the  $k$ -order combined propagation likelihood matrices  $CL^{(k)}$  is highly relevant to our research. Indeed, varying  $k$  produces a spectrum (i.e., a range) of possible costs curves: a low value of  $k$  is associated with a ‘low cost curve’ in the cost space (close to the lower bound of the cost space), thereby highlighting well-controlled functional-expansion-induced propagation. A high value of  $k$  is associated with a ‘high cost curve’ in the cost space (close to the upper bound of the cost space), thereby highlighting the costs induced by ill-controlled functional-expansion-induced propagation.

Traditionally, the capability of an engineering department to master change propagation depends on its capacity to design a smart structure of dependencies that will not be too prone to generate propagated changes (Sinha 2014; Clarkson et al. 2004; Fricke and Schulz 2005; Carlson and Doyle 2002; Fu et al. 2012). More specifically, an engineering department’s mastery of propagation depends on the department’s capital of architectural knowledge (e.g., design rules, capability of the departments responsible for specific functional entities to be aware of their respective design constraints and to coordinate each other, and capital of change propagation management tools). For instance, a modular architecture facilitates the implementation of engineering changes by allowing “module designs to be changed and improved over time without undercutting the functionality of the system as a whole” (Baldwin and Clark 2006). The use of techniques and tools that highlight the possible propagation paths within the network of interrelated entities (e.g., functions, components, requirements, and design parameters) that make up a product enables design engineers to be highly knowledgeable regarding the possible consequences (in terms of propagation) of a considered intentional change, and thus helps them determine whether it is relevant to implement the change (Clarkson et al. 2004; Koh and Clarkson 2009; Hamraz 2013).

The integer ‘ $k$ ’ in Figure 3 can be seen as an indicator of the engineering department’s capital of architectural knowledge under functional expansion. Additionally, we can ask the following question: under functional expansion, should an engineering department mobilize the same capital of architectural knowledge as in scenarios in which there is no functional expansion? We start to address this wide question in Simulation 2 by investigating whether modularization strategies can slow down the pure-functional-expansion-driven cost escalation curves obtained in Simulation 1.



*A theoretical pure-functional-expansion-driven product evolution dynamics*

**Figure 3. Possibility to generate a family of propagation cost evolution curves, from the least cost-effective (the highest curves; high values of  $k$ ) to the most cost-effective (the lowest curves; low values of  $k$ ) using the Rebentisch et al. (2017) algorithm**

Thus, in this subsection, we have seen that cost propagation models can be adapted to generate functional-expansion-induced propagation costs. Associated with a function that defines the nominal costs of the newly added functions, the Rebentisch et al. (2018) cost propagation model satisfies the first requirement (i) mentioned at the beginning of this section.

Additionally, if we record the evolution of the architecture in a DSM and a propagation likelihood matrix, we can simulate modularization strategies: indeed, there exist algorithms for clustering DSM, that is, rearranging the elements of the DSM into modules (Thebeau 2001). Thus, we can capture how functional-expansion-propagation

costs evolve as we shift from a non-modular architecture to a modular architecture. This satisfies the second requirement (ii) mentioned at the beginning of Section 2.

Thus, in Section 3, we propose a method to simulate the following:

- (A) the cost of functional-expansion-induced propagation; to achieve this, we adapt the Rebentisch et al. (2018) cost propagation formula with a direct propagation likelihood matrix  $L$  whose size increases;
- (B) the nominal cost of a newly added entity that was not on the product at time  $t$ ; and
- (C) the cost reductions allowed by the modularization of the architecture defined by a direct propagation likelihood matrix  $L$ .

### ***2.3. Comparing functional-expansion-induced cost variations with other forms of product-driven cost variations that might occur during a product evolution trajectory***

Propagation costs are only one specific type of cost variation that may impact (and increase) the costs of the individual entities of an architecture: the costs of individual entities may evolve not only under the influence of propagation costs, but also under the influence of intentional changes (with the nominal costs of the very intentional changes that might trigger propagation) or learning effects (with the benefits that might be yielded from the reuse/repetition of activities that have already been completed on the previous-generation product).

In Section 2.1, we noted that ill-managed propagation could have a dramatic cost impact (e.g., cascading changes). In this case, the nominal cost of intentional upgrades and the benefits caused by learning effects might become negligible compared with the propagation costs. However, if engineering departments have a high capital of, for example, know-how, methods, tools, and practices, this enables them to master propagation (i.e., to have a low value of  $k$  in Figure 3), there is no guarantee that the nominal cost of intentional, targeted upgrades and the benefit from learning economies will be negligible with respect to propagation costs.

In Section 3, for Simulation 3, we propose a method to compute three additional types of possible cost variations:

- (D) learning effect-induced cost reductions for functions that were already present at time  $t$  and which are reused at time  $t + 1$ ;
- (E) the nominal cost increase caused by the intentional upgrade of already-existing functions  $t$ ; and



- (F) the cost of upgrade-induced propagation (to achieve this, we adapt the Rebentisch et al. (2018) cost propagation formula with a fixed-size direct propagation likelihood matrix  $L$ ).

## 2.4. Research questions

If we define a method to compute (A)-type, (B)-type, (C)-type, (D)-type, and (E)-type cost variations, we will have the necessary components to run simulations that (i) allow the isolation of functional-expansion-induced costs and (iii) assess the extent to which functional expansion might be a significant cost driver, with respect to functional upgrades that have already been demonstrated as a major cost driver. Additionally, the use of Rebentisch et al. (2018) is compatible with a modularization algorithm (Thebeau, 2001). Thus, we will have the necessary components to run simulations that will help us to address the following three research questions:

(RQ1): What is the shape of ‘pure’-functional-expansion-driven development cost escalation curves? Can functional expansion potentially have a large magnitude impact on development costs?

(RQ2): To what extent can well-proven modularity strategies slow down the cost escalation curves under functional expansion?

(RQ3): With respect to functional upgrades that have already been demonstrated as a significant cost driver, under which conditions may functional expansion also be a significant cost driver (conditions under which functional expansion would be an important object of cost management)?

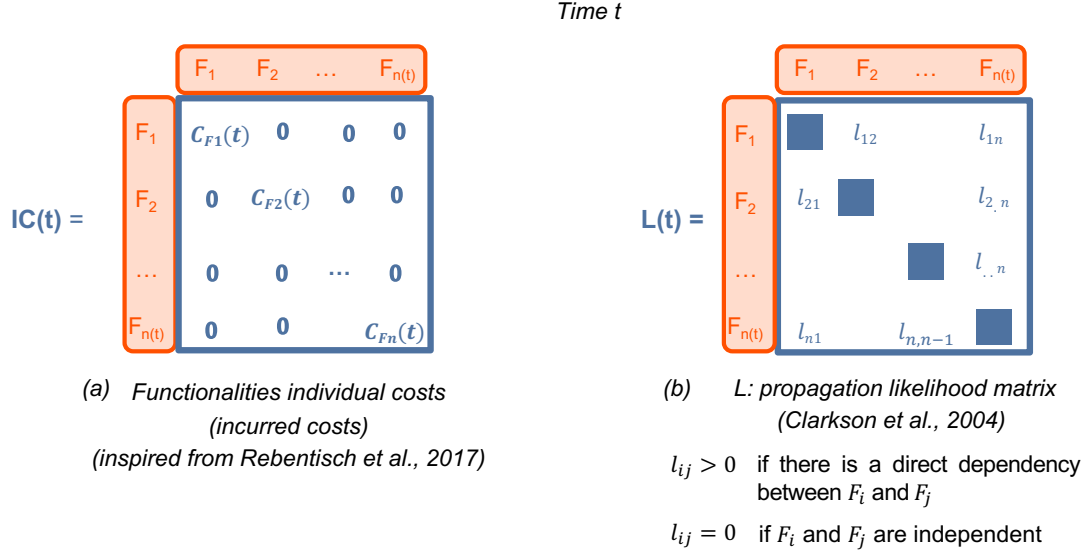
The following section details the method that supports the three simulations that we use to address these research questions.

## 3. Method: simulation of cost escalation curves based on cost propagation models

We introduce two main inputs (shown in Figure 4) that we use to generate cost escalation curves in Simulations 1, 2, and 3:

- the costs of each individual function on the previous-generation product, incurred at a given moment in time  $t$ . Following Rebentisch et al. (2017), we record these individual costs in a diagonal matrix  $IC(t) = (IC_{ij}(t))_{1 \leq i, j \leq n(t)}$ , where  $IC_{ii}(t) = C_{F_i}(t)$  is the incurred costs for  $F_i$  at time  $t$ ; and

- a propagation likelihood matrix  $L(t)$  (Clarkson et al. 2004) which represents the structure of dependencies of the modeled product. We use this matrix to compute  $k$ -order combined propagation likelihood matrices.



**Figure 4. Matrices  $IC(t)$  and  $L(t)$ , which we use to compute the evolution of the costs of individual functional entities with time**

At each time step  $t$ , the total development cost is the sum  $\sum_{i=1}^{size\ of\ L} C_{F_i}(t)$ .

In Section 2, we mentioned that, as we move from  $t$  to  $t + 1$ , five types of cost variations (from (A)-type to (E)-type), from  $C_{F_i}(t)$  to  $C_{F_i}(t + 1)$ , may be induced.

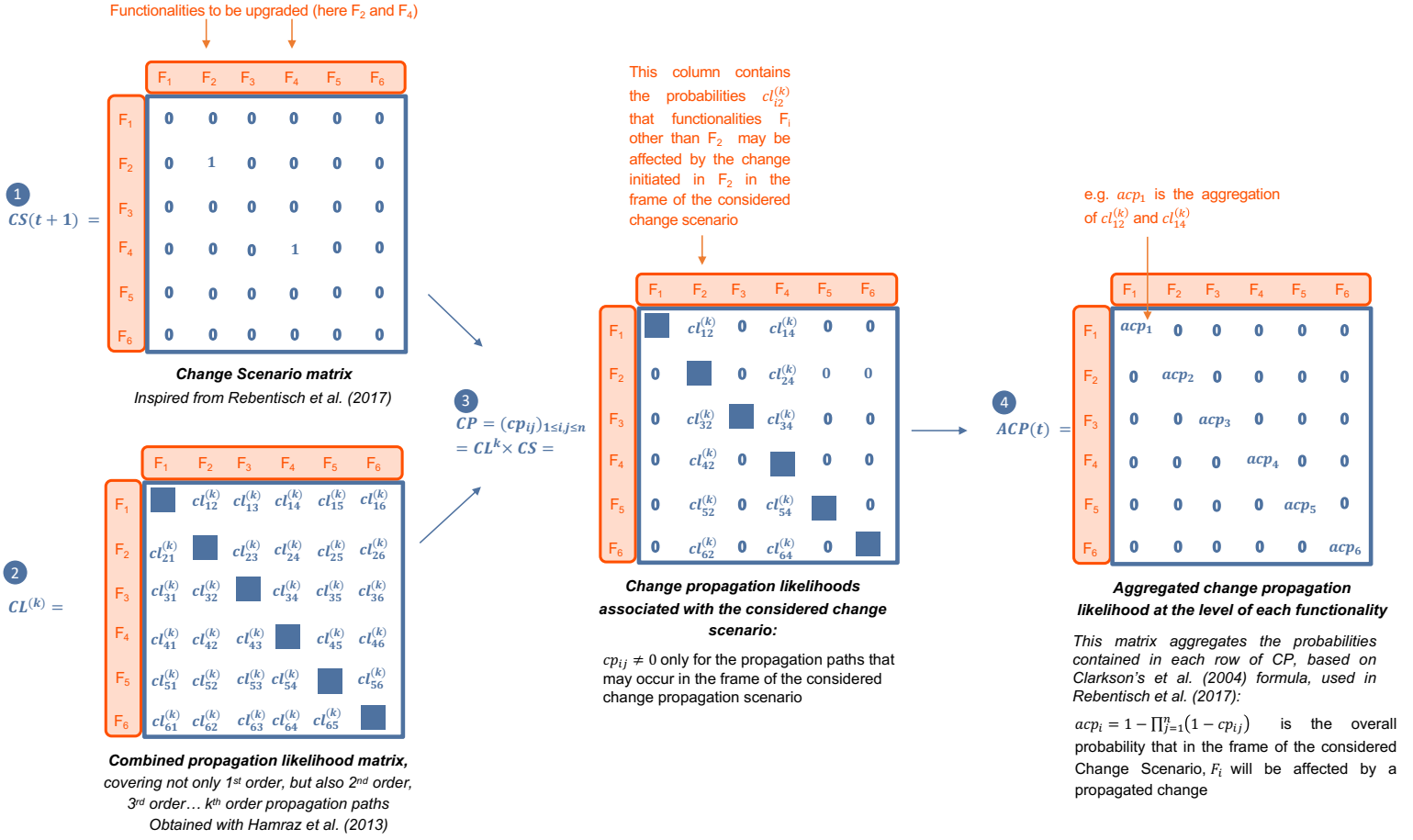
In the next subsection, we explain the approach that we use to compute upgrade-induced propagation costs and functional-expansion-induced propagation costs.

### 3.1. Computing propagation costs ((A)-type and (F)-type cost variations))

At time  $t + 1$ , we need to compute, for each individual function  $(F_i)_{i=1..n}$ , the probability that, in the framework of the considered change scenario, it will be affected by propagation (which will require redesign/rework activities). To achieve this, we take the following steps (illustrated in Figure 4):

- First, we create a change scenario matrix  $CS(t + 1) = (cs_{ij})_{1 \leq i, j \leq n}$  (inspired by Rebentisch et al. 2017): the functions  $F_i$ , which are part of the bundle of functions that have been selected to be intentionally upgraded at time  $t + 1$ , are assigned a value of 1 on the diagonal, that is,  $cs_{ii} = 1$ . All other elements are set to zero.

- Second, by applying the matrix-multiplication-based algorithm of Hamraz et al. (2013) to the direct propagation likelihood matrix  $L$ , which describes the architecture of the product being considered, we obtain a combined propagation likelihood matrix  $\mathbf{CL}^{(k)} = (\mathbf{cl}_{ij}^{(k)})_{1 \leq i, j \leq n}$ . For each couple  $(F_i, F_j)$ , this matrix enables us to identify the probability  $\mathbf{cl}_{ij}^{(k)}$ , that is, the aggregated probability that a change initiated in  $F_j$  propagates to  $F_i$  through propagation paths of length less than or equal to  $k$ . In the view provided by such a matrix, all the functions can initiate change propagation; that is, such a matrix provides information regarding all possible propagation paths of length less than or equal to  $k$ . However, within the framework of our model, only a given number of ‘selected functions’ (in the framework of the considered change scenario) can initiate propagated changes. Additionally, only a restricted number of propagation paths associated with the functions are possible.
- Consequently, in the third step of our model, we compute  $\mathbf{CS}(t + 1) \times \mathbf{CL}^{(k)}$  to retain only the occurrence probabilities of the propagation paths that may be triggered by the changes initiated under the considered change scenario. We obtain the matrix  $\mathbf{CP} = (\mathbf{cp}_{ij})_{1 \leq i, j \leq n}$ .
- Fourth, based on this new combined likelihood propagation matrix restricted to the change scenario initiated at time  $t$ , for each individual function, we compute the aggregated likelihood  $\mathbf{acp}_i$  of being affected by a propagated change. In this step, we use the formula developed by Clarkson et al. (2004).



**Figure 5. Four steps that enable us to compute, for each individual function, the probability that in the framework of a given change scenario, it will be affected by propagation, and hence redesign**

Using  $acp_j$  and an additional parameter  $\gamma_j$  (which represents the proportion of  $F_j$  that needs to be redesigned if it is affected by a propagated change, and hence, the share of the total costs of  $F_j$  that is additionally incurred if it is affected by a propagated change), we can compute the expected value of propagation costs in the case of a product without functional expansion using the following formula:

$$Expected\ propagation\ costs(t+1) = \sum_{j=1}^n \gamma_j \cdot acp_j \cdot C_{F_j}(t+1). \quad (D1)$$

(D2) If the product is subject to functional expansion, the propagation costs that may affect the  $n(t)$  functions that were already present in the previous-generation product can be computed by applying the same formula as in (D1) with individual cost matrices, propagation likelihood matrices, and change scenario matrices whose size increases; that is, the expected propagation costs caused by functional expansion are given by the following equation:

$$Expected\ propagation\ costs(t+1) = \sum_{j=1}^{n(t)} \gamma_j \cdot acp_j \cdot C_{F_j}(t+1). \quad (D2)$$

### **3.2. Computing (E)-type cost variations: cost increases induced by the additional activities required by upgrades**

This type of cost variation concerns the functions  $F_i$  that fulfill functional requirements that were already present in the previous-generation product at  $t$ , and which become more demanding at time  $t + 1$ ; for instance, some performance aspects of the function need to be improved or the function needs to be optimized. Meeting the higher functional requirements requires additional design activities from engineering departments in comparison with the activities that had been completed for the same function at time  $t$ ; for example, finer control over the behavior of the function, the phrasing of more detailed functional (sub)requirements, the recombining of design parameters, looking for and mobilizing new design parameters, and additional testing may be required. To compute this type of variation, we introduce  $\alpha > 1$ , such that<sup>42</sup>  $C_{F_i}(t + 1) = \alpha \cdot C_{F_i}(t)$ .

### **3.3. Computing (D)-type cost variations: cost reductions (learning economies) induced by learning effects**

This type of cost variation concerns the functions  $F_i$  that were already present in the previous-generation product at time  $t$  and that require neither upgrade nor optimization. They can be reused: the associated list of functional requirements and design parameters remain unchanged. The performance points that are to be attained are already attainable with the use of the functional requirements and the design parameters of the previous-generation product. The design activities that have been performed to design these functions at time  $t$  are repeated at time  $t + 1$ , potentially with a learning effect. Some activities may not need to be repeated (a 100% learning rate). However, in the case of highly complex products (which are precisely the type of products that may experience both cost escalation and functional expansion), some activities remain indispensable (although they may benefit from a learning rate); for example, resizing activities for the specificities of the generation product at time  $t + 1$  and testing activities for safety certification. To simulate the cost  $C_{F_i}(t + 1)$  of the design activities required to reuse  $F_i$ , we introduce  $\beta \leq 1$  such that<sup>43</sup>  $C_{F_i}(t + 1) = \beta \cdot C_{F_i}(t)$ .

### **3.4. Computing (B)-type cost variations: nominal cost of newly introduced functions**

This type of cost variation concerns the functions  $F_i$  that were not present in the previous-generation product at time  $t$ .

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<sup>42</sup> We sum the costs of all the functions that need to be upgraded. Hence, we do not introduce a specific  $\alpha_i$  for each  $F_i$ .  $\alpha$  can be considered as the average value of all possible  $\alpha_i$ .

<sup>43</sup> We sum the costs of all the functions that need to be upgraded. Hence, we do not introduce a specific  $\beta_i$  for each  $F_i$ .  $\beta$  can be considered as the average value of all possible  $\beta_i$ .

If  $z$  new functions  $F_i$  are introduced at time  $t + 1$ , the sizes of  $IC(t)$  and  $L(t)$  need to be increased by  $z$  as we move to  $IC(t + 1)$  and  $L(t + 1)$ . Individual costs  $C_{F_i}(t + 1)$  needs to be assigned to these  $z$  new functions  $F_i$  in  $IC(t)$ .

### **3.5. Computing (C)-type cost variations: cost reductions allowed by the modularization of the architecture defined by a direct propagation likelihood matrix L**

It is important to note that modularization also involves costs, such as the *costs of the modularization effort* or the *costs incurred by the possible drawbacks* of modularization, for example, a reduction in product performance (Ulrich 1995; Höltta-Otto and de Weck 2007). In the real world, these costs need to be considered (and compared with the potential benefits) before deciding on the implementation of a modularization strategy. However, at this stage of this research, we only quantify the *benefits* of modularization in the cost curves, that is, the extent to which modularization slows down the cost escalation curves obtained in Simulation 1.

It is also important to note that, for a given architecture, there may be several possible modularization solutions, but they may not be equal in terms of, for example, benefits and costs. The costs of some modularization solutions might outweigh the benefits and render these solutions unacceptable. In particular, the entire modularization of an architecture might be neither feasible, realistic, nor desirable. In this context, methods involving the identification of the relevant or high-priority entities to be modularized allow superior (partial) modularization solutions (e.g., Koh et al. 2015). However, at this stage of the research, we only model one particular and very basic modularization solution, which is the modularization of the entire architecture. We are aware that this is not necessarily realistic, but as a first step, this seems sufficient to provide a rough quantification of the extent to which modularization can slow down cost escalation.

To simulate a ‘basic’ modularization strategy, we use the following steps:

- We first use Thebeau’s (2001) algorithm<sup>44</sup> for clustering the DSMs, that is, for rearranging the architecture into modules.

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<sup>44</sup> We used the open access MATLAB code available at <https://dsmweb.org/matlab-macro-for-clustering-dsms>.

- Once the DSM is organized into clusters, we perform decoupling by detecting the inter-cluster dependencies, that is, the dependencies between functions that belong to different clusters (by detecting the non-null propagation likelihoods that are ‘far’ from the diagonal), and deleting some of them.
- According to our new (clustered–decoupled) DSM, we modify the input likelihood matrix  $L$ ; that is, we rearrange the functions into clusters. The propagation likelihoods  $l_{ij}$  that correspond to the deleted inter-cluster dependencies are assigned a value of 0.
- Based on this new likelihood matrix  $L$ , we generate five combined propagation likelihood matrices  $\mathbf{CL}^{(1)}$ ,  $\mathbf{CL}^{(2)}$ ,  $\mathbf{CL}^{(3)}$ , and  $\mathbf{CL}^{(4)}$  using the algorithm of Hamraz et al. (2013).

Additionally, we introduce a parameter called *decoupling\_effort*, which is an integer that denotes the number of deleted inter-cluster interactions.

## 4. Simulation parameters and findings

### 4.1. Simulation 1: shape of pure-functional-expansion-driven cost escalation curves

#### 4.1.1. Setting the simulation parameters

The objective of this simulation is to isolate functional expansion and its effects on costs. We aim to identify the cost increase pattern of a pure-functional-expansion-driven cost escalation trend. Hence, we ‘neutralize’ the cost effects of functional upgrades and the cost effects of functional reuse.

In this simulation, we only need to compute (B)-type cost variations, that is, the nominal costs of newly added entities. Additionally, the cost of individual, already-existing functions only evolve under the influence of the propagation effects caused by the newly introduced functions (i.e., (D2)-type cost variations); that is,

$$\begin{aligned} & \text{Total Expected cost increase between } t \text{ and } t + 1 \\ &= \sum_{F_i \text{ new functions}} C_{F_i}(t + 1) + \sum_{j=1}^{n(t)} \gamma_j \cdot acp_j \cdot C_{F_j}(t + 1). \end{aligned}$$

What order of magnitude do we want to simulate in terms of the increase of the size of the functional space? If we examine the commercial aircraft industry, the size of the functional space related to safety tripled between February 1965 and November 2018 (see Appendix 1). Without seeking to model this specific product, we can rely on this indication to define functional-expansion-driven product evolution dynamics.

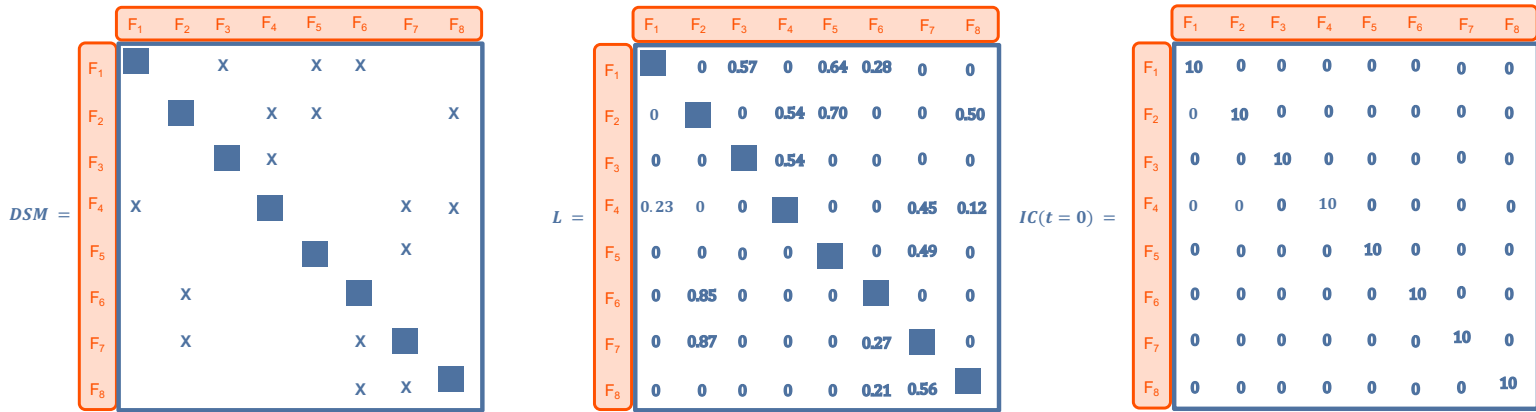
A second ‘guideline’ originates from practical constraints related to the calculation of propagation costs. Combined propagation likelihood matrices  $\mathbf{CL}^{(k)}$  with high values of  $k$  (as soon as  $k = 5$ ) require increasingly long computing

times as the size of the input propagation likelihood matrix  $L$  increases (several hours for a  $CL^{(6)}$  when the size of  $L$  is more than 20).

If we consider a ‘modest’ functional expansion scenario in which one function is added at a time (i.e.,  $\mathbf{new}(t) = \mathbf{1} \forall t$ ), starting from  $\mathbf{n}(t = 0) = 8$  functions in the initial functional space enables us to reach  $\mathbf{n}(t_{final} = 16) = 24$  functions. In terms of computing times, this remains acceptable (a few hours to compute the entire sequence of propagation costs from  $t = 0$  to  $t_{final} = 16$  using MATLAB).

After setting the number of initial functions, we can define  $L(t = 0)$  and  $IC(t = 0)$ . We do not aim at modeling the accurate cost escalation curve experienced by a particular real-world product. Hence, any matrix of size 8 is chosen, without any particular modularity property. We chose a likelihood matrix (featured in Figure 6) such that the associated DSM has a density in terms of interfaces which is realistic ( $\frac{\text{number of interfaces}}{\text{matrix size}^2} \approx 0.2$ ).

In  $IC(t = 0)$ , the cost of the individual functions is summed. Hence, we set all the  $C_{F_i}(0)$  to the same value  $C(0) = 10$ , which can be considered as an average value over all the functions.



**Figure 6. Initial architecture (characterized by its DSM and its propagation likelihood matrix)  $L(t = 0)$  and an initial matrix of individual costs**

At each iteration, we add one new function  $F_{n(t+1)}$ . We record its individual cost in  $IC(t)$  by increasing the size of  $IC(t)$  by 1 and by assigning the value  $C(F_{n(t+1)}(t+1)) = C_{F_{new}}$  to its last diagonal element.  $C_{F_{new}}$  can be considered as the average nominal cost over all the functions that are to emerge during the product evolution trajectory. The objective of this simulation is to improve our understanding of the shape of the cost escalation curve that can be expected in the context of functional expansion. In particular, we are interested in being more



knowledgeable about the minimal increasing trend that can be expected under functional expansion. Thus, we assign a value to  $C_{F_{new}}$ , which can be considered as a lower bound:  $C_{F_{new}} = C(0) = 10$ .

The parameter ***interface\_with(t)*** denotes the number of already-existing functions that interface with the newly introduced function. We define the function ***interface\_with(t)*** so that the density of the architecture in terms of interfaces remains constant. Table 1 provides values taken by the function *interface\_with(t)*. We consider the newly introduced function to be both a propagation ‘sender’ and ‘receiver.’ Therefore, at each iteration, there are  $2 \cdot \text{interfaces\_with}(t + 1)$  new interdependencies (denoted by non-null propagation likelihoods in the direct propagation matrix  $L$ ). In practice, the ***interface\_with(t)*** already-existing functions that interface with the newly introduced function are randomly chosen based on a discrete uniform distribution defined over the  $n(t - 1)$  already-existing functions. If the new function  $F_{n(t+1)}$  interfaces with  $F_i$ , we independently generate (with a uniform distribution) one random value strictly between 0 and 1 for the direct propagation likelihood  $l_{i,n(t+1)}$  and one other random value for  $l_{n(t+1),i}$ .

**Table 1.** Number *interfaces\_with(t + 1)* of already-existing functions that interface with the newly introduced function and number of new interfaces introduced at time  $t$

$t$	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
$n(t)$	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24
<i>interface_with(t)</i>	N/A	2	2	2	2	2	3	3	3	3	3	4	4	4	4	4	5

The depth  $k$  of a propagation (on which  $CL^{(k)}$  depends) varies between 1 (good control of propagation by engineering departments because of a high capital of architectural knowledge) and 4 (deteriorated control since length-4 propagation chains contribute to increased costs). For each value of  $k$ , we generate one cost escalation curve.

Finally, we must assign a value for the parameter  $\gamma$ , which is the proportion of  $F_i$  that needs to be redesigned if it is affected by a propagated change, and corresponds in our simulations to the rate by which the individual cost of a function is increased if it is affected by the propagated change. The larger  $\gamma$ , the steadier the pace of that cost increase. In what follows, we present our simulation results with  $\gamma = 20\%$ .

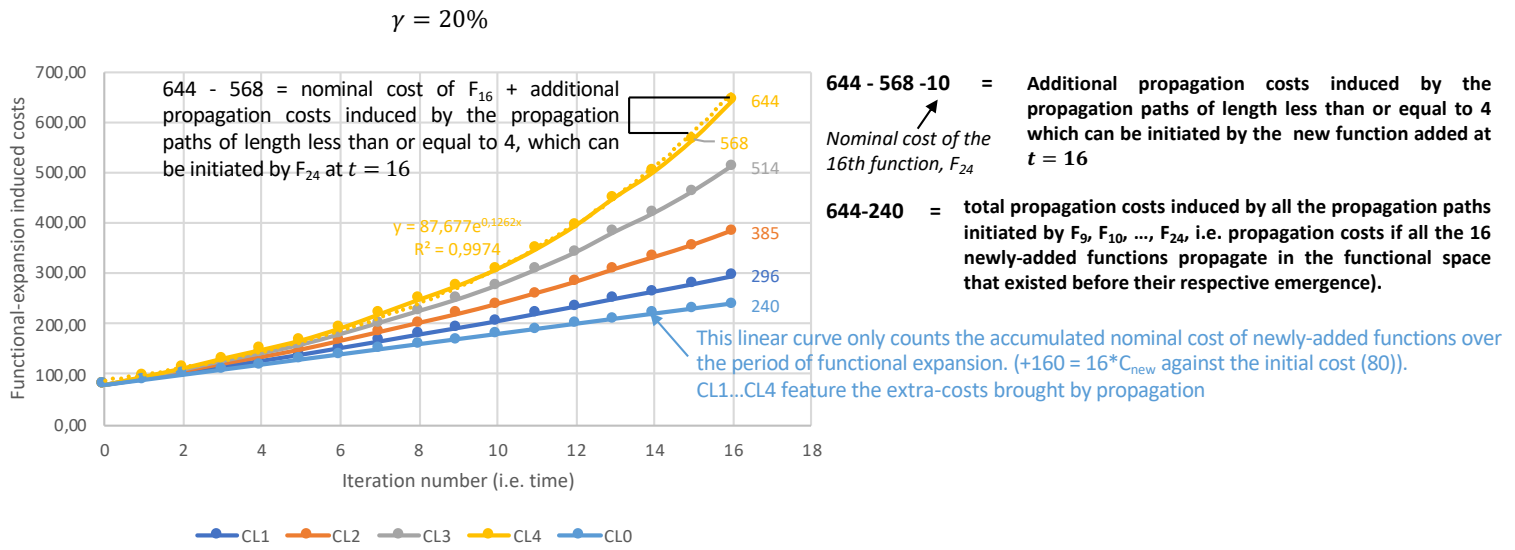
#### 4.1.2. Simulation results

Based on the ‘input’ likelihood matrix  $L(t = 0)$ , we computed four combined likelihood propagation matrices  $CL^{(1)}$ ,  $CL^{(2)}$ ,  $CL^{(3)}$ , and  $CL^{(4)}$  using the algorithm of Hamraz et al. (2013). Using the equation

$$Expected\ total\ cost\ increase\ between\ t\ and\ t + 1 = \sum_{i=n(t)+1}^{n(t+1)} C_{F_i}(t + 1) + \sum_{j=1}^{n(t)} \gamma_j \cdot acp_j \cdot C_{F_j}(t + 1)$$

and the simulation parameters described above, for each of the combined likelihood propagation matrices  $CL^{(1)}$ ,  $CL^{(2)}$ ,  $CL^{(3)}$ , and  $CL^{(4)}$ , we generated a cost curve over 16 iteration steps.

Figure 7 shows the accumulated costs that the 16 new functions generated over the 16 iteration steps, from the cost of the initial product, which was 80.



**Figure 7. Isolated functional-expansion-driven cost escalation, with  $\gamma = 20\%$ . Because the new interfaces were randomly selected, this family of cost curves represents the average cost curves obtained over 10 simulations. See the details of the mean values and standard deviations in Appendix 2.**

In the legend of Figure 7, a ‘CL $k$ ’ (with  $k > 0$ ) curve is a cost escalation curve computed with the combined likelihood propagation matrix  $CL^{(k)}$ . CL0, the lowest, linear curve (slope + 10) represents the cumulated costs that functional expansion induces if the newly added functions generate no propagation at all (+160 with respect to the initial cost 80). The difference between CL0 and the CL $k$ ,  $k > 0$  curves highlights the extra costs associated with the accumulation of new propagation paths; for instance, at time  $t = 16$ , if the 16 newly added functions are all initiators of first-order propagated changes among the functions that existed before their emergence<sup>45</sup>, they induce

<sup>45</sup> We do not count the propagated changes that a newly added function  $F_t$  could send to a new function that emerges after it. For instance, in the case of  $F_{10}$ , we only count the propagated changes it may trigger in  $F_i$ ,  $i=1..9$ . In this simulation, after having introduced  $F_{11}$ , we do not count how changes in  $F_{10}$  may affect  $F_{11}$ .

an extra cost of +56<sup>46</sup>. Additionally, if the 16 newly added functions are all initiators of fourth-order propagated changes, they induce an extra cost of +404<sup>47</sup>.

If the propagation depth of an intentional change can be as large as 4, and if all the newly added functions can be sources of propagated change, the cost impact of tripling the functional size by adding one function at a time may be dramatic. We note that we have only added one function at a time; the magnitude of a real-world functional expansion phenomenon could be greater (or accelerate with time).

This representation of functional-expansion-induced costs, where at time  $t$  all the newly added functions  $F_9, F_{10}, \dots, F_t$  propagated changes in the functional space that existed before their emergence, corresponds to all ill-controlled functional expansion phenomenon in terms of propagation, particularly if the propagation order is high. Traditionally, modularization is a well-proven method to prevent undesirable propagation chains. In the next subsection, we investigate whether modularization could break some propagation chains initiated by newly added functions and slow down the cost escalation curves in Figure 7.

#### ***4.2. Simulation 2: cost-reduction properties of modularization under functional expansion***

We applied the modularization strategy explained in Section 3: a clustering–decoupling strategy. First, we performed a ‘control test’ (Simulation 2a) in a fixed-size functional space to ensure that this modularization principle indeed enabled a slowdown of propagation costs in a traditional scenario.

##### *4.2.1. Simulation 2a: the effects of modularity propagation in a fixed-size functional space*

We set  $\mathbf{n}(t = 0) = \mathbf{8}$  functions and  $\mathbf{n}(t_{final} = 16) = \mathbf{8}$ . We used the same  $\mathbf{IC}(t = 0)$  and the same  $\mathbf{L}(t = 0)$  as in Simulation 1.

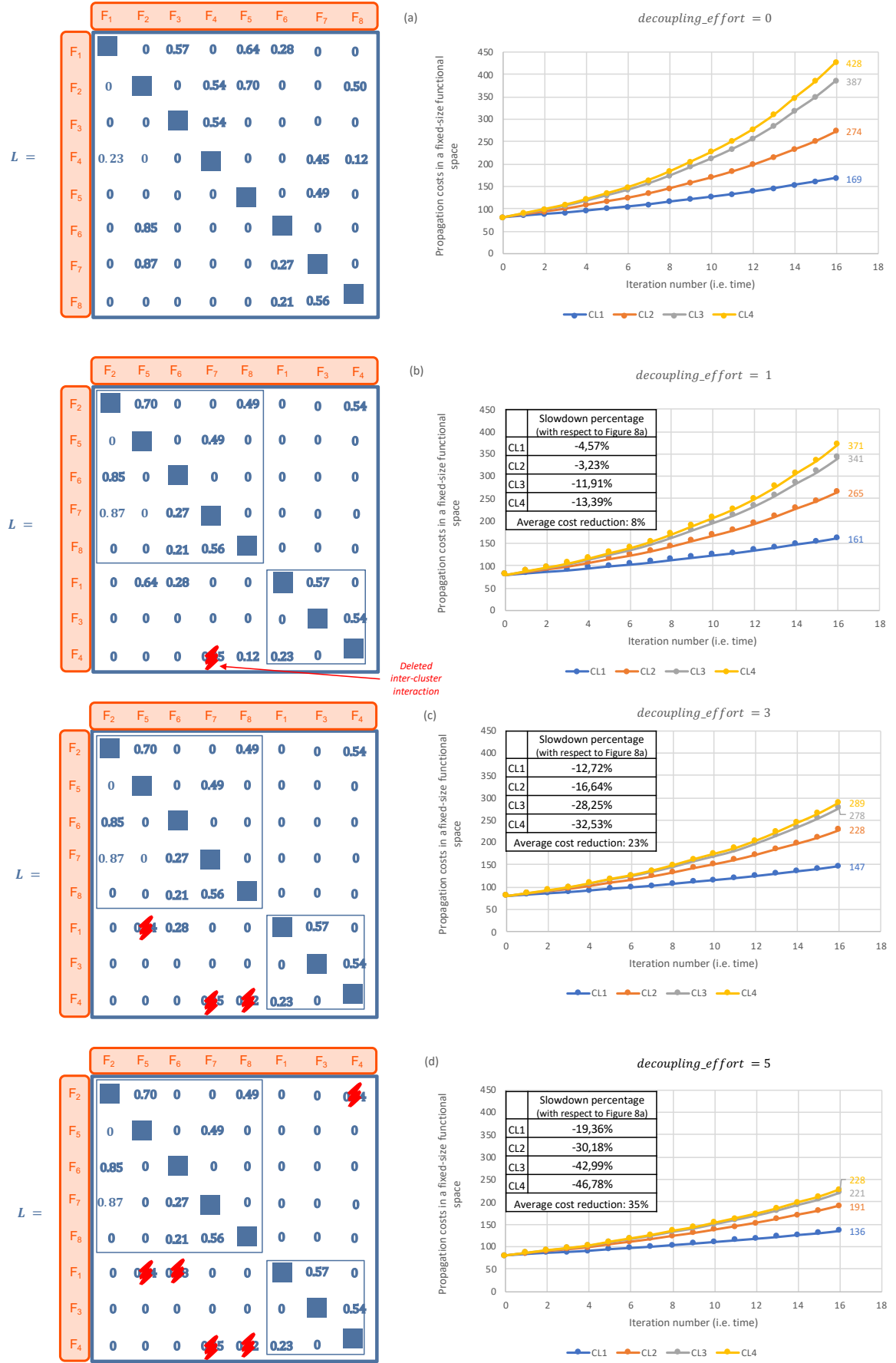
We set a proportion  $p \leq 1$ , which represented the proportion of functions of the functional space that can initiate propagation. Then we computed the first family of cost escalation curves (Figure 8a) with the non-modularized matrix  $\mathbf{L}(t = 0)$ . Additionally, we computed three families of cost escalation curves (Figures 8b, 8c, and 8d), with a clustered-decoupled  $\mathbf{L}(t = 0)$ . Figure 8 illustrates the outcomes of one such modularization strategy with  $p = 1/3$ . Because the outcomes of the modularization algorithm are random, and the selection of the functions that are sources of propagation are random, we ran the modularization strategy 10 times with  $p = 1$ , 10 times with  $p =$

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<sup>46</sup> Equals 296 – 240.

<sup>47</sup> Equals 644 – 240.

1/3, and 10 times with  $p = 2/3$  to be able to compute average values of the average cost reduction (cf. the example of the average cost reductions in Figure 8) and associated standard variations. Table 2 summarizes these results. In these simulations, we set  $\gamma = +20\%$  (impact of propagated changes).



Figures 8b, 8c and 8d: cost escalation curves after having clustered the DSM and respectively deleted one, three and five inter-cluster dependency(ies).  $p = 1$  ;  $\gamma = 20\%$ . These are average cost escalation curves. See details about mean value and standard deviation in Appendix 2.

**Table 2.** Average values and standard deviation associated for 10 executions of each scenario of Simulation 2a

$p = 1/3$		$p = 2/3$	
Clustering and decoupling effort	Average cost slowdown*	Clustering and decoupling effort	Average cost slowdown*
1	8.89% (Standard deviation: 0.07)	1	8.78% (Standard deviation: 0.06)
3	24.23% (Standard deviation: 0.10 )	3	22.46% (Standard deviation: 0.10 )
5	36.68% (Standard deviation: 0.09 )	5	37.67% (Standard deviation: 0.08 )

$p = 1$	
Clustering and decoupling effort	Average cost slowdown*
1	7.25% (Standard deviation: 0.04 )
3	18.70% (Standard deviation: 0.06 )
5	29.08% (Standard deviation: 0.06)

\* With respect to the cost escalation curves obtained with a non-modular  $L$ , obtained for 10 executions of the simulation

The control test was conclusive: our modularization strategy systematically reduced cost escalation in a fixed-size functional space.

Additionally, to be fully rigorous, we should state the following regarding how we model modularization. Given the formula used to compute our cost curves (equation (D1) derived from Rebentisch et al.'s (2017) propagation cost model), the deletion of some inter-cluster dependencies necessarily reduces costs. Conversely, if we apply the propagation cost equation to a clustered matrix, but without deleting any inter-cluster dependencies, we obtain exactly the same family of cost curves as if the matrix was not clustered. Therefore, we have checked that the slowdown in cost escalation illustrated in Figures 8b, 8c, and 8d is indeed caused by the clustered architecture of the matrix and not solely by deleting some interactions that are no longer counted in equation (D1). To check that the slowdown in cost escalation illustrated in Figures 8b, 8c, and 8d is indeed caused by the clustered architecture of the matrix and not solely by deleting some interactions that are no longer counted in equation (6), we simulated additional families of cost curves corresponding to the following scenarios:

- not clustering the DSM and randomly deleting 1 interaction in  $L$  (i.e., *decoupling\_effort* = 1);
- not clustering the DSM and randomly deleting 3 interactions in  $L$  (i.e., *decoupling\_effort* = 3); and

- not clustering the DSM and randomly deleting 5 interactions in  $L$  (i.e.,  $decoupling\_effort = 5$ ).

These simulations confirmed that the slowdown in costs observed in Figures 8b, 8c, and 8d was indeed caused by *both* the clustering and decoupling efforts. The magnitude of the slowdown observed in these families of cost curves is much smaller than in the families of cost curves plotted in Figures 8b, 8c, and 8d, as summarized in Table 2.

**Table 3.** Results of additional tests conducted to ensure that the cost reductions obtained with the modularization strategy were not solely caused by the deletion of dependencies. A comparison of the two tables confirms that this is true.

$p = 1/3$		$p = 2/3$	
Clustering and decoupling effort	Average cost slowdown*	Clustering and decoupling effort	Average cost slowdown*
1	0.99% (Standard deviation: 0.007)	1	2.16% (Standard deviation: 0.005)
3	2.45% (Standard deviation: )	3	5.44% (Standard deviation: 0.011)
5	3% (Standard deviation: 0.014)	5	5.76% (Standard deviation: 0.012)
$p = 1$ (no random effect)			
Clustering and decoupling effort		Average cost slowdown*	
1		1.93% (Standard deviation: 0)	
3		5.28% (Standard deviation: 0)	
5		5.52% (Standard deviation: 0)	

\*With respect to an equivalent of Figure 8a – no modularization attempt; statistics over 10 simulation executions

#### 4.2.2. Simulation 2b: the effects of modularizing $L(t = 0)$ on the pure-functional-expansion-driven cost escalation curve obtained in Simulation 1

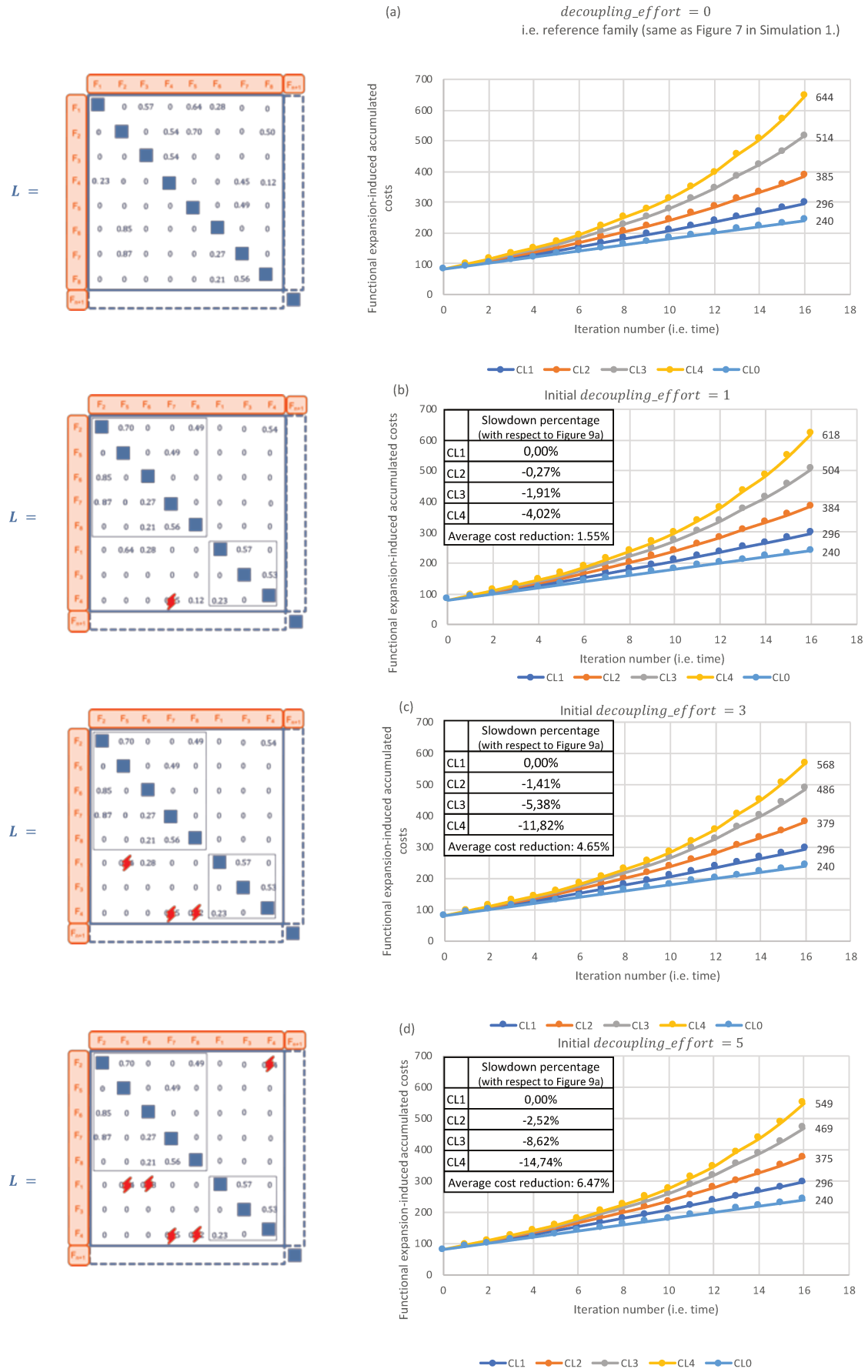
First, we consider a modularization strategy such that, at time  $t = 0$ , we have three *initially* modularized architectures (clustered and decoupled): the modularized version of  $L(t = 0)$ <sup>48</sup> with  $decoupling\_effort = 1$ ; the modularized version of  $L(t = 0)$  with  $decoupling\_effort = 3$ ; and the modularized version of  $L(t = 0)$  with  $decoupling\_effort = 5$ . We then generate a pure-functional-expansion-driven product evolution dynamics, and we compute the costs induced by functional expansion, in these modularized architectures.

<sup>48</sup> That is, the version used in Simulation 1.

Additionally, we compare the results with a reference family of cost escalation curves, which is a representative of the cost escalation curves obtained in Simulation 1 (e.g., the cost of Figure 7), with a non-modular architecture. We ran 10 simulations, which computed (a) the reference family of cost escalation curves (which is equivalent to re-executing Simulation 1), (b) a family of functional-expansion-induced cost curves generated in a modular architecture with *decoupling\_effort* = 1, (c) a family of functional-expansion-induced cost curves in a modular architecture with *decoupling\_effort* = 3, and (d) a family of functional-expansion-induced cost curves generated in a modular architecture with *decoupling\_effort* = 5.

Additionally, whereas modularization systematically significantly reduced the cost of propagation in the fixed-size functional space in Simulation 2a, this simulation in an expanding functional space revealed that modularization slowed down cost escalation only very slightly, in particular in the cases where the newly added function reintroduced a dependency between functions that had been decoupled when we modularized  $L(t = 0)$ . This introduced new possible propagation chains in the architecture, which then may induce extra propagation costs (function-expansion-induced propagation costs); that is, the newly added functions in functional-expansion-driven product evolution dynamics may undermine the modular properties of an architecture and render this architecture obsolete, thereby requiring the restructuring of the structure of dependencies. Figure 9 illustrates the (slight) cost reductions achieved in cases in which functional expansion destroyed decoupling.





**Table 4. Total average cost slowdown associated with Simulation 2b**

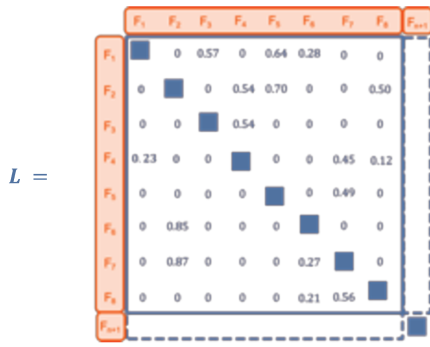
Clustering and decoupling effort	Average cost slowdown (with respect to the cost escalation curves obtained with a non-modular $L$ ) (obtained for 10 executions of the simulation)
1	-1.74% (Standard deviation: 0.01)
3	-4.39% (Standard deviation: 0.01)
5	-6.97% (Standard deviation: 0.01)

If functional expansion renders modular architectures obsolete, and by implication, does not preserve the economic properties of modularity that we illustrated in Simulation 2a, in a scenario without functional expansion, could we restore the cost-reduction potential of modularity by considering a renewal of the structure of dependencies (i.e., a re-modularization) *after each functional expansion*? Could such a continuous modularization strategy slow down costs effectively? In Simulation 2c, we simulate such an approach.

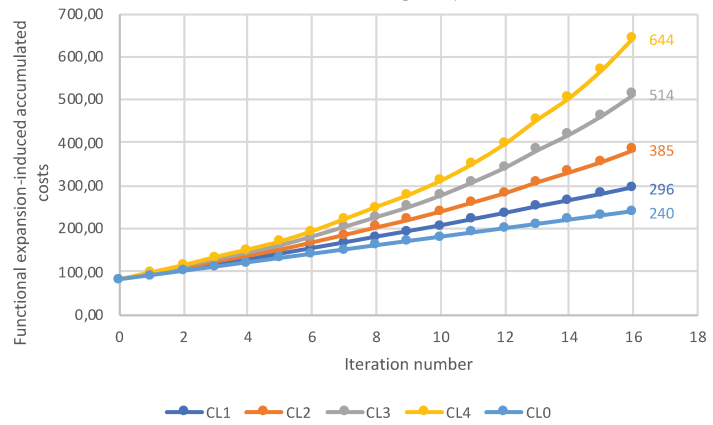
#### 4.2.3. Simulation 2c: the cost effects of a continuous re-modularization strategy

We start with the same three modular matrices as in Simulation 2b. At each iteration, after having added one new function, we re-cluster and decouple the entire architecture before computing the costs. In this simulation, the total number of deleted interactions increases in proportion to the size of the matrix. In Figure 10, the tables indicate the associated values of *decoupling\_effort* (which correspond to the total number of interactions that have been deleted, compared with the neither clustered nor decoupled reference matrix that we used to obtain the reference functional-expansion-driven cost curves in Simulation 1).

(a)

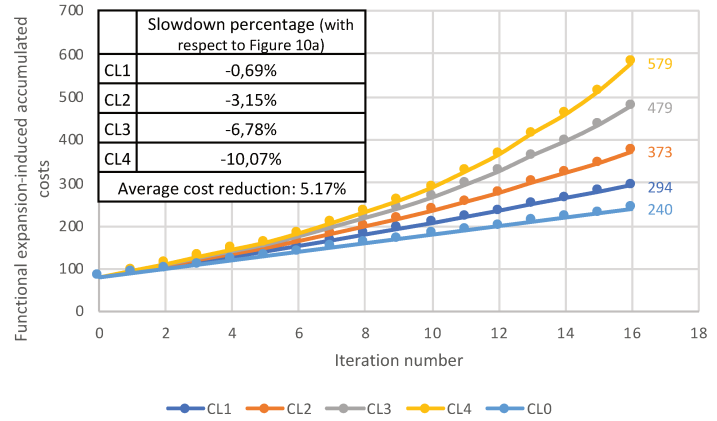


reference family (same as in Simulation 1 & same as Figure 7)



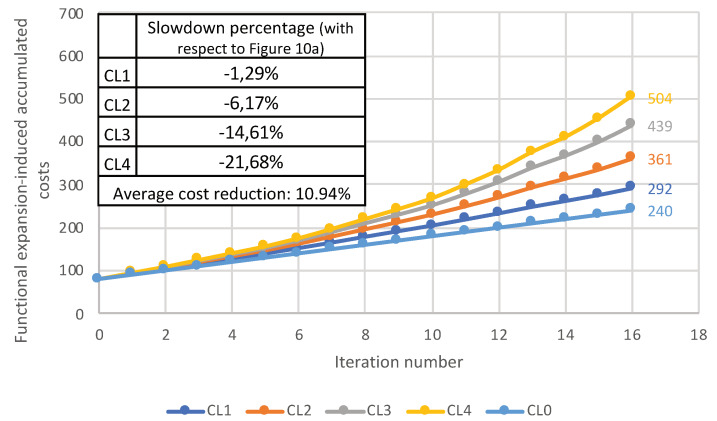
(b) Clustering-decoupling carried out after each expansion, with the following decoupling effort\*:

n(t)	decoupling effort	n(t)	decoupling effort
8	1	17	2
9	1	18	2
10	1	19	2
11	1	20	3
12	2	21	3
13	2	22	3
14	2	23	3
15	2	24	3
16	2		



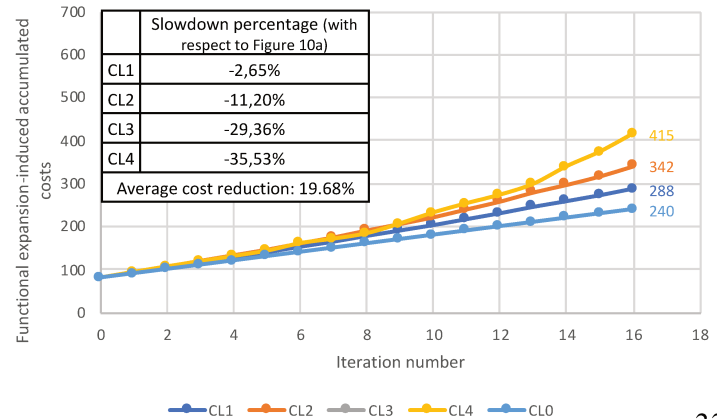
(c) Clustering-decoupling carried out after each expansion, with the following decoupling effort\*:

n(t)	decoupling effort	n(t)	decoupling effort
8	3	17	6
9	3	18	6
10	3	19	7
11	4	20	7
12	4	21	8
13	5	22	8
14	5	23	8
15	5	24	9
16	6		



(d) Clustering-decoupling carried out after each expansion, with the following decoupling effort\*:

n(t)	decoupling effort	n(t)	decoupling effort
8	5	17	10
9	5	18	11
10	6	19	11
11	6	20	12
12	7	21	13
13	8	22	13
14	8	23	14
15	9	24	14
16	9		



\*The  $L$  matrix used to compute  $CL^{(1)}$ ,  $CL^{(2)}$ ,  $CL^{(3)}$ ,  $CL^{(4)}$  and  $CL^{(5)}$  in these simulations owns *decoupling\_effort* dependencies less than the matrix  $L$  in Figure 10a.

Figure 10. Simulation 2c – carried out with a modularization effort that is re-iterated at each iteration

Contrary to an ‘only initial’ modularization effort (simulation 2a), modularizing the architecture at each iteration, after each expansion, turns out to be a powerful lever to slow cost escalation down in a context of expansion.

The curves are average cost curves obtained over 10 simulations. See details on mean values and standard deviations in Appendix 2.

**Table 5. Cost-reduction effects of a continuous re-modularization strategy**

Clustering and decoupling effort	Average cost slowdown (with respect to the cost escalation curves obtained with a non-modular $L$ ) (obtained for 10 executions of the simulation)
1	- 4.0% (Standard deviation: 0.01 )
3	- 8.6% (Standard deviation: 0.02)
5	- 17.0% (Standard deviation: 0.03)

Simulation 2c reveals that, *provided the modularization effort is renewed at each iteration, modularization can significantly slow down the cost escalation in a context of functional expansion*. Such a renewed modularization effort can be compared with the *continuous platform renewal effort* advocated by Meyer and Lehnerd (1997). We discuss this later in the paper.

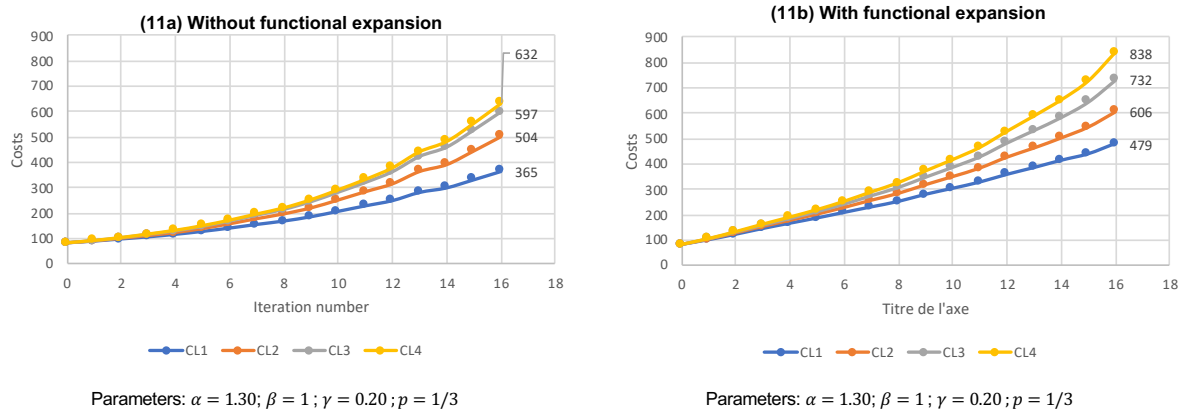
Thus far, we have only studied pure-functional-expansion-induced cost escalation curves, and we have highlighted a possible cost management architectural approach specific to functional expansion: a continuous re-modularization strategy that eliminates the change propagation risks associated with the destruction of critical independences by newly added functions. However, real-world product evolution dynamics is generally mixed, driven by both functional upgrades and functional expansion. In the case of mixed product evolution dynamics, to what extent may functional-expansion-induced costs dominate over upgrade-induced costs, and vice versa? When confronted with a mixed cost escalation curve, under which conditions should a cost manager focus on traditional cost-reduction approaches, that is, cutting the cost of individual functions and/or implementing traditional architecture management strategies (e.g., modularization), to handle upgrade-induced costs? Under which conditions should a cost manager focus on newly added functions and implement a continuous re-modularization strategy to eliminate the risk of having critical independences destroyed by a newly added function?

In an attempt to address these questions, we generated some mixed cost escalation curves and experimented with the following parameters introduced in Section 3:

- the cost of an upgrade, that is, the rate  $\alpha \geq 1$  such that  $C_{F_i}(t + 1) = \alpha \cdot C_{F_i}(t)$  if  $F_i$  is upgraded;
- the learning rate  $\beta \leq 1$  such that  $C_{F_i}(t + 1) = \beta \cdot C_{F_i}(t)$  if  $F_i$  remains unchanged is reused from the previous-generation product; and
- the proportion  $p$  of functions that are upgraded (the others being reused).

#### 4.3. Simulation 3: the relative weight of functional expansion and functional upgrades in a cost escalation curve

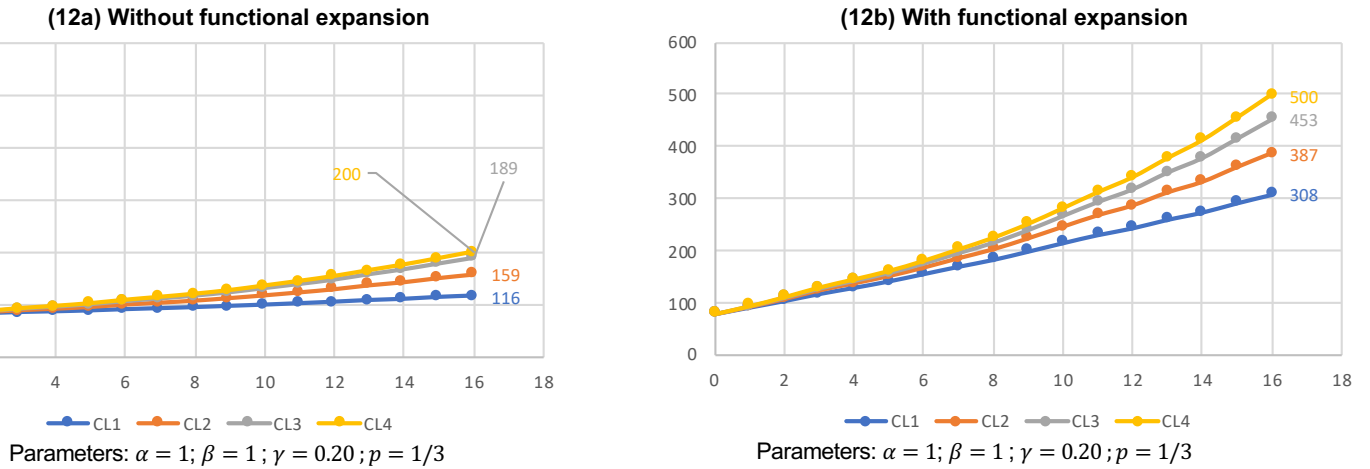
We found that there exist conditions (corresponding to a scenario in which the upgrade of the eight already-existing functions is poorly managed) such that the cost impact of functional expansion is not substantial (i.e., upgrade-induced costs dominate). For instance, under conditions such that  $\alpha = 1.30$  (i.e., a 30% increase if a function is upgraded),  $\beta = 1$  (i.e., no learning effect),  $\gamma = 20\%$ , and  $p = 1/3$ , we generated the first family of upgrade-driven cost escalation curves, without functional expansion (Figure 11a). Additionally, we generated a second family of cost escalation curves (Figure 11b), where at each iteration, one-third of the functions of the initial functional space were upgraded, and one new function was added. In this simulation, the 16 newly added functions were not upgraded. They only created additional propagation paths that could propagate costs when the existing functions were upgraded. Additionally, they created functional-expansion-induced propagation costs at the time of their introduction, in addition to their nominal costs. With these values of  $\alpha$ ,  $\beta$ , and  $p$ , we observed that the addition of functional expansion contributed to the acceleration of cost escalation, but not in a spectacular manner.



**Figure 11. Non-negligible, but non-spectacular impact of functional expansion on an ill-controlled upgrade-driven cost escalation curve**

In such a scenario, already-existing functions are not upgraded in a cost-effective manner. A trivial response to this deficiency is to take measures to cut the costs of the individual functions. This might be an interesting first step to start reducing the costs contained in the mixed cost escalation curve plotted in Figure 11b (and naturally, this would also work on Figure 11a).

We can also exhibit values of  $\alpha$ ,  $\beta$ , and  $p$ , where the costs of functional expansion substantially dominate the costs induced by upgrades. For instance, if the engineering department is capable of upgrading  $p = 1/3$  of the already-existing functions without incurring additional costs ( $\alpha = 1$ ), and if we set  $\beta = 1$  (i.e., no learning effect) and  $\gamma = 20\%$ , we obtain the cost escalation curves of Figure 12a if there is no functional expansion, and the cost escalation curves of Figure 12b if there is functional expansion. As in the previous simulation, the newly added functions are not selected to be upgraded. They only create additional propagation paths. Additionally, they induce propagation costs and nominal costs at the time of their introduction.



**Figure 12. Example of a case in which the addition of functional expansion violently changes the shape of the cost escalation curve**

In the scenario depicted in Figure 12b, design engineers are already able to upgrade functions cost-effectively. The potential of cost reduction associated with efforts dedicated to cut the costs associated with the individual costs of the functions is rather low. Additionally, according to our Simulation 3b, the investment in a classic one-shot modularization of the architecture (e.g., the development of a platform) is likely to be disappointing; indeed, nothing guarantees that the newly added functions introduced by the functional expansion dynamics will not destroy critical architectural independencies of the platform and render it obsolete. According to our Simulation 3c, a continuous re-modularization strategy would be more promising; this would require design engineers to take special care of the critical already-achieved independencies that the newly added functions might destroy, and that would require being renewed after the introduction of the new functions.

In Figure 12b, functional expansion plays a major role in the cost increasing trend. This example confirms that functional expansion may be a major cost driver. Additionally, this confirms the importance of considering functional expansion as an object of cost management that matters. More specifically, this confirms the importance

of identifying the capital of architectural knowledge and tools that could be applied to effectively slow down cost escalation curves such as those depicted in Figure 12b.

## **5. Conclusion: new insights regarding cost-effectiveness evaluation and management under functional expansion**

### ***5.1. Summary of the contributions***

#### ***5.1.1. Answer to (RQ1): the shape ‘pure’-functional-expansion-driven development cost escalation curves***

We found that functional expansion dramatically accelerates propagation cost escalation. The reason is that the continuous addition of new functions (even one function at a time) entails dramatic shocks at the level of a product’s structure of dependencies. It dramatically increases the number of possible propagation paths between two functional entities.

Our finding of a high cost potential of newly added functions is consistent with the findings of Arena et al.’s interviews (2008). This result is also consistent with the literature on complexity, which pinpoints the dramatic consequences (i.e., the high costs) of the events and contemporary trends that generate perturbations (emergence of new components and emergence of new interfaces) in product architectures (e.g., Carlson and Doyle 2005; Sinha 201; Luo and Wood 2017), that is, which increase the complexity of an architecture.

When confronted with increasing complexity, this literature recommends designing structures of dependencies that will not be too prone to generate propagated changes (e.g., Sinha 2014; Clarkson et al. 2004; Fricke and Schulz 2005). This calls for the leverage of, for example, appropriate architectural knowledge, design rules, and tools. As mentioned in our literature review, interestingly, the literature generally analyses an architecture and its interrelated composite entities without distinguishing between already existing and newly added functions. Newly added functions may very well be present in the structure of dependencies considered in studies on complexity, but they are neither identified as ‘new’ nor managed in a specific manner, which differs from the approach to managing already-existing functions. Managing functional expansion could be seen as managing complexity in an expanding architecture; it could be seen as managing a type of specific complexity, that is, an ‘expansion complexity,’ which, as with traditional complexity, calls for, for example, specific architectural knowledge, know-how, and tools.

By addressing RQ2, we investigated whether a famous complexity management approach, well-known for contributing to cost reduction, could be part of the architectural knowledge that would enable us to manage functional expansion and reduce the associated costs.

#### 5.1.2. Answer to (RQ2): the cost-reducing power of modularity under functional expansion

We found that in an expanding functional space, traditional modularization strategies do not necessarily slow down cost escalation as efficiently as they do in fixed-size functional space. If the newly added functions destroy previously achieved independencies, significant cost reduction cannot be achieved. In this study, we tested one alternative strategy, which, after each functional expansion, consists of continuously renewing the architectural independencies that may have been rendered obsolete by the newly added functions. According to our simulation, such a continuous re-modularization strategy effectively slows cost functional-expansion-induced propagation costs. We can draw a parallel with Meyer and Lehnerd (1997) by stressing the importance of *platform renewal* for ensuring long-term market leadership and economic growth.

These results suggest that functional expansion calls for a specific capital of, for example, architectural knowledge and tools, which requires the continuous design of independencies in expanding architectures, that is, which requires the design of independencies in the unknown so as not to lose control over the architecture, and to master its propensity to propagate. This calls for further research regarding what a continuous re-modularization strategy is specifically and what type of design tasks it involves. The evolution of avionics retraced by Lakemond et al. (2020) highlights a long history of functional expansion, and we can ask whether a continuous re-modularization effort underlies the integration of these very newly introduced functions.

Additionally, in the semiconductor industry, the role of cross-application managers theorized by Kokshagina et al. (2016) illustrates the nature of the activities that renewing architectural independencies in the unknown may consist of; to efficiently and successfully design generic technologies in a double-unknown context (unknown technologies and unknown markets), these actors design independencies either between unknown markets, between unknown technologies, or between unknown markets and unknown technologies.

#### 5.1.3. Answer to (RQ3): respective weight of functional upgrades and functional expansion in a cost escalation curve



Simulation 3 has revealed that the costs incurred by functional expansion may dominate the costs incurred by functional upgrades, that is, in scenarios where the nominal cost of functional upgrades is well-managed. This suggests that functional expansion is a major object of cost management. Additionally, this encourages us to further discuss the very cost management issues it entails.

## ***5.2. Discussion: cost management issues associated with cost escalation***

### ***5.2.1. Toward other strategies to slow down cost escalation under functional expansion***

With Simulations 2 and 3, we started to discuss how possible cost-reduction strategies (a: a cost-reduction effort at the level of individual functions; b: a traditional modularization effort; c: a continuous re-modularization effort) can be prioritized by a manager who aims at slowing down cost escalation. If individual functions are very ill-managed in terms of nominal development costs, rationalizing these costs could be the first step to slow down cost escalation. Contrarily, if the nominal costs of individual functions are well-controlled, the cost-reduction potential of strategy (a) is very low. The investment in a modularization strategy may yield disappointing results because nothing guarantees that the newly added functions will not destroy critical architectural independencies of the modularized architecture and render it obsolete. Among strategies (a), (b), and (c), the third (i.e., continuous re-modularization) addresses the core of the functional expansion phenomenon. It directly addresses its impact, that is, the destruction of previously achieved functional independencies. In Simulation 2c, we only tested the extent to which this type of architectural strategy could reduce costs. The test was conclusive. Further work could also try to simulate the extent to which the combination of a continuous re-modularization strategy (c) with a strategy that aims to reduce the cost of individual functions (e.g., in the framework of a digitalization strategy) could further improve cost-performance (i.e., could slow down cost escalation even more than strategy (c) alone). In addition, further works searching for more sophisticated cost management techniques under functional expansion could investigate how grouping functional expansions together versus implementing functional expansions sequentially impact costs – in particular, one could study how this factor may enhance or slow down the cost reduction power of strategy (c).

### ***5.2.2. Parallel between the need for a continuous re-modularization effort under functional expansion and the problem of platform architecture renewal***

As mentioned above, a parallel can be drawn between the continuous re-modularization effort tested in Simulation 2c and the importance of platform renewal (to ensure long-term market leadership and economic growth of the

platform) stressed by Meyer et al. (Meyer and Lehnerd 1997; Meyer et al. 1997; Meyer and Mugge 2001): design engineers need to continuously exert preparatory ‘renewal’ efforts (i.e., innovative design efforts) to be ‘ready’ when market-pull or techno-push factors render obsolete the current product platform (Meyer et al. 2001). Alongside new product development activities, the preparation of this renewal of the set of architectural rules that underlie the platform is an activity in its own right, which requires a demanding innovation effort from design engineers, for which firms are not necessarily organized (Meyer et al. 1997)<sup>49</sup>.

In this study, we showed that functional expansion is a major obsolescence factor from the viewpoint of a product architecture. Indeed, under functional expansion, the destruction of critical, previously achieved functional independencies may be a brutal factor of architectural obsolescence, thereby requiring the complete restructuring of the architecture after the addition of the new function. This suggests that regarding watching for signs of platform obsolescence, design engineers should be very attentive to obsolescence that may originate from functional expansion.

*5.2.3. Could contemporary design crises be the result of the combination of inappropriate cost-reduction strategies and functional-expansion-induced architectural obsolescence having gone unnoticed?*

As we mentioned above, if an engineering department is already proficient at mastering the cost of upgrades, the cost-reduction potential associated with a strategy that consists of cutting the cost of already-existing functions is very low. What would happen if a cost manager is misled by the steady functional-expansion-driven cost escalation curve generated by this engineering department and decides to pressure the organizations responsible for the development of individual functions to cut costs? A first (harmless) possibility is that this classic cost-cut strategy will yield little cost reduction and be disappointing. However, a second, more detrimental, possibility is that the cost cuts will prevent the engineering department from performing critical activities dedicated to observing the signs of obsolescence of the architecture. The engineering department will lack the necessary resources to prepare the renewal of the architectural design rules accordingly. Additionally, if a product is under functional expansion, the potential sources of platform obsolescence are numerous: each newly added function may destroy a critical independency and require the restructuring of the architecture. Thus, cutting the cost of an engineering department in an attempt to reduce the nominal development costs of the functions may have a detrimental effect: depriving

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<sup>49</sup> Therefore, Meyer et al. advocated setting up appropriate organizations (processes) dedicated to platform renewal and for “making platform renewal a continuous process in R&D.”

the engineering department of the necessary resources to observe and manage architectural obsolescence sources that would come from functional expansion. The non-detection of architectural obsolescence may have catastrophic impacts: a flawed architecture that does not allow functional requirements to be met and requires additional architectural work during development. Even worse, it could encourage cheating or generate an unanticipated propagation phenomenon as the product is in service. We could ask whether the combination of an inappropriate cost-reduction strategy and unnoticed functional-expansion-induced architectural obsolescence (caused by lack of engineering resources) could explain design catastrophes, such as the case of the Boeing 737-MAX or Dieselgate.

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## **Appendix 1. Functional expansion and cost escalation in the commercial aircraft industry**

Following studies on functional expansion (El Qaoumi 2016; Le Masson et al. 2019), we sought to identify whether commercial aircraft are subject to a phenomenon of functional expansion. To achieve this, we studied the evolution (with time) of a regulation known as ‘14 CFR Part 25’<sup>50</sup> or FAR-25<sup>51</sup>. This regulation is prescribed by the U.S. Federal Aviation Administration. Additionally, for an aircraft to be certified, and allowed to be commercialized and operated in U.S. airspace, commercial aircraft manufacturers must demonstrate that its design complies with the relevant airworthiness requirements contained in FAR-25. This demonstration is a significant effort in the framework of a new aircraft development program. FAR-25 is structured into subparts (e.g., Flight, Structure, and Powerplant), which are themselves structured into paragraphs. Changes at the paragraph level of (by the addition of new paragraphs, or the modification or deletion of existing paragraphs) can result from two main mechanisms. First, a change in design can be triggered by a change in the airworthiness requirements, following the report of an unsafe scenario (typically revealed by incidents/accidents) or following the decision to internationally harmonize airworthiness requirements. Second, new functions or designs initiated by aircraft manufacturers trigger the enactment of new airworthiness requirements in FAR-25 to make the new design certifiable. Therefore, changes in FAR-25 record the introduction of new or updated (e.g., reprioritized) intended purposes at the aircraft level (i.e., new functions). Hence, the evolution of FAR-25 can be regarded as a good proxy of the functional evolution of commercial aircraft.

In the framework of this research, a commercial aircraft manufacturer gave us access to a summary of successive amendments of FAR-25 that it has been compiling since February 1965. Using this document, we studied the evolution of FAR-25 between February 1965 and November 2018. This led us to find that FAR-25 has been subject to substantial changes:

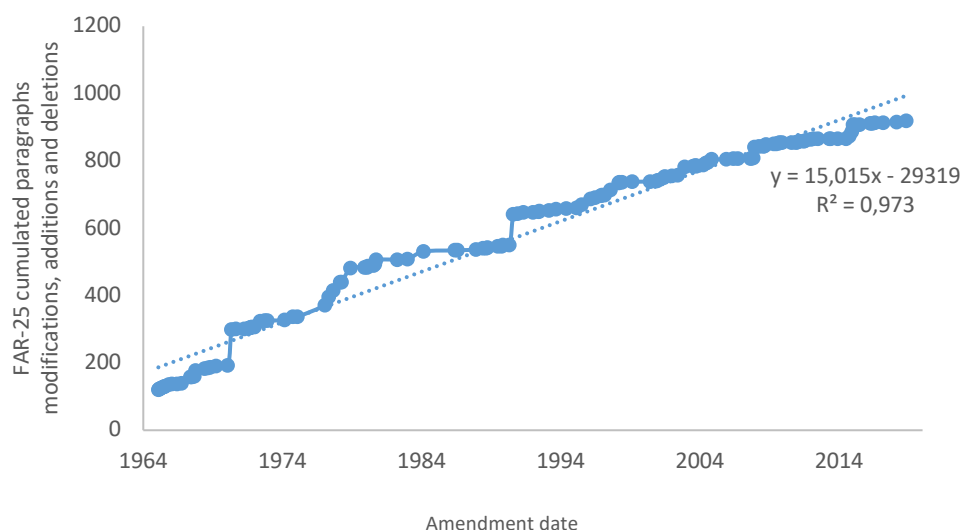
- The total number of paragraphs surged, from 122 paragraphs in 1965 to 416 in 2018 (i.e., it more than tripled).
- The number of paragraph deletions over the same period is derisory (12 paragraphs deleted).
- Modifications, that is, updating/rewriting, concerns 381 already-existing paragraphs.

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<sup>50</sup> CFR is the abbreviation of Code of Federal Regulations and 25 denotes the aircraft level.

<sup>51</sup> FAR is the abbreviation of Federal Aviation Regulations.

We plotted a curve (Figure 2) that shows the cumulated number of modified, newly added, or deleted paragraphs in the 146 successive amendments made to FAR-25 over the studied period.



**Figure 1. Capturing the commercial aircraft functional evolution by retracing FAR-25 evolution in terms of the cumulative number of newly added, rewritten, or deleted paragraphs**

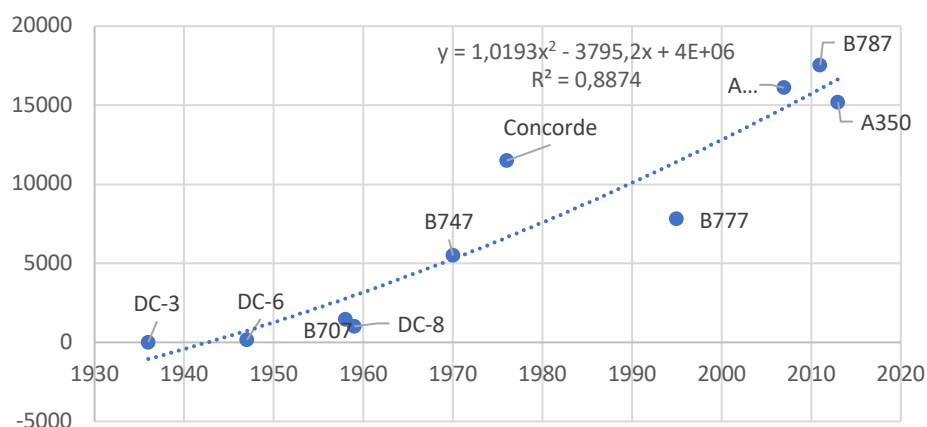
We implemented the statistical method that enables the detection of the presence of functional expansion (Le Masson et al. 2019) and found that the pattern of increase in Figure 2 corresponded to that of the phenomenon of functional expansion.<sup>52</sup>

This approach captures the potential functional expansion that might affect commercial aircraft over a specific, restricted functional space, that is, the safety functional space: we only measured a lower bound of the functional expansion phenomenon that affects commercial aircraft; that is, this leads us to think that under the seemingly stabilized architecture of commercial aircraft, this product is subject to a significant trend of functional expansion.

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<sup>52</sup> The presentation and discussion of analysis of the detailed results of the test will be the objective of another paper.

Additionally, public data (Nolte et al. 2012) regarding the development costs of commercial aircraft reveal a strong cost escalation phenomenon.



**Figure 2. Commercial aircraft development costs (in \$m, 2012 economic conditions, (Nolte et al. 2012))**

Among commercial aircraft manufacturers, there is mounting concern about this trend. Indeed, although development costs represent only a portion of total life cycle costs, and although additional efforts may be conceded during the development phase to generate savings later in the product life cycle (e.g., to reduce production costs or reduce operating costs), this dynamic is worrisome and alarming for several reasons. First, in the commercial aircraft industry, development costs (non-recurring costs) represent an extremely expensive investment ticket (even when the new design is not started from scratch). Launching one program has an impact on the cash reserves of the company and limits the possibility for launching other programs (Spitz et al. 2001). Second, the most recent products are not only costlier (and longer to develop than their predecessors), they are also characterized by major overcosts and delays with respect to the initial target<sup>53</sup>. These cost growth phenomena raise concern because they make the attainment of the investment break-even point more difficult. With the halt in production of the A380 and the final shutdown of the program, this huge investment ticket will not have paid for itself. Some press articles also ask whether the Boeing 787 will reach profitability. The CSeries program has depleted Bombardier's financial resources and even forced the company to, for example, cancel a business jet program, delay the development of another, and sell a 30-percent stake of its train building divisions.

<sup>53</sup> For example, the development costs of the A380 are estimated to be between \$20 billion and \$25 billion, which represents more than +90% with respect to the initially planned budget (\$10.5 billion). Additionally, the program experienced a two-year delay. The development costs of the Boeing 787 are also twice the initial target (\$440 billion compared with \$20 billion initially planned), and the program was 40 months behind schedule. Another example is that of the A220 (Bombardiers' CSeries): the program experienced +90% cost growth: \$6 billion incurred compared with \$3.2 billion initially planned, and a three-year delay.



Thus, in absolute terms, this cost trend seems alarming. However, in relation to functional expansion, is it? This study attempts to provide some insights in this regard.

## Appendix 2. Cost escalation curve variations caused by the random nature of the selection of the new interfaces resulting from the emergence of new functions and the random nature of the selection of the functions to be upgraded

### Simulation 1

Figure 7, for  $\gamma = 20\%$

$t$		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Average value of functional-expansion-induced-propagation_costs(t)	CL1	80	92	104	116	128	140	153	166	180	193	206	221	235	251	265	280	296
	CL2	80	94	107	122	135	150	167	184	202	220	240	261	283	309	332	357	385
	CL3	80	95	111	127	143	160	181	203	226	250	277	308	342	383	420	463	514
	CL4	80	96	113	132	150	168	192	220	249	277	310	350	396	452	503	568	644
Standard deviation	CL1	0	0	1	1	2	2	2	3	4	3	3	3	4	4	5	5	5
	CL2	0	1	3	4	5	5	7	9	10	10	11	13	18	20	22	23	22
	CL3	0	2	5	6	8	9	12	16	18	19	22	28	38	47	53	60	61
	CL4	0	3	5	7	9	11	15	17	19	22	30	40	53	68	82	99	108

(Sample size = 10)

### Simulation 2

#### Simulation 2a: control test, modularization over a fixed-size functional space

(Figure 8a.) No modularization. Reference family of cost curves. $\gamma = 20\%$ , $p = 1/3$																		
$t$		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Average value of functional-expansion-induced-propagation_costs(t)	CL1	80	84	88	91	95	100	104	109	115	121	126	132	138	145	153	161	169
	CL2	80	87	94	100	109	118	127	138	151	164	177	192	208	226	248	268	292
	CL3	80	89	97	106	118	130	142	156	174	193	212	233	257	285	318	349	387
	CL4	80	89	99	109	121	134	148	164	183	205	226	251	278	309	348	384	428
Standard deviation	CL1	0	1,70	2,10	2,25	1,88	2,28	2,53	2,40	3,36	3,08	3,01	2,30	4,36	4,63	5,54	6,79	6,20
	CL2	0	2	3	4	4	5	5	6	8	9	9	10	14	16	20	22	18
	CL3	0	2	4	5	4	7	5	8	8	10	11	11	19	21	26	29	25

	CL4	0	2	4	5	5	7	5	8	8	11	12	12	20	23	28	32	27
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(Figure 8b.) Modularization. Decoupling effort = 1. $\gamma = 20\%$ , $p = 1/3$																		
$t$		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Average value of functional-expansion-induced-propagation-costs(t)	CL1	80	84	87	90	95	99	103	107	113	118	123	128	134	140	147	153	161
	CL2	80	87	93	99	107	115	123	133	144	156	167	179	194	210	228	245	265
	CL3	80	88	96	104	114	125	135	148	163	180	196	213	234	257	285	310	341
	CL4	80	89	97	105	117	129	140	154	170	189	207	226	249	276	306	335	371
Standard deviation	CL1	0	1,7	2,2	2,4	2,2	2,8	3,4	3,7	4,6	4,7	4,5	4,3	6,3	6,1	5,7	5,1	4,4
	CL2	0	2	4	4	4	6	7	9	11	13	14	14	19	21	23	23	22
	CL3	0	2	4	5	5	8	9	12	15	18	20	21	27	29	35	35	37
	CL4	0	2	4	5	6	9	9	13	14	18	20	23	30	33	38	40	42

(Figure 8c.) Modularization. Decoupling effort = 3. $\gamma = 20\%$ , $p = 1/3$																		
$t$		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Average value of functional-expansion-induced-propagation-costs(t)	CL1	80	83	86	89	93	97	100	103	108	112	116	120	125	130	136	141	147
	CL2	80	85	90	96	102	109	115	123	131	141	149	157	169	182	195	208	223
	CL3	80	86	92	99	108	117	125	134	145	158	170	182	198	216	235	255	278
	CL4	80	87	93	100	108	118	126	137	149	162	174	187	204	223	244	264	289
Standard deviation	CL1	0	1,22	1,87	2,31	2,42	3,07	3,28	4,56	4,84	5,55	5,56	7,42	8,14	8,03	7,18	8,20	7,60
	CL2	0	1	3	4	5	7	7	11	11	14	15	18	22	24	26	30	30
	CL3	0	1	3	5	7	10	11	15	17	21	23	27	33	37	42	48	52
	CL4	0	1	3	5	7	10	11	16	18	22	24	28	35	39	45	51	56

(Figure 8d.) Modularization. Decoupling effort = 3. $\gamma = 20\%$ , $p = 1/3$																		
$t$		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Average value of functional-	CL1	80	83	86	88	91	94	97	100	104	107	111	114	118	122	127	131	136
	CL2	80	85	90	94	98	105	111	117	124	130	137	145	153	162	171	180	191

expansion-induced-propagation-costs(t)	CL3	80	86	91	96	102	110	117	124	133	140	150	160	169	181	193	205	221
	CL4	80	86	92	97	102	111	118	126	135	143	153	163	174	185	199	211	228
Standard deviation	CL1	0	1,38	2,16	2,40	2,63	3,12	3,45	4,32	4,98	5,86	6,25	8,62	9,15	8,62	8,25	8,87	8,30
	CL2	0	2	4	4	5	7	8	10	12	15	17	22	24	24	24	27	27
	CL3	0	2	4	5	6	8	10	13	15	19	23	29	31	32	32	36	38
	CL4	0	2	4	5	6	9	10	14	16	20	24	31	32	33	34	38	41

### Simulation 2b: First modularization strategy (modularization at $t = 0$ only)

(Figure 9b.) Decoupling effort = 1																		
$t$		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Average value of functional-expansion-induced-propagation-costs(t)	CL1	80	92	104	116	128	140	153	166	180	193	206	221	235	251	265	280	296
	CL2	80	94	107	122	135	150	167	184	202	219	239	260	282	308	331	356	384
	CL3	80	95	110	126	142	158	179	201	223	246	272	303	336	376	412	455	504
	CL4	80	96	112	129	146	164	187	212	239	266	298	337	379	433	483	546	618
Standard deviation	CL1	0	0.49	0.95	1.39	1.92	2.19	2.33	3.10	3.67	3.11	2.71	2.65	3.96	4.30	4.70	5	4.88
	CL2	0	1	3	4	5	5	7	9	10	10	11	13	18	20	22	24	22
	CL3	0	2	5	7	8	10	12	16	18	19	22	28	38	47	53	61	62
	CL4	0	2	5	8	11	12	17	21	25	26	32	40	55	70	82	97	107

(Figure 9c) Decoupling effort = 3																		
$t$		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Average value of functional-expansion-induced-propagation-costs(t)	CL1	80	92	104	116	128	140	153	166	180	193	206	221	235	251	265	280	296
	CL2	80	94	107	121	135	149	166	182	200	217	237	258	280	305	328	352	379
	CL3	80	95	109	125	140	156	176	197	218	240	265	295	326	364	399	440	486
	CL4	80	95	110	127	143	159	181	204	228	253	282	318	356	404	449	504	568
Standard deviation	CL1	0,00	0,49	0,95	1,39	1,92	2,19	2,33	3,10	3,67	3,11	2,71	2,65	3,96	4,30	4,70	5,00	4,88
	CL2	0	1	3	4	5	5	7	8	9	10	11	13	18	20	22	23	22
	CL3	0	2	4	7	8	9	12	15	17	18	23	28	38	48	54	61	62

	CL4	0	2	5	8	10	11	16	20	24	26	34	42	56	74	87	104	115
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(Figure 9c) Decoupling effort = 5																		
$t$		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Average value of functional-expansion-induced-propagation-costs(t)	CL1	80	92	104	116	128	140	153	166	180	193	206	221	235	251	265	280	296
	CL2	80	93	107	121	134	149	165	182	199	215	235	256	277	301	324	348	375
	CL3	80	94	109	124	139	155	175	195	214	235	259	288	318	354	387	426	469
	CL4	80	95	109	125	141	158	180	202	224	248	276	310	347	393	435	488	549
Standard deviation	CL1	0,00	0,49	0,95	1,39	1,92	2,19	2,33	3,10	3,67	3,11	2,71	2,65	3,96	4,30	4,70	5,00	4,88
	CL2	0	2	3	4	4	5	7	7	7	10	12	17	20	23	27	31	36
	CL3	0	2	4	6	8	9	12	15	16	18	22	27	37	45	51	57	58
	CL4	0	3	5	7	10	12	16	20	22	24	31	38	52	67	78	91	98

Sample size = 10

Simulation 2c: Continuous re-modularization strategy

(Figure 10b.) Initial decoupling effort = 1																		
$t$		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Average value of functional-expansion-induced-propagation-costs(t)	CL1	80	92	104	116	128	140	153	166	179	192	205	220	234	249	264	278	294
	CL2	80	93	107	121	135	149	165	182	199	216	235	256	277	301	323	346	373
	CL3	80	95	110	126	141	156	175	197	218	240	266	295	326	363	396	434	479
	CL4	80	96	111	128	145	161	183	207	233	259	289	327	366	415	459	514	579
Standard deviation	CL1	0	0,49	0,95	1,39	1,92	2,19	2,12	3,04	3,59	3,09	2,87	3,45	4,82	5,27	5,65	6,12	6,01
	CL2	0	1	3	4	4	5	6	8	10	10	12	15	19	23	25	27	26
	CL3	0	2	4	6	7	8	12	15	18	19	23	31	39	50	55	63	64
	CL4	0	2	5	7	9	11	16	20	24	25	31	41	54	71	82	96	104

(Figure 10c) Initial decoupling effort = 3																		
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$t$		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Average value of functional-expansion-induced-propagation-costs(t)	CL1	80	92	104	116	127	139	152	165	179	192	205	219	233	249	262	276	292
	CL2	80	93	106	120	133	147	163	179	196	213	230	250	271	294	314	335	361
	CL3	80	94	108	123	138	153	170	190	210	231	252	279	307	340	368	400	439
	CL4	80	94	109	124	140	156	175	196	219	243	267	299	333	375	410	453	504
Standard deviation	CL1	0,00	0,49	0,94	1,57	2,15	2,39	2,63	3,19	3,74	3,21	2,58	2,51	3,98	4,33	5,13	5,45	5,52
	CL2	0	1	2	4	5	5	6	7	8	9	9	10	14	16	18	18	18
	CL3	0	2	3	5	7	8	9	11	13	14	14	18	24	30	34	36	35
	CL4	0	2	4	6	8	10	11	13	16	17	18	23	31	40	48	53	55

(Figure 10c) Initial decoupling effort = 5																		
$t$		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Average value of functional-expansion-induced-propagation-costs(t)	CL1	80	92	103	115	127	139	152	164	177	190	202	216	230	245	259	272	288
	CL2	80	93	106	119	132	145	160	174	191	206	222	240	259	280	298	318	342
	CL3	80	94	107	121	135	149	166	181	200	217	236	260	283	312	334	363	396
	CL4	80	94	107	122	137	151	168	184	204	223	244	272	298	332	358	394	436
Standard deviation	CL1	0	0,51	0,97	1,45	2,02	2,36	2,42	3,05	3,59	3,23	3,26	2,99	4,08	4,29	5,19	5,63	5,34
	CL2	0	1	3	4	5	6	6	7	8	8	9	9	11	14	15	16	15
	CL3	0	2	4	6	7	8	9	10	12	12	13	14	18	23	24	27	26
	CL4	0	2	4	6	8	10	10	12	14	14	15	17	22	29	31	36	37

Sample size = 10

### Simulation 3



## Résumé

Cet article s'intéresse à un projet d'innovation intrigant, impliquant l'introduction d'une nouvelle machine dans une usine de l'industrie aéronautique. Ce projet a attiré notre attention car il a atteint des résultats de performance remarquablement élevés malgré un business case initial négatif. La tendance observée en matière de performance ne correspond ni à une réduction d'incertitude, ni à un pur investissement dans l'inconnu : nous démontrons que ce cas est une anomalie par rapport aux logiques de décision d'investissement et aux dynamiques d'apprentissage (Learning by Doing) qui expliquent traditionnellement les gains de performance. Ce papier met en évidence que la dynamique de performance a été rendue possible par approche managériale originale et rigoureuse adoptée pour faire face à la présence importante d'inconnu au lancement du projet et pendant son déploiement. Sur la base de cette étude de cas, nous identifions trois principes visant respectivement à orienter les décisions d'investissement, à gérer (économiquement) les projets et à organiser l'apprentissage dans l'inconnu. Le premier principe recommande de continuer à utiliser les outils économiques classiques (par exemple, les analyses de rentabilité) qui déconseillent le projet, mais de manière "détournée", c'est-à-dire comme un moyen de maîtriser les pertes en cas d'échec. La deuxième suggère de fixer clairement dans la mission de l'équipe l'objectif de découvrir de nouvelles variables de performance. La troisième propose de déployer une stratégie d'apprentissage liée aux variables nouvellement découvertes qui est basée sur l'objectif même d'accroître la rentabilité et de transformer le projet en un projet rentable.

# Managing Learning Curves In The Unknown: From ‘Learning By Doing’ To ‘Learning By Designing’

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**Abstract:** Central in this paper is a puzzling innovation project involving the introduction of a new machine in an assembly plant in the aviation industry. The project drew our attention because it achieved remarkably high performance results despite being launched with a negative business case. The observed performance trend neither corresponds to uncertainty reduction nor results from a pure investment in the unknown: we demonstrate that this case is an anomaly with regards to investment decision-making logics and learning dynamics (*Learning by Doing*) which traditionally explain performance gains. We find that the dynamics at work were made possible by an original and rigorous managerial approach adopted to address the significant presence of unknown at project launch and during project deployment. Based on this case study, we identify three principles respectively aiming at guiding investment decisions, at (economically) managing projects and at organizing learning in the unknown. The first principle recommends to keep using the classical economic tools (e.g. business cases) which advise against the project, but in a “diverted” way, i.e. as a means to keep the losses under control in case of failure. The second one suggests to clearly set in the team’s mission the objective to discover new performance variables. The third one proposes to deploy a learning strategy related to the newly-discovered variables that is based on the very objective to build profitability and turn the project into a profitable one.

## 1. Introduction

This paper focuses on a puzzling innovation project observed in a plant in the aviation industry. Firstly, this project was launched in spite of a negative business case. However, this feature is not the most disconcerting of the case since at their outset, such improvement projects are often marked by uncertainties. Further information and evidence must be acquired in order to reduce and eliminate the uncertainties. During project deployment, *Learning by Doing* mechanisms enable to collect such uncertainty-reducing information and evidence. Consequently, *Learning by Doing* represents the opportunity for the plant to achieve in the long run performance improvements (e.g. cost reduction, productivity growth...) which are higher than those that would be obtained without having invested in the project at all (Terwiesch and Bohn, 2001).



Therefore, the perspective of *Learning by Doing* represents the possibility to justify the launch of initially costly innovation projects (Mody, 1989 ; Terwiesch and Bohn, 2001). But in our case, uncertainty reduction during project deployment did not lead to performance improvements: quite the contrary, it oriented the project toward a less favourable conjuncture than forecasted in its business case. However, surprisingly, the overall performance of the project did turn out to be remarkably higher than estimated in the business case. Therefore, this case appears not to have followed traditional *Learning by Doing* dynamics. This leaves us with a couple of options to explain the rationale driving the decision to invest in this project and to explain the dramatic performance trend that it experienced. Either this project brings to light a new form of uncertainty reduction, in a form other than *Learning by Doing*. Either the literature suggests that not only uncertainties (related to previously identified states of the world) but also unknown-unknowns (related to non-initially identifiable states of the world) could have played in role in this project which would be the result of an investment decision in the unknown (Pich et al., 2002 ; Sommer et Loch, 2004 ; Sommer et al., 2008 ; Feduzi et Runde, 2014). This “investment in the unknown” scenario, implies managers paying in order to gather further information and evidence, but without initially knowing the nature of the information to be collected nor their related states of the world. In this second scenario, unknown-unknowns are expected to uncover additional information. In other words, this second explanation would imply that the plant, counting on serendipity, deployed a “gambling” strategy and accepted to pay in order to see what would happen.

In this paper, we demonstrate that our case does not fall within the scope of any of these two explanations. And we describe and attempt to characterize the original managerial response to unknown-unknowns that positively impacted performance. In other words, we observed an investment in the unknown framed by a rigorous managerial approach, which neither consisted in reducing uncertainties nor in awaiting unknown-unknowns to emerge, but which consisted in an organized approach structured around three principles respectively aiming at guiding investment decisions, at (economically) managing projects and at organizing learning in the unknown.

Consequently, we seek to provide answers to the two following research question. To what extent is the performance trend achieved in this case anomalous with respect to performance gains traditionally achieved by reducing uncertainty? How can we characterize the managerial approach (and its associated principles in terms of investment decision, economic steering and learning strategy) which resulted in the observed dramatic performance gains?

The plan of this paper is as follows. In Section 2, we review the literature related to investment decisions, *Learning by Doing* and uncertainty management. Section 3 explains the research method. Section 4 is dedicated to our case study: we demonstrate that our case is indeed an anomaly and we attempt to characterize the managerial logic and principles enabling to explore the unknown and to build such significant performance gains. We call ‘Learning by Designing’ this approach. Finally, research implications and practical implications are respectively given in Sections 5 and 6.

## **2. Review of the literature: *Learning by Doing*, investment decisions and uncertainty management**

### *2.1 Learning by Doing as a lever to justify (costly) investments in innovation, engineering... efforts*

Learning by repeating fixed tasks is the most obvious form of learning: by repeating tasks in a fixed-facility environment, individual workers accumulate experience and learn, which results in a reduction in the time needed to fulfil the tasks, and as a consequence allows costs reductions. This form of autonomous, free of charge and non-intentional learning has often been associated with both Wright’s (1936) curves which describe labour-hours (and thus production costs) decreasing with cumulative production volumes in aircraft assembly plants and with Arrow’s (1962) seminal notion of *Learning by Doing*. However, Wright (1936) notes the diminishing returns of the phenomenon described by his curves and he is less interested in learning at the level of individual workers than in levers which could reduce production costs, such as scaling effects, investment in new equipment... (Bell et Scott-Kemmis, 2010). Similarly, whereas many studies claiming to represent the ‘Learning by Doing’ frame focus on the relationship between cumulative production volumes (cumulative outputs) and firm’s productivity, Arrow’s original work studies the relationship between cumulative investments (cumulative inputs) and productivity (Bell et Scott-Kemmis, 2010). By focusing on cumulative inputs (which record investments in new equipment, triggering “new situations” in the production environment) rather than on cumulative outputs, Arrow is more interested in productivity gains resulting from “changed situations” than in productivity gains resulting from repetition:

*“Learning associated with repetition of essentially the same problem is subject to diminishing returns. There is an equilibrium response pattern for any given stimulus, toward which the behaviour of the learner tends with repetition. To have steadily increasing performance, then, implies that the stimulus situations must themselves be steadily evolving rather than merely repeating.” (Arrow, 1962)*

Thus, Wright’s (1936) approach and Arrow’s (1962) definition of *Learning by Doing* represent a lever to justify (costly) investments in projects involving new situations (e.g. new equipment, new processes...). In particular, they already indirectly represent a lever to justify investments marked by a part of uncertainty (triggered by the new situation).

Arrow’s (1962) work introducing the concept of ‘Learning by Doing’ is theoretical. But more or less recent cases show that investments in *new* equipment and technical improvement turn out to explain dramatic productivity gains that could not be attributed to “Learning by repeating” mechanisms. This is namely the case for impressive productivity gains in aircraft and ship production during WWII which occurred in spite of important turnover and / or scarce labour force (Mishina, 1999 for the case of the Boeing B-17 ; Budrass et al., 2010 for several German manufacturers, and Thompson, 2001 for the Liberty ship producers). A study carried out in a more contemporary firm producing specialty chemicals (Sinclair et al., 2000) also finds cost reductions which seem to be more attributable to process R&D aiming at introducing technological innovations than to learning at the level of individual workers.

If investments in new situations trigger learning effects, which enable to reduce uncertainty and reach performance improvement targets, one can seek to better understand how exactly such learning mechanisms operate (subsection 2.2) and which managerial principles “efficient learning” and efficient performance improvement rely on (subsection 2.3).

## *2.2 Reducing uncertainty by experimenting*

Let us consider a process characterized by a set of  $n$  variables in a plant. The performance of the process is determined by the value of some of these  $n$  variables. In order to improve performance, a manager can decide to introduce new equipment. Such a deliberate decision to invest in new equipment can be seen as a decision to invest in a form of induced (deliberate) learning (Dutton and Thomas, 1984) which Terwiesch and Bohn (2001) call *experimentation*. This triggers a new situation (as described by the "Learning by Doing" theory) which destabilizes the knowledge that the manager has about some of the  $n$  variables characterizing the process (Bohn, 1994). For instance, instead of accurately knowing the value of a given variable, only a probability distribution indicating the mean and a standard deviation around this mean is known. In other words, the new equipment introduces uncertainty and the plant loses control of some of the  $n$  variables of the process (Bohn, 1994). The reason is that some information is missing (Tyre and von Hippel, 1995) (because of uncertainty), which therefore induces problems to solve. A classical approach enabling to acquire the missing information, in order to reduce uncertainty and regain control over the destabilized variables is to deploy an experimentation plan. Thomke (1998b, 2003) namely proposes an "experimentation model" relying on iterative experimentation cycles, and more specifically, on trial-and-error cycles involving the four following steps:

- Designing an experiment
- Building the necessary models or prototypes (the "apparatus") to deploy the experiment
- Running the experiment
- Analysing the results: the results provide new information (new knowledge). Armed with these new information (i.e. having learned), designers can deploy consecutive cycles which will enable to progressively converge toward a performance target (or an "*acceptable result*" (Thomke, 1998b)).

Efficiently reducing uncertainties, and therefore efficiently reaching the performance targets requires a well-designed experimentation plan: this implies deploying a relevant sampling strategy, by devoting experimentation efforts to topics marked by critical uncertainty.

Numerous research works have proposed to characterize and model what well-designed experimentation strategies are. For example, Thomke (1998) notes that the heterogeneity in terms of firm performance can be explained by the heterogeneity of their experimentation strategy, i.e. by the way in which they combine the possible forms of experimentation (simulation, prototyping...). Consequently, Thomke (1998) proposes a model aiming at enabling managers to identify the optimal "mix" of these different strategies: building this mix represents a lever to optimize both development costs and lead-time when developing a new product. More specifically, the model identifies an optimal point which informs managers of when it is time to switch from one form of experimentation to the other. Thomke (1998) insists on the fact that budgets (e.g. simulation budget, prototyping budget) should be allocated depending on this optimal point, instead of following an arbitrarily-decided budget distribution (such as switching from simulation to prototyping once all the ex-ante allocated simulation budget is exhausted).

Justifying investments in experimentations in a plant is not obligatorily straightforward, since in the short run, deploying improvement initiatives and associated experimentations as enablers to reach performance targets can appear costly, in comparison with focusing on ensuring that the production line is run at full-capacity (indeed, with such experimentations, problems are encountered, which slows down the production rate...) (Terwiesch and Bohn, 2001). However, Terwiesch and Bohn (2001) stress the idea a plant which accepts to bear experimentation costs earlier is likely to yield better performance in the long run, namely in terms of quality (which involves economic cost avoidance). In order to help guide decisions of investments in experimentation, they propose a model involving the costs and benefits of experimenting.

In sum, in this subpart, we described the mechanisms through which experimenting generates new insights and learning effects which enable to address uncertainty. However, in the two following subsections, we will see that 'Learning by experimenting' is applicable up to a certain degree of uncertainty only. In particular, when the degree of uncertainty is such that we are in the presence of unforeseeable uncertainties (i.e. in the presence of unknowns), experimentation as defined above is ill-adapted (Gillier et Lenfle, 2018). One major issue is managerial: Gillier and Lenfle (2018) demonstrate that the management principles proposed by Thomke (2003) to frame the above-mentioned experimentation cycles are not adapted when it comes to deal with the unknown, which calls for new principles (and they identify new managerial principles, as we will explain later).

The rest of this section is structured as follows. Since subsection (2.2) highlights that deploying experimentations to reduce uncertainty is all but automatic and that it calls for strategic decision-making and for a rigorous organization during deploying:

- subsection 2.3 is devoted to the existing approaches to manage experimentation in the presence of (basic) uncertainty (and thus reduce uncertainty)
- subsection 2.4 focuses on the issues faced in the presence of unknowns and on the possible management principles

### *2.3 Managing uncertainty reduction*

In line with Arrow's conception of learning stemming from 'changed' situations, Mishina (1999) investigates the learning mechanisms responsible for dramatically increasing productivity in the plants producing the Boeing B-17 during WWII, and supports the idea of "learning" as the consequence of new situations, with the expression "*Learning from new experiences*". However, the sole modifications of the production systems do not lead to productivity gains. Mishina (1999) stresses that learning at the level of core managers plays a significant role in the observed productivity gains (through an efficient work coordination, namely making it possible for operators to be tasked with operations in which their potential is optimally exploited: no waiting times...)

According to many research works (Adler et Clark, 1991 ; Sinclair et al., 2000 ; Carrillo et Gaimon, 2004 ; Budrass et al., 2010), the efficiency of learning mechanisms relies on managerial learning, with managers being empowered to make decisions on two dimensions:

- (1) Investing in new equipment / in technical improvement / innovation process / in engineering activities (which boil down to experimentations which will trigger new situations)
- (2) Investing in new knowledge, competences, expertise (through trainings, hiring new employees...)

Researchers have investigated the mechanisms through which the decisions on these dimensions enable to build learning, and performance eventually. A first issue is the very decision of investing. Carrillo and Gaimon (2004) seek to model how managers allocate their budgets on these two dimensions (investing in process change and investing in human capital). Modelling situations in which managers are motivated by the incentive to reach a given target, they study how uncertainty impacts the manager's choices: they demonstrate that the nature and the rules of the firm's managerial system

(more or less severity toward failed projects for example...) have a strong impact on what is learned, and on firm's performance as a consequence.

Budrass et al. (2010) epitomize the importance of objectives and incentives managers are subject to: during WWII, the National Socialist regime imposed fixed-price contracts on German aircraft manufacturers: this represented the incentive to reduce costs for the industrials, all the more so as they could keep the benefit of the margin resulting from cost reduction). New contracts being based on the reached production costs at the expiration of the previous contract, managers were encouraged to think ahead of time about new technical solutions which could be deployed to reduce costs even more. For this reason, several successive waves of cost reduction are observed.

Therefore, a clear purpose (set at the level of managerial objectives) seems to be an important characteristic of economically-efficient learning.

The mechanisms through which ‘Learning by doing’ takes place and impacts firm's performance can be summarized as follows: firm's managerial system and objectives set at the managers' level influence managerial investment decisions in

- (1) Technical capital (new equipment, new processes...)
- (2) Knowledge capital

These investments trigger experimentation situations, which enable to collect further information and evidence (i.e. learning), which enables to reduce uncertainty and bring about economic improvements phenomena (productivity gains, cost reduction...). These very learning mechanisms are due to steered experimentations which are deployed in an attempt to solve problems raised by the perspective to introduce or raised by the introduction of new equipment or processes. As these problems are being solved by experimenting, the convergence toward an initially-set performance target is achieved.

The approaches so far described consist in ‘Learning by reducing uncertainty on  $n$  known variables’ (by deploying well-designed experimentation plans). They apply when the states of the world involved in the project are exhaustively known and when managers have the possibility to estimate how the different variables will be impacted by their decisions (knowing at least the mean and the standard deviation of the variables). In this case, when it comes to decide whether to launch a project or not, managers have the possibility to compute expected values to inform and guide their investment decision. All the states of the world being known, the topics (e.g. the performance variables) most affected by critical uncertainty are identifiable. Therefore, managers and their teams can establish clear objectives, deploy a sampling strategy and design experimentations which will efficiently address these critical topics, by generating the necessary knowledge. In sum, when all the states of the world are known, managers can knowingly make their investment decisions. And they can design experimentation plans likely to effectively reduce uncertainty and optimize the convergence of the project toward an initially-set performance target. In some, a rather straightforward and clear process (implying the identification of the critical topics affected by uncertainty, the definition of a sampling strategy, the definition of hypotheses and associated experiments, the deployment of tests and the collection of the new information) appears when it comes to manage uncertainty reduction.

However, ‘Learning by reducing uncertainty on known variables’ is unsuitable when it comes to deal with *unforeseeable uncertainties* or *unknown unknowns*. In particular, the perspective of ‘Learning by reducing uncertainty on known variables’ does not offer the possibility to justify costly investments in the presence of unknowns, nor to efficiently manage project deployment in the unknown.

## *2.4 Investment decisions and project management in the unknown*

When dealing with uncertainty, all the ( $n$ ) variables affecting performance are known beforehand, although for some of them, only the mean and the standard deviation (instead of a precise value) are identified. In contrast, when dealing with the unknown, initially unforeseen events are likely to arise and to uncover new performance variables (related to new states of the world) whose standard deviation cannot even be estimated.

### *- Investment decisions in the unknown*

*Unknown unknowns* can be defined as the inability to identify beforehand “all relevant variables affecting performance”, i.e. as the inability to identify beforehand all states of the world (Sommer et al., 2009) or as “events which could not be imagined as a possibility prior to their occurrence” (Feduzi et Runde, 2014). The unknown makes it difficult to justify investment decisions: managers have an incomplete view of all possible the states of the world which are likely to be involved and impacted in the course of the project (Pich et al., 2002 ; Sommer et Loch, 2004 ; Feduzi et Runde, 2014). The calculation of investment costs and benefits being based on incomplete information, it is imperfect, if not impossible. For this reason, economic tools, such as the Net Present Value and Business Cases often predict non-profitability for promising innovation projects. Therefore, managers face a dilemma. Either they do not invest in the project at all, which represents the risk for the firm to miss opportunities, or to be unprepared to face market or competitive shocks... Either they invest, i.e. they pay to see what happens. The firm’s level of risks and error acceptance, more or less openness regarding ‘Learning from errors’ ... play a role in deciding on the dilemma. However, these considerations do not involve elements related to the economic performance of the project. What makes the economic decision tricky is that some information are missing (e.g. non identifiable states of the world). Feduzi and Runde’s (2014) propose an algorithm enabling to generate beforehand “alternative hypotheses” (which enrich the initial decision space with new states of the world): the rationale is to proactively eliminate unknown-unknowns, in order to avoid encountering them during project deployment. But the problem related to guiding the economic decision remains open: traditional managerial techniques do not indicate when generating alternative hypotheses and testing them with experiments (in order to create new knowledge) stops being economically-efficient (Feduzi et Runde, 2014).

If managers decide to invest, the rationale of the investment decision is to gather further knowledge and information during project deployment. Experimentation is again a key approach to collect initially-missing information (Feduzi et Runde, 2014 ; Gillier et Lenfle, 2018). But in this case, managers are in charge of experimentations in the unknown, which cannot be steered in the same way as described in subsection 2.3.

### *- Project management in the unknown*

One can note that Thomke’s (1998b, 2003) trial-and-error model is resilient to unknown-unknowns. With its successive experimentation cycles, the approach is flexible enough so that managers can integrate in the subsequent experimentation cycles the unknown-unknowns discovered during a previous experiment. Indeed, trial-and-error learning offers the possibility to actively search for new information, and to progressively adjust, refine and even redefine the objectives depending on what is learnt when collecting further information and knowledge (Sommer et al., 2009). However, the decision to invest in experiments and the design of these experiments (especially the definition step) are based on the initially-known states of the world only. If unknown unknowns are to be unveiled, they will only be taken into

consideration from the next experimentation cycles onwards. Managers patiently wait for unknown-unknowns to emerge and are ready to adapt the project accordingly (Snowden et Boone, 2007). Trial-and-error learning consists in a *reactive* approach to unknown unknowns. Accompanied with Thomke’s (2003) management principles (cf. table 1 below), this approach works as long as managers and their team own the necessary knowledge to generate hypotheses and define experiments to test these hypotheses. But in the absence of prior knowledge, i.e. when uncertainty reaches a degree that can be designated as “extreme”, Thomke’s (2003) principles do not apply (Gillier et Lenfle, 2018). Gillier and Lenfle (2018) explain that “completeness” (in the sense of Garud et al., (2008)), represents the “boundary” for the applicability of Thomke’s principle: under the completeness condition, managers and their teams own sufficient knowledge to fairly specify the problem they need to address and thus, to design experiments accordingly. From the case of the Manhattan project, they also define key features which characterize an experimentation in the unknown (1- lack of theoretical knowledge, 2- lack of theoretical instruments, 3- absence of a pre-established organization) and which render Thomke’s principles inapplicable. Consequently, based on the Manhattan Project, Gillier and Lenfle (2018) propose principles to manage experimentation in the unknown, in the absence of prior knowledge (cf. table 1 below).

<b>Thomke’s principles</b>	<b>Gillier and Lenfle’s principles</b>
Anticipate and exploit early information	Identify what cannot be predicted by current theory & focus on the most challenging aspect
Experiment frequently but do not overload your organization	Create new divisions and recruit new expertise
Combine new and traditional technologies	Observe and measure unknown phenomena with new instruments
Organize for rapid experimentation	Conduct overlapping experiments
Fail early and often, but avoid mistakes	Do not expect perfect tests & learn from imperfect tests

**Table 1:** comparison between Thomke’s (2003) experimentation principle & Gillier and Lenfle’s (2018) experimentation principles

Contrarily to the knowledge developed in Thomke’s (2003) approach, which draws on already known information, the knowledge generated when applying Gillier and Lenfle’s (2018) principles is related to new states of the world since they draw on a diagnosis on the unknown (completed by identifying what cannot be predicted by current theory).

Regarding the deployment of a learning strategy, the extended principles proposed by Gillier and Lenfle (2018) highlight that the processes that govern experimentation in the unknown are far less straightforward and sequential than the process that appears for uncertainty reduction (that is: identification of the critical topics affected by uncertainty, definition of a sampling strategy, definition of hypotheses and associated experiments, deployment of tests and collection of the new information). In the unknown, as outlined in the fourth principle, experiments overlap: one given experiment does not match one clear set of hypotheses to be tested and associated tests results. Thomke and Gillier’s (2018) principles imply carrying out “crude experiments” in order to “see what happens” and start building basic knowledge and theory. In this approach, close interactions between theory-building and practical experimentations enable to progressively structure the exploration of the unknown.

In sum, the important point to bear in mind is that most of what has been so far theorized regarding how to manage experiments actually draws on uncertainty reduction dynamics. Gillier and Lenfle's (2018) paper on the Manhattan project is a notable exception. This is not very surprising since, cases of management in the unknown are quite rare.

We did identify in a plant in the aviation industry a case which seems to have been driven by logics which have to do with management in the unknown, especially in terms of investment decision, economic steering and learning strategy. In this case, everything tended to support the hypothesis according to which the project was not profitable. No alternative information indicated that the project could turn out to be profitable. In spite of that, it was launched and proved far more economically-efficient than any hypothesis could suggest. Consequently, in the paper, we try to understand how an initially highly improbable hypothesis turned out to prove true during project deployment. We namely seek to characterize the managerial logics and principles which played a significant role in turning this highly improbable hypothesis into reality.

To this end, using the method described in Section 3, we draw on this case in an attempt to provide new insights regarding the management of investment decisions, economic performance and learning strategies in the unknown.

### **3. Method**

Central in this paper is a puzzling innovation project which involved the introduction of a machine relying on a new drilling technology in an assembly plant in the aviation industry. This project drew our attention because it achieved remarkably high performance results despite being launched with a negative business case.

As mentioned in the introduction, we seek to address two research questions. To what extent is the performance trend achieved in this case anomalous with respect to performance gains traditionally achieved by reducing uncertainty? How can we characterize the managerial approach (and its associated principles in terms of investment decision, economic steering and learning strategy) which resulted in the observed dramatic performance gains?

In order to address the first research question (anomalous nature of our case), the method is the following. This case does not fall into the category of improvement projects experiencing uncertainty reduction, be it in the form of 'Learning by Doing' or in another form, nor into the category of investments in the unknown consisting in paying in order to collect further knowledge and "see what happens", in the absence of any structured economic steering approach. Consequently, this single-case study constitutes an anomaly (Siggelkow, 2007) which epitomizes the management of an investment in the unknown. We demonstrate this by applying a statistical method on data related to the costs and the savings generated by the project, with a particular interest in the trends and dynamics that followed these data. In order to understand the dynamics which affected the costs and the savings in our case, we broke these latter down and tried to understand how their nature evolved over time, as the project performance was evolving.

After having demonstrated the anomalous nature of our case, we attempt to characterize the managerial logic at work behind the observed performance trend (and thus answer the second research question) with an in-depth case study. As Thompson (2010) notes, studies at the level of individual plants offer the opportunity to gather rich data and precisely retrace dynamics followed by the firm, such as moving down its learning curve. Interviews with the managers carrying out the project provided us with narrative elements that we analysed, which offered us insights to better understand the mechanisms underlying the observed upward performance trend.



#### **4. Case study: an innovation project demonstrating a highly remarkable performance evolution – from ‘Learning by Doing’ to ‘Learning by Designing’**

The case described below occurred in the assembly plant in the aviation industry. The project considered in this case consisted in launching automated-drilling robots on some stations of a given production line. The R&T department had been developing and testing the technology for four years when the plant started studying the opportunity of a pilot deployment in the production environment, as TRL 6 had just been reached. The project seemed very promising, with interesting benefits in terms of costs and lead-time reduction. However, it faced a classical hurdle for innovation projects: the business cases did not meet the profitability criterion (a less than two-year payback time) required to launch the project. In total, eleven business cases scenarios were studied. None was positive with respect to the two-year profitability criterion. In spite of that, because the plant managers supporting the project were confident in its potential, the project was launched (with a subsidy from the R&T department financing the part of the investment which made the business case negative). The selected scenario implied a step-by-step deployment of two robots on a given production line. After this step-by-step deployment project (project 1), another project (project 2) involving the deployment of 16 robots on another production line was launched.

We will describe and analyse the learning mechanisms, the economic trends and the managerial logics on both projects. To that end, we first demonstrate that project 1 is an anomaly since its performance trend is not governed by an uncertainty reduction dynamic, nor does it consist in a form of “investment in the unknown” theorized in the literature. Consequently, we will propose in subsection 4.4 the concept of ‘Learning by Designing’ to account for the observed mechanisms and managerial approach.

##### *4.1 Project 1: Step-by-step deployment of two robots*

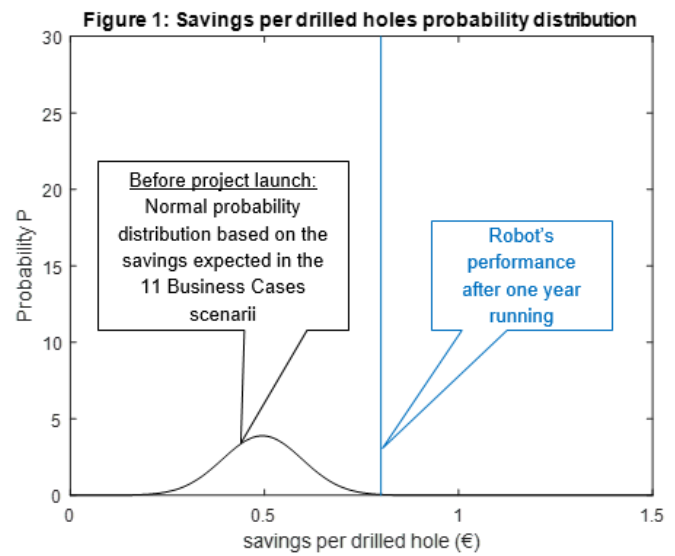
Given the uncertainties and the level of unknown surrounding the project (in spite of the fact that the managers responsible for the project were confident in its value), a first robot was deployed alone, as a pilot phase in Project 1, in order to validate some assumptions and get further knowledge before launching the second robot part of the project.

After one year running the first deployed robot, the results in terms of costs and benefits were stunning:

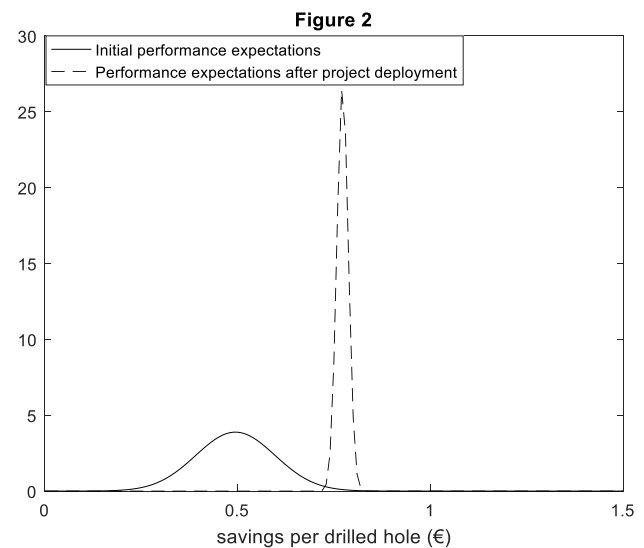
- Overcosts: +14% versus what was planned
- But also extra-savings per product: +31% versus what was expected

In the following subsection, we sought to understand better the nature of this upward trend in terms of savings. We namely attempted to understand what was contained in these extra 31%.

Using the mean and the standard deviation of the 11 savings estimations in the 11 initial business cases, we were able to plot the probability distribution of the expected savings-per-drilled-hole (we used a normal distribution). After one year running the first deployed robot, the performance in terms of savings per drilled hole (represented by the vertical line in Figure 1) is far higher than expected and corresponds to a value which had a very low probability of occurrence in the initial distribution.



Before installing the second robot on the production line, savings estimations were re-adjusted, based on the savings generated by the first robot: eight new Business Case scenarii were realized. We represented the probability distribution of these adjusted savings with the dashed curve plotted in Figure 2. (The solid curve is the same as the curve plotted in Figure 1: it represents the initial saving estimations.) We observe a dramatic shift on the right, i.e. a dramatic performance gain with respect to the initial estimations before project launch.



Today, these two robots associated to initially non-profitable business cases are still running and generate profit. In other words, the performance gains were such that they enabled to turn Project 1 into a profitable project.

In the next subsection, we seek to explain this stunning robot's performance gain illustrated by the change from the solid curve to the dashed curve.

#### 4.1.2 *Narrative description of the events which arose during Project 1 deployment*

As a link with the literature review in Section 2, we can consider that the drilling robot impacted a process characterized by  $n$  variables at the outset of the project. In particular, the savings estimations were distributed across two variables:

- Recurring Costs savings (induced by lead-time reduction: the robot installation-drilling-uninstallation time is supposed to be less than the time of the initial human-operated process ; and induced by a reduced number of blue-collars required to work on the line)
- Non-quality reduction

As described in the literature review, the introduction of the new robot destabilized some of the  $n$  variables, i.e. the managers and the operators lost full control of some of these variables.

If we look at the perspective of learning on the 2 known variables, by solving encountered problems, it did not help improve the performance in terms of savings per drilled holes. It even oriented the project toward a less favourable conjuncture because of some deficiencies in the initial assumptions. In particular, the specifications assumed that the robots would cover 100% of a certain kind of contingencies previously managed by human operators. However, they turned out to only be able to cover  $x\%$  ( $x < 100$ ) of these contingencies. For instance, some perpendicularity conditions had to be met for the robots to be able to operate. The specifications implicitly assumed that these conditions would be met. But they were not always met: in this case, the robots were not able to autonomously restore perpendicularity, as human operators would do. Such events paralyzed the line and extra labour hours (implying extra Recurring Costs) were needed to restore perpendicularity (as a solution to the encountered problem). As a consequence, regarding the 2 known variables, the plant learnt that the robots operated on a restricted action field with respect to what was initially planned. So learning on these known variables impacted the performance negatively. In terms of probability distribution, this deterioration would correspond to the solid curve shifting on the left.

On the other hand, unforeseeable uncertainties (unknown unknowns) emerged during deployment, in the form of unanticipated events. Among these unknown unknowns was the interest that the robot, (as a co-worker of a new kind which was not working well and which was paralysing the production line), aroused among the operators. This unknown-unknown (the operator's interest) was strategically managed by the managers carrying out the project. They used it as an opportunity of “free trainings”, to provide the operators with the opportunity to enhance their skills and become robots' programmers. Ultimately, the association between the robot operating on a restricted action field and the more-skilled operators turned out to be more efficient than the initial assumption of robots covering 100% of the contingencies. And a third variable emerged in the savings generated by the robot: *Rework Avoidance*, in addition to *Recurring Costs savings* and *Non Quality avoidance*.

In sum, managers turned the unknown-unknown which emerged (that was the operators' interests' in the robot) into an opportunity for the project. More specifically, they turned it into the opportunity to build a new variable / a new “dimension” (*Rework avoidance*) in the structure of the savings. This contributed to the robot's performance gains.

The narrative story of the deployment of the two first robots (project 1) suggests that “Learning in the Unknown” effects have played a significant role in the robot's dramatic performance gains. In other words, it suggests that the observed performance trend has nothing to do with “uncertainty reduction on known variables”, i.e. with classical Learning by Doing and uncertainty reduction. In subsection 4.3, using the data concerning the savings per drilled hole, we test this hypothesis.

But before that, we describe the logics at work in project 2.

#### *4.2 Project 2: Deployment of 16 new robots on the production line of another product*

Following the first project, a second project involving the deployment of 16 new robots (identical to those deployed in Project 1) was launched. In this second project, not only was the affected product different from the one involved in the

first project, but it also faced an important challenge in terms of ramp-up (contrarily to the product of the first project whose production rate was in a rather ‘stable’ dynamic). Aware that the two robots involved in Project 1 had become profitable and considering the strategic nature (due to the ramp-up challenge) of the affected production line, we expected the Business Cases of this second project to be positive. Interestingly, as we collected the data, we observed that they were negative, despite taking into account the learning effects induced by Project 1, namely by incorporating in the savings structure the *third* variable unveiled during Project 1, that was ‘*Rework Avoidance*’. Faced to this finding, we attempted to understand the learning effects and performance trends related to this project.

Project 2 clearly benefited from the learning effects that Project 1 yielded: the technology moved up from TRL6 to TRL9. Under the regime of cooperation between robots and programmers, the technology was now reliable. However, due to the ramp-up challenge, Project 2’s managers imagined a more ambitious project aiming to efficiently *reduce the lead-time*. To that end, they imagined a more complex organization of the robots’ work on the station (for instance implying concurrent tasks instead of sequential tasks in Project 1). How does Lead-Time Reduction impact the performance variables, namely, the 3 known savings variables?

- Firstly, lead-time reduction impacts the first variable of the savings structure that is *Recurring Costs Reduction* (since Recurring Costs directly result from the number of hours spent by blue collars on the process).
- Besides, the more reduced the lead-time, the more important the benefits over one period of time (one month, one year...), since more products are delivered: this aspect is not captured by our probability distribution which represents the savings per unit product per drilled holes (this view was interesting to illustrate the technical progress of the machine). However, in the firm’s Business Cases evaluating the profitability of the projects, the estimated savings per unit product are multiplied by the expected production rate over a given year. As a consequence, the benefits resulting from the possibility to produce more units thanks to reduced lead-time was taken into account in the firms’ prevision.
- However, in a context of ramp-up, lead-time reduction does not only impact the savings variable that is *Recurring Costs Reduction*. Indeed, behind each hour of delay lies the perspective for the firm of not meeting the ramp-up objective and facing severe penalties as a consequence. This aspect was not introduced into the Business Cases assumption, whereas the objective of using these robots as an enabler for a successful ramp-up (and *avoiding costs due to delays*) is the very managerial orientation which structured the deployment and the organization of Project 2. Consequently, *avoidance of costs which would be induced by unsuccessful ramp-up* is a new variable which joins the three other variables structuring the savings (that were *Recurring Costs reduction*, *Non Quality avoidance* and *Rework Avoidance*). This fourth variable was invisible in the Business Cases.

This variable has not been integrated in the Business Cases and has not formally been used as a lever to demonstrate the profitability of the project, because this new variable is ‘being built / designed’ by the project managers. This new performance variable has been ‘imagined’ ahead of time, very early in the project. The first months of the project consist by the way in a setup period aiming to accurately identify the number of lead-time hours that can be saved with the ambitious reorganization of the tasks carried out by the blue-collars and the robot (and to adjust and adapt the reorganization of the tasks so that lead-time savings are optimized). One can note a shift in the nature of the objective: whereas in Project 1, the purpose was to demonstrate the robot’s capacity to reduce costs, in Project 2, the purpose is to optimize lead-time (for a successful ramp-up), even if this implies higher costs.

This addition of new performance variables can be seen as a generative / and expansive dynamic affecting the structure of the savings attributable to the robot. Managers play an important role in imagining and building these new variables. And building performance on these new variables seems to put learning mechanisms at work: by designing these new initially unknown variables, managers structure the unknown. We call 'Learning by Designing' this managerial logic, which we describe in more details in Subpart 4.4 to address our second research question.

Before that, we address our first research question (to what extent is the performance trend achieved in this case anomalous with respect to performance gains traditionally achieved by reducing uncertainty ?) with a statistical method in subsection 4.3.

#### *4.3 Testing the hypothesis according to which that the observed performance trend is not governed by "uncertainty reduction on known variables"*

In the Bayesian statistics, reducing uncertainty consists in updating prior probability distributions on the basis of new information (gathered by making observations). The above-mentioned narrative elements show that 'Learning by Doing' mechanisms during project deployment are not responsible for the observed performance trend. Consequently, using the Bayesian statistics, we seek to verify whether another form of 'uncertainty reduction' played a significant role in this case. To that end, we test the hypothesis  $H_1$  according to which the curve shift would result from "Learning by reducing uncertainty" on the  $n$  known variables of the process

Then, we test  $H_1$  with Bayesian statistics. Indeed, if  $H_1$  is true, the change from the solid curve to the dashed curve results from Bayesian updating. This would mean that observations on initially known variables (estimated and summarized in the solid curve) have been made through probabilistic draws among these parameters. In other words, the savings estimations contained in the 11 initial business cases (solid curve) can be considered as corresponding to a prior probability  $\theta \sim N(\mu_p, \tau_p^{-1})$  (where  $N$  is the normal probability distribution). If  $H_1$  is true, the estimations contained in the 8 new business cases (dashed curve) adjusted on the basis of the performance of the first robot, can be considered as a posterior  $\theta | y \sim N(\mu_{post}, \tau_{post}^{-1})$ , where  $y$  is an observation of a known variable which would have yield the learning effects. In this case, since the prior and the posterior follow normal distributions,  $y | \theta$  also follows a normal distribution  $N(\theta, \tau_l^{-1})$  whose parameters we can determine.

We find that the observation  $y=1.04$  would

have been made, with a precision  $\tau_p = 90$ . We plotted the corresponding distribution with a dotted curve in Figure 3. If the change from the solid curve to the dashed curve resulted from regaining control on known variables by reducing the uncertainty affecting these variables, this would mean that the value 1.04 has been drawn when making observations. However, the value 1.04 was very improbable in the prior distribution (the associated p-value is less than 0.002%). Furthermore, 1.04 is far beyond the robot’s performance reached after one year in operation.

Consequently, it is highly improbable that learning on the  $n$  initially known variables is responsible for the change from the solid curve to the dashed curve, which allows us to eliminate  $H_1$ .

This strengthens the hypothesis  $H_{unk}$  according to which “Learning in the unknown” effects have played a role in the dramatic robot’s evolution. And this answers our first research question, confirming that our case is an anomaly with regards to economic performance gains traditionally achieved by reducing uncertainty.

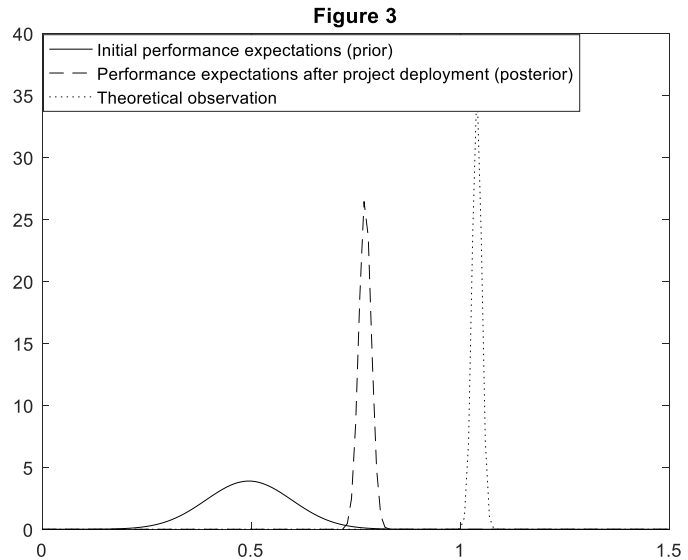
Consequently, in what follows, we attempt to describe the managerial logic which accompanied the decision to invest in this project in the presence of unknown. Extending the “Learning by Doing” framework, we propose the notion of “Learning by Designing”, as a frame for the managerial principles enabling these learning mechanisms to occur (Subpart 4.4). Doing so, we address our second research question, (How can we characterize the managerial approach (and its associated principles in terms of investment decision, economic steering and learning strategy) which resulted in the observed dramatic performance gains?)

#### 4.4 ‘From Learning by Doing’ to ‘Learning by Designing’

In Section 2, we noted that the efficiency of ‘Learning by Doing’ mechanisms rely on managerial learning, with managers being empowered to make decisions on two dimensions (Adler et Clark, 1991 ; Sinclair et al., 2000 ; Carrillo et Gaimon, 2004 ; Budrass et al., 2010):

- (1) Investing in new equipment / in technical improvement / innovation process / in engineering activities (which will trigger new situations)
- (2) Investing in new knowledge, competences, expertise (through trainings, hiring new employees...)

Managerial decisions on these two dimensions induce “Learning by Doing” effects since the introduction of new equipment and process change triggers problems, which call for experimentation and new competences and knowledge acquisition in order to be solved, which trigger learning. An optimal mix in terms of (investment) efforts on these two dimensions yields optimal “Learning by Doing” effects, both “before doing” and “during doing”. The decision of such a mix is the manager’s responsibility. In a “Learning by Doing” frame, the manager’s decision is influenced and determined by performance objectives: one of the plant’s processes is characterized by  $n$  known variables, and the manager’s



decisions are oriented toward the objective to achieve a performance target on some of these variables. This performance is reached progressively, by experimenting on these known  $n$  variables.

“Learning in the unknown” is not covered by the (1) and (2) decisions dimensions. This raises the following question: what kinds of managerial decisions, objectives and principles are “Learning in the unknown effects” related to? The anomalous nature of case study seems to bring about some understanding to that question.

As we noted already, Project 1 was launched, in spite of a negative business case, by managers who firmly believed in its potential for the plant. After having overcome the financial hurdles requiring a positive business case (with a subsidy from the R&T), the Project 1’s managers had one major objective in mind: proving that the robot was effectively valuable for the plant and that it could yield valuable performance. Consequently, the managerial actions during project deployment were oriented toward this very objective. In addition, interviews the managers who carried out Project 1 revealed that at the outset of the project, they were aware that new dimensions would be discovered during the project. Indeed, introducing such a machine in the production environment was a real rupture and, to some extent, a leap in the unknown: the necessary competences were not available within the plant and were not even precisely identifiable at the beginning of the project. The plant had no referent Business Units nor experts regarding automation... Everything needed to be built. Project 1’s managers were aware of this and they knew that beyond the initially identified risks, other unforeseen events were likely to arise. Consequently, they were expecting unknown unknowns to emerge. And they were ready to manage them and find a value-creating solution at the moment of their emergence. More specifically, finding value-creating solutions consisting in designing new variables in the unknown. In sum, managers had set themselves the objective to turn the new dimensions into opportunities at the moment they would emerge. This is what happened when they strategically managed the operators’ interest in the robots, which triggered a valuable “cooperation” between more-skilled operators and the robots.

This example of the operators’ interest in the robots, despite having been strategically managed, has perhaps a slight serendipitous connotation. However, in Project 2, the design of a new variable in terms of savings is all but accidental, since it constitutes the very purpose of the project: this new performance variable (that was *Avoiding costs which would result from an unsuccessful ramp-up*) was imagined at the outset of the project. And the perspective to design it and to turn it into a performance variable guides and structures the project (which is still ongoing today): it is a key element of the project since the outset.

Therefore, these findings lead us to add a third action dimension, including with three associated management principles (a., b. and c.), to the two existing dimensions ((1) and (2)) in the “Learning by Doing” theoretical frame. Whereas decisions (1) and (2) are oriented toward converging to a *performance target*, decision (3) is oriented toward the objective of meeting a *learning target* which implies adding new variables in a given action field by designing in the unknown.

- (1) Investing in physical capital (new equipment, technical improvements...)
- (2) Investing in new knowledge, competences, expertise (through trainings, hiring new employees...)
- (3) Investing in the unknown, i.e. investing in the design of new variables which are “in the unknown” at the outset of the project.

Associated with this decision dimension, we propose the following management principles:

- a. **Regarding the economic decision.** The economic calculation is imperfect, and often unfavourable, because of incomplete information, when it comes to estimate the profitability of an investment in the

unknown. The Net Present Value (involved in business cases) is ill-adapted to fairly assess the value of innovation projects and is widely criticized for that. However, this classical economic tool can all the same have a usefulness in the economic steering of projects in the unknown (even if the nature of this usefulness is not the same as when NPV classically is classically applied). In the unknown, the "negative" or "positive" result of the NPV is not reliable. Consequently, this feature is not the one of the greatest importance for managers and some innovation or improvement projects are launched in spite of a negative NPV. In this case, the risk of not succeeding in turning the project into a profitable one exists. However, the computed NPV also provides managers with a kind of "railing", a kind of "protection" to ensure that the investment is capped and under control in case the project proves indeed non profitable. Thus the first managerial principle that we infer from our case is to use classical (perhaps unfavourable) economic tools as a means to ensure that in case of failure, no investment other than the initial one will be lost.

- b. **Regarding managing project deployment in the unknown and especially regarding building economic profitability in the unknown.** Owning only incomplete information and evidence, managers who invest in the unknown, in spite of an unfavourable business case, seek to collect new knowledge. However, they do not know the nature nor the content of the information they are going to collect. Consequently, they cannot set precise objectives in terms of knowledge to be collected. However, one managerial objective can be clearly set and cascaded to the teams: the objective to discover new performance variables (new states of the world) which were initially unknown and to develop learning related to these new variables in order to find levers that will help either reduce the costs or increase the benefits (and thus make the project profitable). Thus, the second managerial principle that we infer is to clearly set in the team's mission the objective to discover new performance variables. One can note that these new variables can be imagined and contemplated as soon as the outset of the project, as in Project 2. Or, in cases when everything is to be built, such as in Project 1, they can be inspired by actively awaited unknown unknowns during project deployment.
- c. **Regarding organizing learning in the unknown.** Once they are discovered, one needs to acquire further knowledge related to the new states of the world in an attempt to make them effectively contribute to the economic profitability. Acquiring this knowledge requires experimenting. As mentioned above, in the unknown, it is not possible to have clear expectations and to set clear objectives regarding the outcomes of these experimentations. However, the objective to reduce costs and / or increase benefits can be set, in order to turn the project into a profitable one. Thus, the third inferred managerial principle is to deploy on the newly-discovered states of the world a learning strategy that is oriented toward the objective of turning the project into a profitable one.

To summarize these three principles, the key purpose orienting the management of such investments in the unknown is to "design" and bring to life these contemplated variables, in order to structure the unknown and to benefit from new performance dimensions in this newly-structured world. The new variables being imagined (be it at the outset or later on in the project), the very objective of the project is to develop learning strategy enabling to take them out of the unknown and to generate knowledge that will make them turn the project into a profitable one. The project steering will be oriented to that



end. Instead of being seen as a threat, initially missing information are seen as an opportunity which must be unveiled.

We call "Learning by designing" the learning process resulting from the managerial decisions and principles contained in (1), (2) and (3).

## 5. Research implications

Based on a single-case study, the 'Learning by Designing' framework extends the frame of "Learning by Doing", by outlining principles to guide investment decisions (a.), (economically) manage project deployment (b.) and organize learning (c.) in the unknown.

The first principle (a.) brings new insights into the problem of the economic evaluation and economic management of R&D projects. Indeed, similarly to the robot project we describe, many R&D projects find it difficult to prove that they are worth being launched when the classical financial tools, (e.g. Net Present Value (NPV)), are negative. Indeed, as highlighted in Hooze's (2010) literature review, the NPV is ill-adapted to fairly assess the profitability of a project in a context marked by uncertainty and incomplete information and has consequently been widely criticized for that (Barger, 1993 ; Phaal, 2005 ; Hartmann et al., 2006). Among other things, the NPV computation assumes that the project is financed up to completion, which introduces very little flexibility and makes the investment decision irreversible. Besides risks and uncertainties are summarized in one figure, the discount rate, ignoring the economic impact of more qualitative aspects. It namely ignores the impacts of learning effects and the potential spillovers that could be beneficial to subsequent projects. All these imperfections and approximations can lead the NPV to kill promising innovation projects. This is the reason why some research works propose new forms of NPV (stochastic NPV...) or even new tools (real options...) that value innovation projects more fairly.

Our case is also based on the observation that the NPV does not fairly assess promising projects. But instead of transforming the NPV into a better tool, our case suggests an original (and even diverted) use the very 'basic' NPV.

The economic management principle we identify does not rely on reaching (more or less artificially) a positive NPV to authorize project launch. If it is positive, it is of course better. However, in case the NPV is negative, the second and third management principles (b. and c.) of the 'Learning by Designing' frame recommend to imagine and design new performance variables and get new insights related to these latter, by experimenting, in order to make them contribute to the economic performance.

Secondly, our case study epitomizes the thesis according to which the unknown can be managed, namely economically. Be it in the presence of uncertainties or unknown-unknowns, the issue in both cases is that some information and evidence are missing. This calls for new knowledge to be developed (knowledge whose nature differs depending on if we manage basic uncertainties of unknown-unknowns). Yet, in the same way as uncertainty is managed in a philosophy of "reduction", most of the approaches that propose ways to manage the unknown aim at reducing and eliminating the unknown. For instance, as mentioned above Feduzi and Runde's (2014) algorithm which generates knowledge related to new states of the world, aims at eliminating unknown-unknowns before encountering them later on. In other words, the unknown is in general negatively perceived (Gillier et Lenfle, 2018).

However, Gillier and Lenfle (2018) detect in the case of the Manhattan project a paradigm which implies identifying what is unknown and designing initially unknown things. In other words, they identified an expansive approach which consists in designing in the unknown, instead of seeing the unknown as an undesirable threat. Our anomalous case turns out to be an additional example which fits into this expansionist paradigm. The managerial principles inferred from the case study encourage to adopt such an expansionist approach: the second principle (b.) encourages managers to clearly set the goal to develop new states of the world and the third one (c.) recommends to organize and deploy a learning strategy related the newly discovered states of the world, by centring this strategy on the very objective to build economic profitability. Thus, our findings contribute to the research related to the Management of the Unknown, by bringing to light a case whose performance gains are attributable to learning in the unknown effects that were made possible by the deployment of three managerial principles. Falling within the research avenue pointed out by Gillier and Lenfle (2018) and as an additional proof to the fact that the unknown can be managed, this paper encourages to further explore the logics related to deploying projects in the unknown.

## 6. Practical implications

The managerial principles identified in the paper represent for firms the possibility to formally justify the launch of projects which, under classical tools and classical management rules, should not be launched. Our case illustrates that an initially unfavourable project can, if launched, represent the opportunity for a firm to allow the occurrence of initially impossible scenarii (dramatic performance gains and an eventually profitable project in our case).

The *Learning by designing* frame does not reject traditional economic tools, but diverts their use, by not paying so much attention to their binary "positive" versus "negative" predictions, but using them as a protection to control costs.

The financial sponsors of a project justified by the first identified principle (a.) are not asked to fund it in a "Let's see what happens" philosophy: the important idea is that they are asked to pay in order to make possible the steered discovery of new action variables which are likely to open new (extended) action fields, and make initially impossible scenarii become possible.

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## Résumé

Il est largement reconnu que plus un projet est incertain, plus la probabilité de le rendre rentable est faible. Dans ce cas, comment expliquer que chez Airbus, un cluster d'innovation possède un portefeuille de projets qui sélectionne des projets extrêmement incertains et fait preuve d'une rentabilité remarquable (taux de rentabilité x6) ? Selon les techniques d'évaluation économique traditionnelles basées sur la théorie de la décision qui quantifient la valeur de la réduction de l'incertitude, il est très peu probable qu'un mécanisme de réduction de l'incertitude génère une valeur " considérable ". Donc, si nous restons dans le cadre de la théorie de la décision, la rentabilité du portefeuille de projets du pôle d'innovation d'Airbus est due soit à la chance (c'est-à-dire au jeu), soit à un mécanisme de réduction de l'incertitude exceptionnellement efficace. Mais si nous passons de la théorie de la décision à la théorie de la conception, une autre explication peut être envisagée : l'exploration de l'inconnu. Alors que l'inconnu a longtemps été considéré comme hors de portée des techniques de décision, des travaux récents ont révélé que la théorie de la conception étend la théorie de la décision à l'inconnu. Appliqués aux décisions d'investissement, ces modèles de décision basés sur la théorie de la conception prédisent que dans l'inconnu, les décideurs peuvent toujours concevoir un espace décisionnel permettant de générer (beaucoup) de rentabilité. Cette prédiction semble absurde. Mais en se basant sur le cas Airbus, cet article spécifie un modèle de décision d'investissement réaliste basé sur la théorie de la conception qui permet de générer des rendements spectaculaires dans l'inconnu, et explique le cas Airbus.

### **Why is investing in the unknown less risky and more profitable than investing under uncertainty?**

#### **Model and empirical evidence**

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## Abstract

It is widely acknowledged that the more uncertain a project is, the lower the probability of making it profitable. But how then can we explain that in Airbus, an innovation cluster's project portfolio selects highly uncertain projects and demonstrates a remarkable profitability (x6 rate of return)? According to traditional decision-theory-based economic evaluation techniques which quantify the value of uncertainty reduction, it is very unlikely that an uncertainty reduction mechanism generates 'considerable' value. So if we remain under decision theory, the profitability of Airbus innovation cluster's project portfolio is either due to chance (i.e. gambling) or to an outstandingly effective uncertainty reduction mechanism. But if we move from decision theory to design theory, another explanation can be considered: unknown exploration. While the unknown was long considered as out-of-reach for decision-making techniques, recent works have revealed that design theory extends decision theory to the unknown. Applied to investment decisions, these design-theory-based decision models predict that in the unknown, decision-makers can always design a decisional space allowing the generation of (a lot of) profitability. This prediction seems absurd. But based on the Airbus case, this paper specifies a realistic design-theory-based investment decision model which accounts for the generation of dramatic returns in the unknown, and explains Airbus case.

**Keywords:** investment decision; uncertainty reduction; unknown exploration; radical innovation; performance; profitability

## 1 Introduction

In Airbus, an innovation cluster's portfolio of incubation and acceleration projects selects highly uncertain projects which have been rejected by the economic evaluation criteria (NPV, pay-back time, etc.) in force in the traditional organizations (Engineering, Manufacturing, Customer Services, R&T, etc.): in terms of time, cost, operational performance, etc., these projects are found to be far too risky. The innovation cluster decides to fund some of these rejected projects that are due to be all but profitable, and guides their incubation or acceleration. And it demonstrates a dramatic profitability rate: in 2018, for 1 euro invested in the innovation cluster project portfolio, 6 euros have been given back to Airbus. This rate of return is extremely puzzling, because the traditional economic evaluation techniques deriving from decision theory (Wald, 1950 ; Savage; 1954 ; Raïffa, 1968) are not known for predicting or explaining the generation of considerable economic value (apart from the eventual dramatic outcomes of a gamble). Indeed, they quantify the value of uncertainty reduction, that is the value that a decision-maker can expect to earn if he or she effectively reduces uncertainty during project run, which is generally moderate, of the order of a variance reduction, noise reduction. Thus, it seems impossible that decision-theory-based economic evaluation techniques and associated investment decision models account for the 'x6' rate of return observed in the case of Airbus, unless we are in the face of the outcomes of a gamble or in the face of an outstandingly effective mechanism of uncertainty reduction. If we remain in the decision-theoretic framework, (i) chance or (ii) outstandingly effective uncertainty reduction are the two sole explanations for the dramatic profitability of the innovation cluster's portfolio.

If we leave the decision-theory-based framework and move to design theory, we can consider another mechanism which is not accounted for by decision theory and which could explain the profitability of the innovation cluster's portfolio: unknown exploration<sup>54</sup>. Contrary to basic uncertainty which calls for being reduced and eliminated, by becoming more knowledgeable about the states of the world of a given states-space, the unknown involves "voids" that call for being filled, i.e. that call for building the missing states of the world (Feduzi et Runde, 2014) and designing and shaping the states-space (Le Masson et Weil, 2020). It is well-proved that decision theory and deriving techniques are unsuitable (if not misleading) to explore the unknown (Loch et al., 2006). And the unknown has long been considered as "out of reach" for any decision-making techniques. However, recent advances in design theory have made it possible to extend decision theory

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<sup>54</sup> In this paper, the term 'uncertainty' designates 'basic uncertainty', i.e. initially identified, but uncertainly known states of the world, in a given, fixed-size states-space. And the 'unknown' refers to 'initially missing, ignored states of the world that are to emerge or to be created in the states-space': this includes unforeseeable uncertainties, also called unknown unknowns (Sommer et Loch, 2004 ; Sommer et al., 2009) and 'unimagined possibilities', unpredictable events (Faulkner et al., 2017 ; Feduzi et Runde, 2014).

to the unknown with a design-theory-based decision model accounting for the possibility to enrich a decisional space with the design of new decisions and new states of the world in the unknown (Le Masson et al., 2018): so far, this design-theory-based decision model is theoretic and formal. It does not yet include associated techniques which would explicitly quantify the value of unknown exploration mechanisms, in the same fashion as decision theory quantifies uncertainty reduction. But it already suggests that when one considers an investment decision in the unknown, one can always design the states of the world or the decisional alternatives which render the decision to invest profitable. This prediction departs from decision theory according to which the more uncertainty a project is, the lower the expected profitability and the riskier the decision to invest. Literally taken, this is extremely counterintuitive, if not absurd. Indeed, it suggests that in the unknown, every situation could be turned into a profitable opportunity. But at the same time, a decision-making model which can account for the generation of a lot of profitability is particularly interesting with respect to the puzzling case of Airbus innovation cluster. It could offer a new explanatory way. Thus, this paper aims at investigating whether one could specify a realistic and reasonable design-theory-based investment decision model, which could account for the generation of dramatic returns in the unknown, and which could explain Airbus case.

This paper will address these two questions together: could the formal decision-making model offered by design theory provide an explanatory framework for the dramatic and puzzling profitability of Airbus innovation cluster's portfolio of incubation and acceleration projects? And the other way around, could the case of Airbus innovation cluster's project portfolio provide empirical evidence that would confirm the (counterintuitive) prediction of the design-theory based decision model regarding the behavior of profitability in the unknown? In addition, could the case of Airbus innovation cluster's project portfolio provide new insights regarding possible investment decision techniques / tools that could be part of a design-theory-based decision model?

To this end, this paper is structured as follows. Section 2 reviews the literature which claims to address investment decision-making questions under high uncertainty or the unknown. The objectives are to characterize more precisely the situation of investment decision-making in the unknown, to identify the models that are proposed to help decision-makers manage such a situation, to identify whether these models tend to be decision-theory or design theory-based, and to identify whether or not they quantify the expected profitability associated with the exploration of the unknown. This will lead us to trace the main outlines of what a design-theory-based investment decision model in the unknown could be. Section 3 describes our twofold method: firstly, a statistical method to test whether the case of Airbus innovation cluster falls into the decision-theoretic or design-theoretic framework, and secondly, an empirical method (single-case study) to analyze the economic management techniques and methods enforced in the frame of this unusual portfolio, in an attempt to get insights regarding the very practices that can possibly enable to generate (a lot of) profitability in the unknown. Finally, Sections 4 and 5 respectively describe and discuss our results.

In what follows, AIC will refer to Airbus innovation cluster.

## 2 Literature review: investment decision models in the unknown

The objective of this section is to characterize more precisely the situation of investment decision-making in the unknown, to identify the models that are proposed to help decision-makers manage such a situation and to identify whether these models tend to be decision-theory or design theory-based, and whether or not they quantify the expected profitability associated with the unknown.

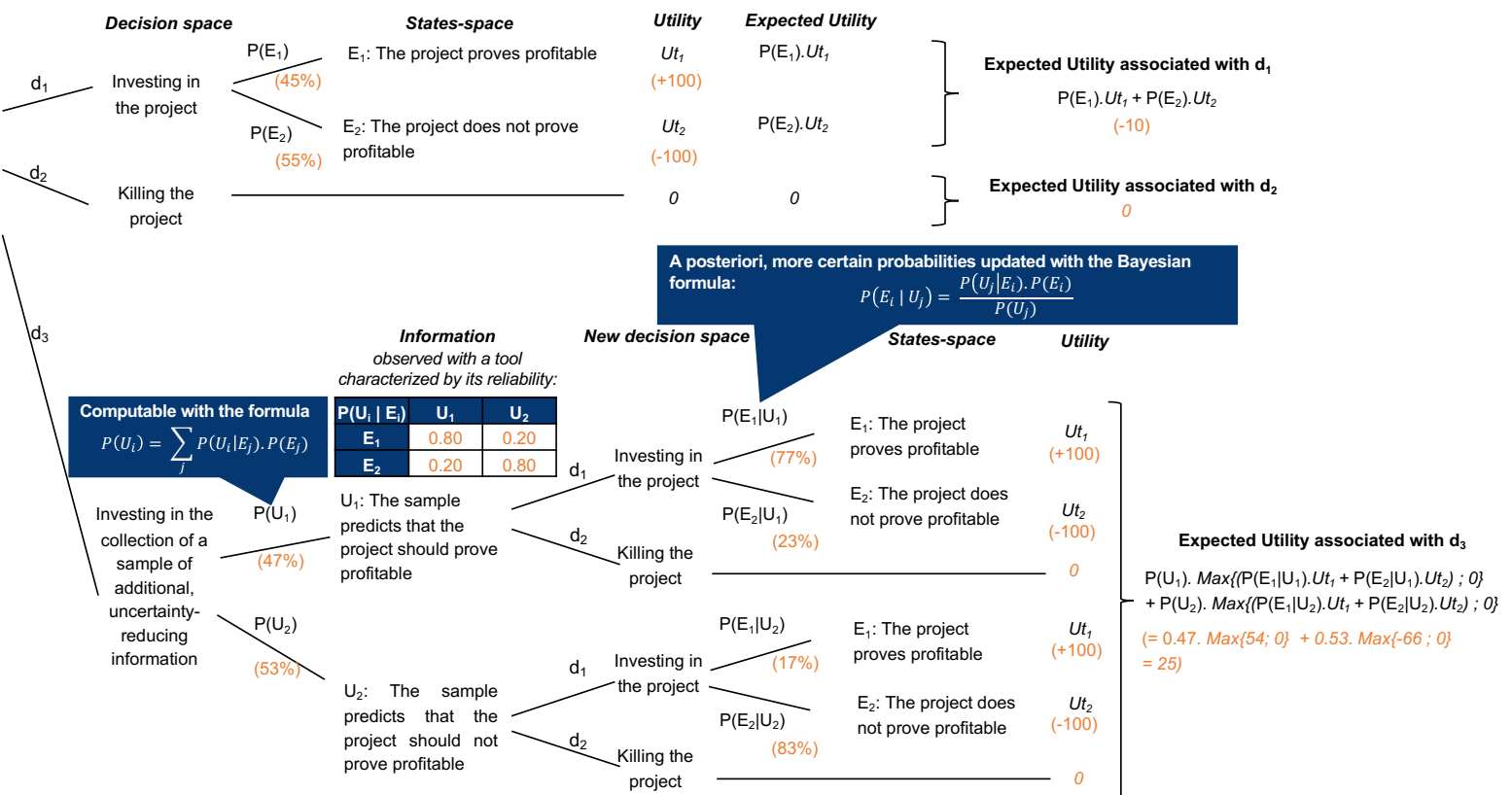
### *2.1 From a situation of investment decision-making under uncertainty to a situation of investment decision-making in the unknown*

#### 2.1.1 Investment decision-making under uncertainty

In order to help managers decide on whether or not to invest in projects the target of which is uncertain (uncertain target market, uncertain technical feasibility, uncertain costs to be incurred, uncertain potential benefits, etc.), the traditional profitability analysis tools (Return On Investment, Payback time, Discounted Cash Flow, etc.) need to be adapted and enriched.

In this respect, investment decision-making techniques (decision trees ; real option analysis ; stochastic NPV...) deriving from Decision Theory (Wald, 1950 ; Savage; 1954 ; Raïffa, 1968) propose to integrate uncertainty in the economic reasoning by using the probabilities of the events that are likely to arise: thanks to a probabilistic description of the uncertainty, these investment techniques pinpoint the investment choice that maximizes the decision-maker's expected utility (e.g. expected profitability). As illustrated by the decisional way  $d_3$  represented in Figure 1 below, decision theory-based investment decision-making considers the possibility of collecting additional information that will reduce uncertainty. If the gain of utility associated with the perspective of collecting additional information is large enough, decision-theory recommends to follow that way. The equations underlying this mechanism of uncertainty reduction come from Bayesian statistics.





**Figure 1. The formal framework associated with a decision-theory-based economic reasoning under uncertainty**

$d_3$  is the decisional way that considers the possibility to reduce uncertainty through the collection of new information, with the use of an observation tool characterized by its reliability  $P(U_i | E_i)$ .  $P(E_1)$  and  $P(E_2)$  being given, formula coming from the Bayesian statistics enable to compute the probabilities of the observations  $P(U_i)$ , and *a posteriori*, more certain probabilities  $P(E_i | U_i)$ , which incorporate the reduction of uncertainty. Choosing  $d_3$  has a cost which we do not explicitly represent in this Figure. But uncertainty reduction also offers value which can be assessed by computing the expected utility associated with  $d_3$ . In orange, we propose an example with values describing what a typical investment situation under uncertainty could be. If we do not consider the possibility of becoming more knowledgeable regarding the two states of the world by reducing uncertainty (i.e. if we do not consider  $d_3$ ), the best decisional way is  $d_2$  (killing the project – with its expected utility  $0 > -10$ ). With respect to this ‘best’ decisional way,  $d_3$  offers an additional gain of expected utility of +25. Therefore, a decision-theory-based reasoning would recommend to reduce uncertainty in our example (provided the decision-maker is equipped with the organizational and managerial tools, capabilities, processes... necessary to effectively reduce uncertainty).

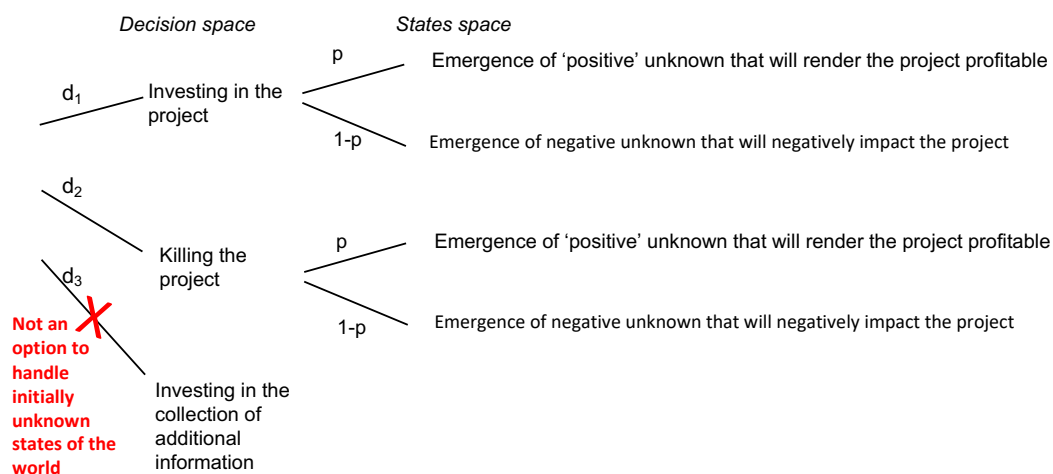
### 2.1.2 Investment decision-making in the unknown

Unfortunately, the Bayesian logic described in the previous subsection no longer works when the level of uncertainty is so high that uncertainty does not only consist in *uncertainly-known states of the world in a given states-space* (i.e. *basic uncertainty*), but also involves initially *ignored or missing states of the world* (i.e. *the unknown*). The reason is that the uncertainty reduction logic, which relies on a Bayesian update of prior knowledge, thanks to the collection of new information is unsuitable in the face of initially unknown states of the world. Indeed, one owns no prior knowledge regarding the initially unknown states of the world (these latter are not characterizable in terms of probabilities – one knows neither the state of the world nor its probability). So in the unknown, there is nothing to update and there is no value to earn by collecting uncertainty-reducing information. Consequently,  $d_3$  is no longer an option to the decision-maker.

Thus, a decision-maker who is ‘consciously’ considering the possibility to invest in a project in the unknown (i.e. a project the profitability of which cannot be fairly assessed, and which is likely to be impacted by unforeseeable events) is aware that if he or she invests, initially unknown states of the world are to unexpectedly arise, disturb the predictions of decision theory, and by implication disturb the plans that he or she may have initially made according to them. Such disturbances may translate into unwelcome developments that destabilize and ruin initial plans. But theoretically, anything can occur in the unknown, and the unpredictable events might also offer welcome opportunities (e.g. Zeckhauser, 2006 ; Roy et Zeckhauser, 2015 ; Lechler et al., 2012). In this context, the decision-maker who has to decide on whether or not to invest in a project in the unknown faces a decision situation involving the two following possible decisional paths:

- Either  $d_1$ : killing the project, considering that he does not want to run the risk of having his plans ruined by the emergence of unwelcome initially unknown events.
- Or  $d_2$ : investing in the project, in the hope that the unknown events that will arise will fill the very conditions that are required to render the project profitable.

This decision situation is depicted the decision tree in Figure 2.



**Figure 2. Decision situation faced by a decision-maker in the unknown**

The decisional situation represented in Figure 1 calls for two comments:

- (1) Following the first decisional path boils down to gambling on the occurrence of welcome developments that could be brought by the unknown, the probability  $p$  of which is very low, but non-zero. Investors engage in this way if their intuition predominates over the recommendations of decision-theory. This reasoning is intuition/heuristic-based, but not necessarily deprived of cognition and rationality, especially if the rational underlying it involves the plan of investing in numerous projects in the unknown, knowing that many of them will fail (and generate losses), but that one of them might by chance turn out to be extraordinarily profitable and pay off for all the initial investments and the losses due to the failed projects – this is a profitability-oriented reasoning used by many angel investors (Huang et Pearce, 2015 ; Zeckhauser, 2006)
- (2) Following the second decisional path consists in following the rational and wise recommendations of decision theory, somehow applied with the scant available information regarding the project. However, this recommendation can be misleading and dangerously biasing, because it represents the risk of killing promising projects (e.g. Alkaraan et Northcott, 2006).

While the first decision path suits 0%-risk averse investors (i.e. gamblers), the second one suits 100%-risk averse investors. But this decisional situation is extremely unsatisfactory for decision-makers who are neither willing to run the risk of killing promising projects under the pretext that they are prone to be affected by the unknown, nor willing to engage in an extremely risky gamble. An examination of the literature for alternative investment decisions models which could guide decision-makers who are neither 0%-risk averse investors nor 100%-risk averse investors has led us to distinguish between two kinds of models:

- The first kind of models puts aside the ‘profitability criterion’ and attempts to assess the potential of projects affected by the unknown with other criteria (strategic value, qualitative tools, quasi-quantitative tools...). Such criteria are supposed to help choose the best alternative between  $d_1$  and  $d_2$ , but considering a state space characterized by something other than profitability. We review the literature related to these methods in subsection 2.2.1
- The second kind of models consists in building on the fact that decision-theory is unsuitable in the unknown (Loch et al., 2006)) and on the result that Design Theory extends decision theory to the unknown (Le Masson et al., 2018). We review the literature related to this second approach in subsection 2.2.2

## *2.2 Alternative models to ‘quantified uncertainty-reduction’, which could help handle investment decision-making in the unknown*

### 2.1.1 Model 1: informing the decision with criteria other than the profitability criterion

According to Schmidt et al. (2009), one should use different sets of evaluation criteria depending on whether one assesses incremental innovation or radical innovation projects – i.e. specific evaluation criteria should be dedicated to highly innovative projects. Numerous research works try to characterize what such evaluation frameworks could be, in terms of the content, of implementation processes... (e.g. Dziallas, 2020 ; Martinsuo et Poskela, 2011 ; Hart et al., 2003...). The criteria that these very studies find to be the most appropriate to fairly value innovative projects in their early stage, when the degree of uncertainty is high (and possibly reaches the unknown<sup>55</sup>) are non-monetary: e.g. strategic fit, customer relevance, communication potential, competitive potential, future business potential, etc.<sup>56</sup> Although they do not incorporate profitability-quantified criteria, these evaluation frameworks do not mean to fully give up profitability ambitions – the application of these criteria is expected to select projects that will open new business opportunities, attract new customers... for the future, and thus will pay off later on (Shenhar et al., 2001 ; Frederiksen et Knudsen, 2017). In this context, economic-oriented criteria are to be reintroduced later on when the project is more mature (e.g. in the later phases of a stage-gate process (Hart et al., 2003 ; Dziallas, 2020)).

Such an economic reasoning seems to remain in a decision-theoretic paradigm. It consists in circumventing the ‘profitability criterion’ in order to render decision-theoretic logics applicable again, on other more qualitative aspects.

The information that feed such evaluation frameworks are collected through more or less formal means (from analysis grids to conversational mode) and may include perception-based assessments (Shenhar et al., 2001), expert evaluation (Chang et al., 2008)... Some works advocate for the establishment of informal assessment systems (Koen et al., 2001 ; Nobelius et Trygg, 2002 ; Martinsuo et Poskela, 2011), arguing that formal systems can represent a threat for creative and innovative ideas. The mission of these frameworks is to offer a strategic view (Eling et al., 2014), offer transparency in the decision-making (provide the necessary elements for understanding, beyond using accurate figures), and allow quick and flexible decision-making. In this regard, Dziallas (2020) advocates for lightened (more flexible and faster) resources allocation procedures (arguing that the delays of ‘traditional’ budget allocation processes may be long and may threaten innovation opportunities).

In sum, such an economic reasoning is associated with a *very lightened economic management* approach. It is primarily concerned by the fact that the profitability requirements and the processes (e.g. resource allocation processes) that are enforced in the “exploitation regime” of a firm are a threat to creative and innovative

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<sup>55</sup> Although, to our best knowledge, these works do not explicitly refer to the unknown

<sup>56</sup> Hart et al. (2003) and Martinsuo and Poskela (2011) suggest that the specific content of the set of criteria may vary depending on the strategic objectives of the firm (no one-size-fits-all set of criteria).

projects, i.e. to exploratory activities. In reaction to this, the above-mentioned works advocate for making investment decisions based on a project evaluation system which is well-separated from the one enforced for the management of mature, exploitative activities. In that, these alternative investment decision models can be seen as *ambidexterity-oriented* (in the sense of March's (1991) definition of ambidexterity). While this reasoning efficiently mitigates the risk of killing promising innovation projects in their early phase, it does not guarantee that the projects will not be killed in later phases, when the profitability criteria are re-enforced (i.e. it does not guarantee that these very selected projects will be eventually reintegrated into the exploitation entities of the firm later on). In that regard, recent works show that such a move from exploration to exploitation is all but automatic, and that having an impermeable frontier to manage the objectives of exploration on the one side, and that of exploitation activities on the other side may actually be a threat to innovation (Le Glatin, 2018 ; Le Glatin et al., 2018).

Table 1 below summarizes the characteristics of this first kind of model (Model 1 column) and compares it with the model of 'uncertainty-reduction assessment' that we described in subsection 2.1 and that is no longer applicable in the unknown (Model 0 column).

	<b>Model 0: Investment decision model based on decision theory</b>	<b>Model 1: Investment decision model based on lightened economic criteria and enriched with additional decision criteria</b>
<b>Nature of the decision criteria</b>	<ul style="list-style-type: none"> <li>Expected utility (or expected profitability, expected Net Present Value, etc.)</li> <li>Value of uncertainty reduction, i.e. the gain of utility the decision-maker can hope to earn if he or she chooses to collect additional information in order to reduce uncertainty (i.e. to follow the decisional way <math>d_3</math>)</li> </ul>	<ul style="list-style-type: none"> <li>Non monetary criteria, distinct from the investment criteria that are used in traditional project management</li> </ul>
<b>Theoretical basis of the reasoning</b>	Decision theory	Decision theory
<b>(Theoretical) Perspective of making profits</b>	The perspective of making profits theoretically depends on: <ul style="list-style-type: none"> <li>- The value of the uncertainty contained in the decisional situation</li> <li>- And on the reliability of the observation tool</li> </ul>	The perspective of making profits cannot be initially quantified since the decision is based on non-monetary considerations.
<b>Necessary conditions to be met in order to effectively generate profit</b>	The possibility of effectively making profits depends on the decision-maker's capacity to manage and organize uncertainty reduction (with the appropriate tools, processes, methods, capabilities, etc.)	The possibility of making profits will depend of whether or not the project passes the later phases of the stage-stage process and is indeed reintroduced in the 'exploitation-oriented' entities of the firm. This cannot be guaranteed by a systematic rule. In other words, the perspective of making profits is very aleatory. (the risk of having the project killed in the later phases of the stage-gate process is even quite high (Le Glatin, 2018 ; Le Glatin et al., 2019))
<b>Management of uncertainty</b>	Quantified uncertainty reduction: The uncertainty is very rigorously managed and controlled, with a quantified technique	A very lightened economic management. The quantified economic approach of Model 0 is put aside
<b>Management of the unknown</b>	The unknown is out of reach for these models	These models propose to handle the selection of highly innovative projects. These projects might contain unknown, but might also only contain a very high level of 'basic uncertainty' – the abovementioned works do not specify. To some extent, they treat the uncertainty and the unknown indistinctly. If the handled projects indeed contained unknown, these models treat the unknown by circumventing it thanks to the use of non-monetary decision criteria: these criteria enable to bring back the decisional situation to a situation where decision theory applies again

**Table 1. Introduction of Model 1 – A restauration of the applicability of Model 0 in the unknown, but with a lightened economic management approach**

A major drawback of Model 1 and its lightened economic management is the risk of wasting resources on projects that will be killed later on.

In the next subsection, we turn our attention to a second alternative model, which, instead of proposing to put aside the monetary criteria when dealing with investment decisions in the unknown, proposes to put aside the decision-theory-based framework and move to a design-theory-based paradigm.

### 2.1.2 Model 2: leaving the decision-theory based reasoning and moving to a design-theory based reasoning

As mentioned in Section 1, decision theory is not suitable to handle the unknown because it only covers the update of one's knowledge related to a fixed-size states-space and it does not manipulate newly-emerging states of the world, i.e. the enrichment of the states-space with new dimensions (Loch et al., 2006 ; Loch et al., 2008). Decision-making in the unknown calls for re-designing, reshaping the states-space, by hypothesizing and assessing possible additional states of the world (Feduzi et Runde, 2014) or by designing new decisional alternatives or new states of the world (Le Masson et al., 2018). Recent progress in design theory can extend decision theory to the unknown (Le Masson et al., 2018): moving from a decision-theory-based reasoning to a design-based reasoning when it comes to make decisions in the unknown means that a decision problem in the unknown no longer consists in identifying the best choice among a set of possible alternatives, but in designing a new, better decision space, either by designing new decisions  $d_i$  (better than the already existing ones) or designing new states of the world (that will lead to reconsider the preference ranking of the decisions that can be possibly taken). In the case of the decisional situation represented in Figure 3, this means that the investment decision problem no longer consists in choosing between  $d_1$  and  $d_2$ , but in designing new states of the world, and thus, a new decision space within which  $d_1$  (i.e. investing) will be the best (rational) alternative. Under a design perspective, the objectives of the “decision-space-design problem” can be phrased in a profitability-oriented fashion (i.e. objective = “designing new states of the world such that in the new decision-space,  $d_1$  is the best decisional path in terms of profitability”). This suggests that theoretically, in the frame of an investment in the unknown, it is always possible to reach one's profitability target without necessarily throwing one's lot with chance, if one designs the judicious states of the world or the judicious decisions to this end.

This design-theory-based prediction is a complete break with the traditional decision-theory-based view of profitability under uncertainty, i.e. with the idea that risk goes up and expected profitability collapses as the degree of uncertainty increases. This prediction is extremely counterintuitive in the first instance. A literal interpretation of it suggests an investment decision model where any investment project in the unknown can be made profitable by anyone who uses a design reasoning. While such a literal interpretation is absurd and unrealistic, we can envisage a sounder investment decision model in the unknown, where any investment could be made profitable, albeit subject to two conditions:

- (iii) the investment is allocated to a project which is suitable for the design of new decisional alternatives or new states of the world, i.e. to a project which contains unknown. This raises the question of the decision maker's capacity to detect the unknown. In this respect, research works

dedicated to the design of value in the unknown highlight the importance of discussing the sources of the unknown, which need to be supported and managed with appropriate tools (Hooge, 2020 ; Hooge et Stasia, 2016)

- (iv) the decision-maker or other actors in charge of developing the project own the indispensable methods, tools, capabilities, processes, organization... to successfully design new states of the world or new decisions. This raises the question of the suitable techniques to explore the unknown. In this respect, works dedicated to the questions of building and managing value in the unknown (Hooge, 2020 ; Hooge et Stasia, 2016 ; Gillier et al., 2014), experimenting in the unknown (Gillier et Lenfle, 2018), designing generic technologies in the unknown (Hooge et al., 2016), etc. stress the importance of being capable to rigorously manage unknown exploration, thanks to dedicated organizations, managerial approaches, tools, evaluation frameworks

Enriched with these two conditions, such an investment decision model seems more realistic. A parallel can be made with the entrepreneurial literature and the logic of opportunity creation which refers to a pursuit of a kind of entrepreneurial opportunities that are not ‘already existing and to be identified and exploited’ by entrepreneurs, but which entrepreneurs are going to create and develop through their own actions (e.g. Sarasvathy, 2007). Considered alone, i.e. literally interpreted, this theoretical concept could suggest that one can always create an interesting entrepreneurial opportunity from any situation, which would be absurd. But there exist works (Welter et al., 2016) which propose to associate the theoretical concept of opportunity creation with an action framework, by investigating and building the behavioral and cognitive models that reflect why and how some people create entrepreneurial opportunities. This boils down to attempting to specify the assumptions that underlie the logic of opportunity creation and define the boundary conditions for its existence. Similarly, (i) and (ii) could be considered as the assumptions that underlie a design-theory-based investment decision model.

A decision-theory which predicts that one can aspire to make a lot of profits out of the unknown without having to gamble, provided one is capable of detecting the projects that contain unknown and provided one is capable of managing unknown exploration seems to intellectually make sense. So in this paper, we would like to test such a model, i.e. to look for empirical evidence. In order to make this model testable, we rephrase the conditions (i) and (ii) into assumptions (A1) and (A2): we assume that the possibility of effectively making profits in the unknown relies on the decision-maker’s capacity:

- (A1) To establish a capability that systematically diagnoses, detects, identifies the project that contain unknown
- (A2) To deploy capabilities, methodologies and managerial approaches enabling to rigorously explore and structure the unknown

In addition to the hypothetical characteristics described by (A1) and (A2), we can contemplate a decisional model in the unknown which

- (A3) Would offer a quantified technique to assess the value of unknown exploration as rigorously as decision-theory based investment decision models assess the value of uncertainty reduction

The last column of Table 2 summarizes the definition of this theoretical investment decision model in the unknown (which we call Model 2) and the assumptions it embeds.

	<b>Model 0: Investment decision model based on decision theory</b>	<b>Model 1: Investment decision model based on lightened economic criteria and enriched with new criteria</b>	<b>Model 2: Investment decision model based on the design of decisions in the unknown</b>
<b>Nature of the decision criteria</b>	<ul style="list-style-type: none"> <li>Expected utility (or expected profitability, expected Net Present Value, etc.)</li> <li>Value of uncertainty reduction, i.e. the gain of utility the decision-maker can hope to earn if he or she chooses to collect additional information in order to reduce uncertainty (i.e. to follow the decisional way <math>d_3</math>)</li> </ul>	Non monetary criteria, distinct from the investment criteria that are used in traditional project management	<p>The decision criteria include profitability. The set of criteria does not need to be different from the one used to assess less innovative, more conventional projects: Expected utility (or expected profitability, expected Net Present Value, etc.) &amp; value of uncertainty reduction, as in Model 0</p> <p><b>In addition: Value of unknown exploration (A3)</b></p>
<b>Theoretical basis of the reasoning</b>	Decision theory	Decision theory	Design theory
<b>(Theoretical) Perspective of making profits</b>	<p>The perspective of making profits theoretically depends on:</p> <ul style="list-style-type: none"> <li>The value of the uncertainty contained in the decisional situation</li> <li>And on the reliability of the observation tool</li> </ul>	The perspective of making profits cannot be initially quantified since the decision is based on non-monetary considerations.	<p>Design theory prediction:</p> <p>Theoretically, in the frame of an investment in the unknown, it is always possible to reach one's profitability target, provided one designs the suitable states of the world or the suitable decisions to this end. In other words, it seems theoretically possible to aspire for making a lot of profitability in the unknown, without necessarily throwing one's lot with chance.</p>
<b>Necessary conditions to be met in order to effectively generate profit</b>	The possibility of effectively making profits depends on the decision-maker's capacity to manage and organize uncertainty reduction (with the appropriate tools, processes, methods, capabilities, etc.)	The possibility of making profits will depend of whether or not the project passes the later phases of the stage-stage process and is indeed reintroduced in the 'exploitation-oriented' entities of the firm. This cannot be guaranteed by a systematic rule. In other words, the perspective of making profits is very aleatory. (the risk of having the project killed in the later phases of the stage-gate process is even quite high (Le Glatin, 2018 ; Le Glatin et al., 2019))	<p><b>The possibility of effectively making profits depends on the decision-maker's capacity to</b></p> <ul style="list-style-type: none"> <li><b>(A1) To establish a capability that systematically diagnoses, detects, identifies the project that contain unknown</b></li> <li><b>(A2) To deploy capabilities, methodologies and managerial approaches enabling to rigorously explore and structure the unknown</b></li> </ul>
<b>Management of uncertainty</b>	Quantified uncertainty reduction: The uncertainty is very rigorously managed and controlled, with a quantified technique	A very lightened economic management. The quantified economic approach of Model 0 is put aside	<p><b>The uncertainty is rigorously managed and controlled, with a quantified technique (which are the same as in model 0*) (A3')</b></p> <p>* Model 2 is supposed to be an extension of Model 0, and thus keep the properties of Model 0 under uncertainty (Le Masson et al., 2018)</p>
<b>Management of the unknown</b>	The unknown is out of reach for these models	<p>These models propose to handle the selection of highly innovative projects. These projects might contain unknown, but might also only contain a very high level of 'basic uncertainty' – the abovementioned works do not specify. To some extent, they treat the uncertainty and the unknown indistinctly.</p> <p>If the handled projects indeed contained unknown, these models treat the unknown by circumventing it thanks to the use of non-monetary decision criteria: these criteria enable to bring back the decisional situation to a situation where decision theory applies again</p>	<p><b>The unknown is rigorously managed and controlled, with a quantified technique (A3)</b></p> <p>(that enables to assess the value of unknown exploration as rigorously as model 0 enables to assess the value of uncertainty reduction)</p>

**Table 2: Introduction of Model 2, an extension of Model 0 to the unknown?**

Provided the assumptions underlying Model 2 are met, it theoretically promises a way to yield systematic returns from investments in the unknown. In comparison with Model 1 (subsection 2.2.1, recalled in Table 2 below) which selects projects with a lightened economic management approach and which does not guarantee that the highly uncertain selected projects will not be killed in the later phases of the stage gate process, Model 2 is very attractive. However, at this stage, this is only theoretical. Therefore, the remainder of this article is dedicated to look for potential empirical evidence for this model.



### 2.3 Research questions

Our literature review has led us to draw the outlines of a theoretical investment decision model (Model 2, defined in the last column of Table 1) which predicts that one can aspire to make a lot of profits out of the unknown without having to gamble, subject to the decision-maker's capacity:

- (A1) To establish a capability that systematically diagnoses, detects, identifies the projects that contain unknown
- (A2) To deploy capabilities, methodologies and managerial approaches enabling to rigorously explore and structure the unknown

Besides, we have made the assumption that:

- (A3) Model 2 could also incorporate a quantified technique to rigorously assess the value of unknown exploration, in the same fashion as decision-theory based investment decision models rigorously assess the value of uncertainty reduction

In what follows, we would like to investigate whether we could find some empirical evidence for Model 2. With its remarkable profits, Airbus AIC's portfolio of incubation and acceleration projects (briefly presented in Section 1) seems to be a good candidate to provide empirical evidence, because it contradicts the widely acknowledged idea that the more uncertain a project is, the lower the probability of making it profitable. More specifically:

- (i) The AIC claims to select projects that address difficult, highly complex and often long-lasting issues: these projects are perceived as very risky and undesirable economically-speaking (incomputable profitability assessment or "negative" profitability assessment). The traditional organizations of Airbus have chosen not to invest in these projects. Had they been, it is highly improbable that they would have been successful and profitable: *"They would have generated nothing but frustration and demobilization. Nothing would have come out"* according to the AIC Innovation Leader who heads the portfolio. In brief, this portfolio only selects highly uncertain projects rejected by the investment decision-making techniques used by traditional uncertainty-reducing organizations. And if the AIC detects 'easy, little-uncertain projects' that traditional organizations would erroneously have rejected with an excessive risk aversion attitude, it rejects them as well (and redirects them to traditional organizations), because its goal is to help innovators boost genuinely disruptive ideas which will *"change Airbus from the inside"*.
- (ii) But surprisingly enough, in spite of the high level of uncertainty of the selected projects, this portfolio demonstrates a particularly performing profitability dynamic: an assessment of the potential of savings and benefits generated by the incubated or accelerated projects revealed that for 1 euro invested in the portfolio in 2018, 6 euros are expected to be given back to Airbus, due to the follow-up that traditional organizations have given to several projects after incubation / acceleration.

In sum, this portfolio selects projects that are highly uncertain and due to be all but profitable, and turns them into profitable projects.

It is undeniable that the perspective of generating additional value, additional utility by reducing uncertainty (i.e. by following  $d_3$  in Figure 1) can justify investments in projects which initially appear not to meet the required profitability criteria (for example, in the form of pre-investment trials). However, the value one can expect to generate by reducing uncertainty often remains moderate (of the order of a variance reduction, of a noise reduction). In Airbus case (and we will explain it in more details in subsection 3.1), the projects initially seemed so far from the required profitability criterion that we have the intuition that even the greatest possible expected utility gain associated with  $d_3$  would have been insufficient to meet this criteria. So the investments of AIC are puzzling with respect to decision theory, because it seems unlikely that decision theory could have recommended to follow  $d_3$ , i.e. the launch of pre-investment trials in such projects: it seems that the decision-makers went against the recommendations of decision theory. Furthermore, the profitability of the portfolio (the x6 rate of return) is puzzling, because it seems unlikely that such dramatic returns can be explained by uncertainty reduction, since again, the utility gains achievable thanks to uncertainty reduction often remain moderate.

Thus, we have the intuition that Airbus' AIC's portfolio of incubation and acceleration projects might fall out of the decision theoretic framework. It might exemplify an investment decision-making case involving investments in the unknown, and falling into the design theoretic framework. For these reasons, Airbus' AIC portfolio of incubation and acceleration projects seems to be a good potential candidate to provide empirical evidence for Model 2.

If we want to test the existence of Model 2 in the real world the test of the validity of A1, A2 and A3 with Airbus case, we need a two-step approach. Firstly, we need to verify the intuition that this portfolio is indeed appropriate to provide insights regarding design-theory based investment decision models in the unknown. To that end, we need to answer the following research question:

*RQ1: Are the remarkable profits of Airbus portfolio of incubation and acceleration projects attributable to unknown exploration?*

Secondly, if the answer to *RQ1* is affirmative, our next research question is:

*RQ2: How are project selection and unknown exploration carried out in the frame of Airbus portfolio? Do these practices confirm (A1), (A2) and (A3)?*

### **3 A twofold method: a statistical method and a single case study**

Internal documents, interviews and field observations have provided us with a large amount of data regarding Airbus' AIC's portfolio of incubation and acceleration projects. This fed a twofold methodology: a statistical method to address *RQ1* and a single case study method to address *RQ2*. In subsection 3.1, we introduce the

quantitative elements which will enable us to describe the AIC's portfolio in statistical terms, which will be key when we introduce our statistical method (based on Bayesian statistics) in subsection 3.2. Finally, in subsection 3.3, we describe our method for a single case study, according to the criteria of Goffin et al.'s (2019) Case Study Evaluation Template.

### 3.1 *A statistical method to address RQ1*

#### 3.1.1 Presentation of the quantitative data which enable to Airbus portfolio of incubation and acceleration projects in statistical terms

In what follows, we introduce the elements which describe the portfolio in quantitative terms and which will enable us to deploy our statistical method to address *RQ1*, i.e. to test whether the portfolio could contain unknown, beyond uncertainty.

Based on the description of the beliefs of traditional organizations (Engineering ; Customer Services ; Manufacturing ; R&T...) regarding the profitability of the selected projects, the projects that enter the portfolio can be quantified by the following *a priori* subjective probabilities:

- A 0.05 probability of being profitable
- A 0.95 probability of being unprofitable

These values are confirmed by the AIC Innovation Leader.

#### - **Portfolio performance between 2013 and 2018**

The AIC Innovation Leader has given us access to a document containing detailed information regarding the status and the performance of 55 projects that have been incubated or accelerated between 2013 and 2018.

This document reveals that after incubation / acceleration:

- 4 projects (i.e. 7% of the 55 incubated / accelerated projects) have been stopped, because inconclusive
- 10 projects (i.e. 18% of the 55 incubated / accelerated projects) have been classified as stored, that is to say the results are considered as conclusive, with a potential which could be exploited later on by one traditional organization. But this is not on its current list of priorities
- 41 of the incubated or accelerated projects (i.e. 75% of the 55 incubated / accelerated projects) have been found to be economically interesting (with '*a potential for a business application*', in the AIC's words) and have given rise to a subsequent project within a traditional organization (Engineering ; Customer Services ; Manufacturing ; R&T...)

In other words, the AIC has discovered that among the 55 incubated or accelerated projects, 75% were potentially profitable. The organizations which had initially rejected these projects (and which would not have invested 1 euro in them) have reconsidered their decision and invested. These post-incubation / acceleration investments can be quantified by the following subjective probabilities (which are degrees of confidence for classical investments):

- A 0.80 probability of being profitable

- A 0.20 probability of being unprofitable

These values are confirmed by the AIC Innovation Leader.

#### - **Additional figures regarding the performance in 2018**

In 2018, the AIC has wished to assess its performance more precisely, in terms of profitability, by gathering information regarding the budgets that traditional organizations raise to pursue post-incubation or post-acceleration projects.

It has considered the 23 projects (11 acceleration projects and 12 incubation projects) which had been launched and completed in 2018. Before incubation or acceleration, traditional organizations would have invested 0 euro in these projects. In total, the AIC invested  $x$  euros in incubating / accelerating these projects. Then, the post-incubation / acceleration outcomes have convinced traditional organizations to raise  $y$  euros to give rise to subsequent projects. And the benefits associated by these projects represent at least  $6.x$  euros for Airbus (a potential rate of return of  $x6$  of AIC).

Be it the shift in the beliefs of the traditional organizations or the  $x6$  rate of return, these quantified information are quite striking. Our intuition is that they cannot result from a basic mechanism of uncertainty reduction. Or if they do, it must be an outstandingly performing uncertainty reduction mechanism. Indeed, uncertainty reduction generally allows improvements of the order of a reduction of variance, a reduction of noise, etc. So our assumption H1 is that the remarkable profits of Airbus portfolio are due to a more powerful mechanism, that is unknown exploration and structuration. We test this assumption in the subsection (4.1) dedicated to *RQ1: Are the remarkable profits of Airbus portfolio of incubation and acceleration projects attributable to unknown exploration and structuration?* To do so, we use the statistical method described in the following subsection.

### 3.1.2 Description of the statistical method

Our assumption **H1** is that **the remarkable profits of Airbus portfolio are due to a more powerful mechanism than uncertainty reduction, that is unknown exploration and structuration.**

We will test H1 by statistically testing whether the dramatic returns and the shift in beliefs observed in the case of Airbus portfolio can be explained by decision theory. H1 will be validated if we statistically prove that the quantitative values which describe Airbus case are inconsistent with decision theory, since the unknown is precisely defined as the realm where decision theory no longer applies (Loch et al., 2006 ; Le Masson et al., 2018),.

In Figure 3 below, we have represented in the classical decision-theoretic framework the statistical data introduced in subsection 3.1. We will test H1 by testing whether the data featured in Figure 3 are consistent with the equations that traditionally underlie a decision theoretic framework.

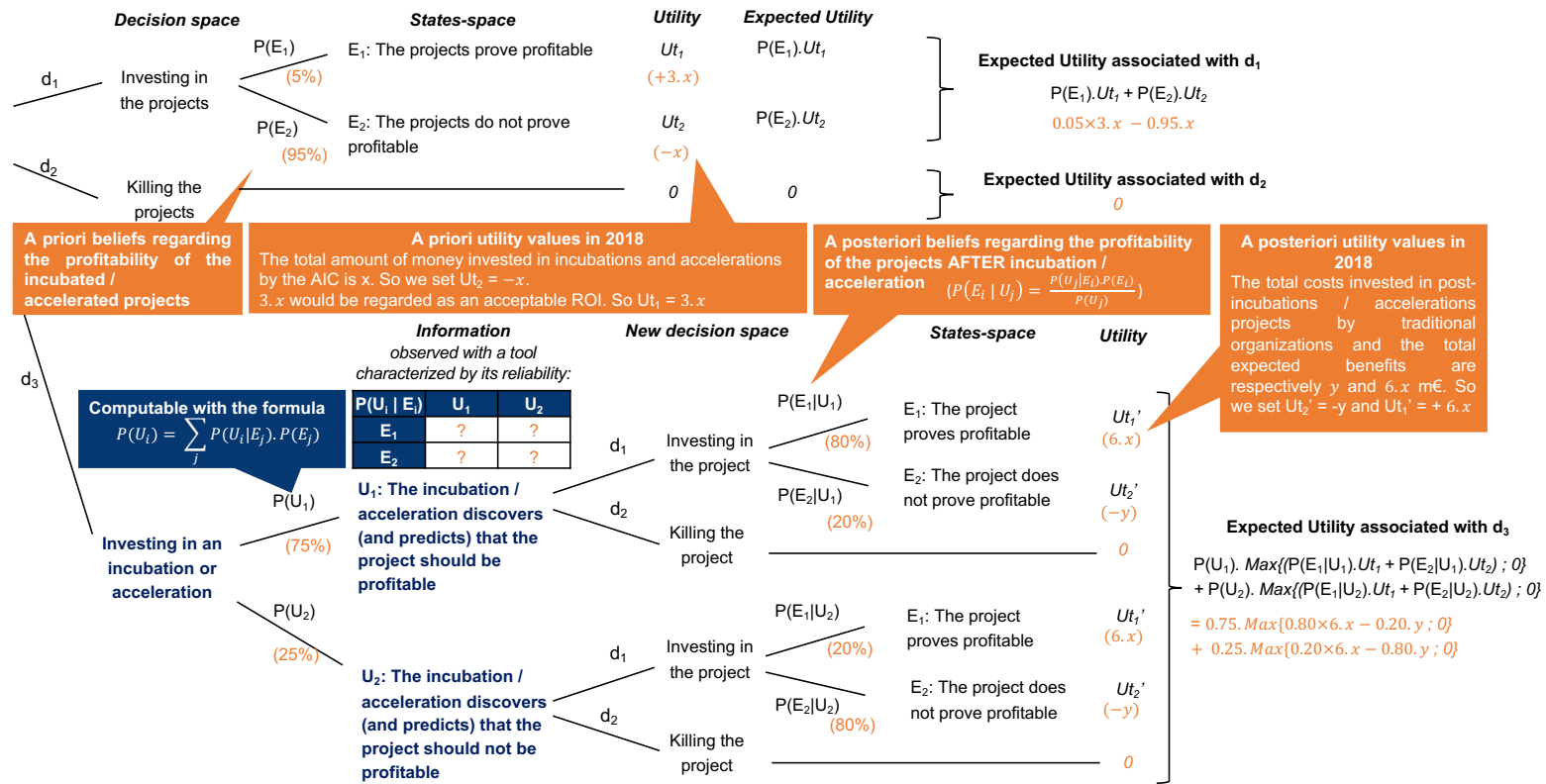


Figure 3. Representation of Airbus case in a decision-theoretic frame

If the profitability of Airbus portfolio of incubation and acceleration projects can be attributed to uncertainty reduction (i.e. if H1 is true), there exists a quadruplet  $\{P(U_1 | E_1), P(U_2 | E_2), P(E_1 | U_1), P(E_2 | U_2)\}$  that fits in the above decision-theory based framework and that accounts for the represented shift from a priori to a posteriori beliefs. In order to test H1, we will try to compute the possible value for  $\{P(U_1 | E_1), P(U_2 | E_2), P(E_1 | U_1), P(E_2 | U_2)\}$  in subsection 4.1.

Figure 3. Representation of Airbus case in a traditional decision-theoretic framework

The values featured in Figure 3 could fit into a decision-theory-based framework in two ways

- Either the shift from *a priori* to *a posteriori* beliefs can be explained as (i) the results from chance (decisional-way  $d_1$ , i.e. a gambling attitude). In subsection 4.1.1, we will compute the probability that such a scenario can indeed explain Airbus case in order to test whether this scenario is plausible.
- Or the shift from *a priori* to *a posteriori* beliefs can be interpreted as the results from an outstandingly performing capacity to reduce uncertainty, through  $d_3$  (pre-investment trials). In this case, there must exist a quadruplet of probabilities  $\{P(U_1 | E_1), P(U_2 | E_2), P(U_1 | E_2), P(U_2 | E_1)\}$  that fits in the above decision-theory based framework (i.e. that respect the Bayesian updating formula) and that accounts for the represented shift from *a priori* to *a posteriori* beliefs. In subsection 4.1.2, we compute and discuss the possible Bayesian values for  $\{P(U_1 | E_1), P(U_2 | E_2), P(U_1 | E_2), P(U_2 | E_1)\}$  in order to test whether this scenario is plausible.

If neither the (i) ‘gambling scenario’ nor the (ii) ‘outstandingly performing uncertainty reduction mechanism’ scenario are suitable to explain the outcomes of the AIC project portfolio, this will mean that decision theory

cannot account for what happened this case and that unknown underlies the values featured in Figure 3. On the one hand, this would imply an affirmative answer to *RQ1*. The outcomes of the portfolio would be attributable to unknown exploration. And the traditional ‘uncertainty reduction decisional way’  $d_3$  in Figure 3 could be renamed ‘unknown exploration decisional way’. On the other hand, if the answer to *RQ1* proves affirmative, an explanatory theoretical framework is still missing for this Airbus case (since decision theory does not explain it). Design theory, which extends decision theory to the unknown (Le Masson et al., 2018) is a very good candidate theoretical framework which could account for what happened in Airbus case. So we make the assumption **H2 that Airbus portfolio of incubation and acceleration projects could fall into a design-theory based investment decision model and that Model 2 could be this very model**. We test this assumption in subsection (4.2) dedicated to *RQ2: How are project selection and unknown exploration carried out in the frame of Airbus portfolio? Do these practices confirm (A1), (A2) and (A3)?*

### 3.2 A single-case study to address *RQ2*

In order to address *RQ2*, we have decided to carry out a single case study. In what follows, we describe our method, according to the criteria of Goffin et al.’s (2019) Case Study Evaluation Template.

#### 3.2.1 Theoretical foundation and theoretical sampling

To our best knowledge, we have encountered no innovation entity which compute its profitability as the AIC does. In general, innovation entities claim that they render non-monetary ‘primary’ outcomes (skills, capabilities...) that will generate monetary returns later on, as secondary outcomes. This makes the AIC’s case appropriate for a single case study research.

Secondly, if *H1* is true, we are in the an exception, an anomaly with respect to decision-theory-based uncertainty reduction. And we are in the face of a case which has no explanatory theoretical framework. Design theory, which extends decision theory to the unknown (Le Masson et al., 2018) could potentially take over decision theory as an explanatory framework in Airbus case. And **design-theory-based Model 2 could be the very theoretical framework that explains what happened in Airbus case (H2)**. In order to test *H2*, carrying out a single-case study (Siggelkow, 2007) seems particularly appropriate.

#### 3.2.2 Data sources, collection and triangulation

Airbus’ AIC department gave us access to a rich base of internal documents (detailed in Table 3). These documents could be accessed thanks to the first author’s status of Airbus employee at the time of the study. These documents are also those which provided us with most of the figures involved in our statistical study. With respect to *RQ2*, they provided us with a first understanding regarding how the AIC handles the unknown.

On this basis, we prepared semi-structured interviews with various actors involved in the AIC and the projects of the portfolio. The profile of the interviewees was the following:

- The AIC Innovation Leader who heads the selection process and follows the progress of each project, namely in terms of value creation. The AIC Innovation leader is the fourth author of this paper
- Four project members who could testify about the incubation / acceleration of their project. This sample of interviews was constituted in the framework of discussions with the AIC Innovation Leader: the objective was to constitute a sample that would give us an overview of the different forms of learnings that occurred in the frame of the portfolio.
- Three coaches in charge of the methodological support offered to the incubated / accelerated projects. This methodological offer is part of a ‘Methodological package’ which was co-developed by Airbus Emerging Technologies and Concepts department (which Airbus AIC reports to) and AirBusiness Academy (which is a learning centre for Airbus and its community of customers, suppliers and partners worldwide). These coaches are employees from AirBusiness Academy and are also involved in the innovation cells of other companies in the aerospace industry
- The person from Airbus coaching department, in charge of providing support to the projects regarding the economic evaluation aspects

We had also the opportunity to exchange with these people (and other people related to the AIC) in informal meetings and discussions.

Internal documents	Interviews	Observations
<b>Documents internal to the AIC:</b> <ul style="list-style-type: none"> <li>The excel file which records the progress of each individual project (in terms of economic value, maturity, prototyping progress, etc.) and which is continuously kept up to date by the AIC innovation leader</li> <li>The AIC evaluation of the year 2018 (critical summary of its way of working, its achievements, its economic performance, its perspectives for 2019)</li> <li>The Mission Letters for each individual project joining the portfolio</li> <li>Projects final presentations summarizing the outcomes of the incubation / acceleration process and presented to the members of the selection board</li> </ul> <b>AIC’s documents for communication within Airbus and outside Airbus</b> <ul style="list-style-type: none"> <li>Brochures, communication documents (toward Airbus employees) which present the AIC and the portfolio, the incubation / acceleration, its resources, its offers, its performance</li> <li>Minutes of roundtables presenting the AIC in external events</li> </ul>	<b>Between mid-February and mid-June 2019:</b> <ul style="list-style-type: none"> <li>4 one-hour interviews with the AIC Innovation Leader (+ additional informal discussions)</li> <li>3 one-hour interviews with incubation / acceleration project owners (responsible for 3 different projects)</li> <li>2 one-hour interviews with the coaches responsible for the methodological support offered to the project (+ additional informal discussions)</li> </ul>	<b>Between mid-February and October 2019:</b> <ul style="list-style-type: none"> <li>Informal discussions with people (often the interviewees) involved in the AIC</li> </ul> <b>October 2019</b> <ul style="list-style-type: none"> <li>Attendance of an ‘Innovation Event’ organized by the AIC.</li> <li>Co-facilitation of a workshop dedicated to ‘the creation of value with prototyping’</li> <li>Informal discussion with project members who presented the outcomes of their incubation / acceleration project in a marketplace format</li> </ul> <b>December 2019</b> <ul style="list-style-type: none"> <li>Attendance of a AIC monthly meeting where candidates pitch their ideas and where technology watch information are shared</li> </ul>

**Table 3. Summary of the data sources**

### 3.2.3 Data interpretation

We structured the gathered data into first-order concepts, second-order themes and aggregate dimensions according to Gioia et al.’s, (2013) methodology. Then, we asked whether this data structure was consistent with A1 and A2.<sup>57</sup>

<sup>57</sup> In order to test A3, we come back to our statistical method

### 3.2.4 Review and validation of evidence

The progress of the study was paced by steering committees attended by the four authors and during which the results were presented to managers from Airbus interested in the more general topic of economic evaluation for innovative design projects.

A first version of this paper, which included a first version of this case study, was presented at IPDM in June 2019. In addition, in December 2019, the study was presented to a meeting gathering the members of the AIC, which enabled to collect additional feedback from the field and make adjustments.

## 4 Results

### 4.1 *Testing whether decision-theory provides an explanatory framework for Airbus case*

#### 4.1.1 Testing whether the dramatic returns and the shift in beliefs observed in the frame of Airbus portfolio can be attributed to chance

If the dramatic returns and the shift in beliefs result from 55 random draws in the frame of  $d_1$ , the probability of observing 75% of profitable projects (i.e. 41 projects) out of 55 investments is  $\binom{55}{41} \cdot 0.05^{41} \cdot 0.95^{14} \approx 9.7 \cdot 10^{-42}$ .

The p-value associated with such a scenario, i.e. the probability of having randomly drawn 41 or more profitable projects is

$$p\_value = \sum_{i=41}^{55} \binom{55}{i} 0.05^i \cdot 0.95^{55-i} \approx 9.8 \cdot 10^{-42}$$

Such a scenario is highly improbable. We can eliminate this theoretical explanation.

#### 4.1.2 Testing whether the dramatic returns and the shift in beliefs observed in the frame of Airbus portfolio can be attributed to uncertainty reduction

In what follows:

$P(E_i)$  denotes to the *a priori* probabilities or beliefs.

$P(U_i)$  denotes to the probabilities of the observations, when collecting samples of additional information that are intended to reduce uncertainty.

$P(E_i | U_j)$  denotes to *a posteriori* probabilities



$P(U_i | E_j)$  denotes to the reliability of the observation tool (i.e. to the probability that when the true state of the world is  $E_j$ , the observation tool indeed predicts  $E_j$  by saying that it “sees”  $U_j$ ).

The equations that account for uncertainty reduction are the following:

$$P(U_i) = \sum_j P(U_i | E_j) \cdot P(E_j) \quad (1)$$

$$P(E_i | U_j) = \frac{P(U_j | E_i) \cdot P(E_i)}{P(U_j)} \quad (2)$$

Coming from Bayesian statistics, they describe how the observations  $U_i$  turn the *a priori* probabilities  $P(E_i)$  into *a posteriori*, updated and more certain probabilities  $P(E_i | U_j)$ . In Airbus case:

$$P(E_1) = 0.05 ; P(E_2) = 0.95 ;$$

$$P(U_1) = 0.75 ; P(U_2) = 0.25 ;$$

$$P(E_1 | U_1) = P(E_2 | U_2) = 0.80 ; P(E_1 | U_2) = P(E_2 | U_1) = 0.20$$

If Airbus case can be explained by decision-theoretic uncertainty reduction, there exists a quadruplet of probabilities  $\{P(U_1 | E_1), P(U_2 | E_2), P(U_1 | E_2), P(U_2 | E_1)\}$  that fits in the above decision-theory based framework and that accounts for the represented shift from *a priori* to *a posteriori* beliefs.

Given that

$P(U_1 | E_2) = 1 - P(U_2 | E_2)$  and  $P(U_2 | E_1) = 1 - P(U_1 | E_1)$ , we actually only need to compute  $P(U_1 | E_1)$  and  $P(U_2 | E_2)$ .

From (1) (and (2)), we can obtain the following relationship between  $P(U_2 | E_2)$  and  $P(U_1 | E_1)$  :

$$P(U_2 | E_2) = \frac{0.2}{0.95} + \frac{0.05}{0.95} \cdot P(U_1 | E_1) \quad (3)$$

Equation (3) already allows us to discuss the values that  $P(U_1 | E_1)$  and  $P(U_2 | E_2)$  can take. Indeed, given that  $0 \leq P(U_1 | E_1) \leq 1$ , equation (3) says that  $P(U_2 | E_2)$  is such that  $0.21 \leq P(U_2 | E_2) \leq 0.26$ . We can already note that an observation tool which is such that  $P(U_2 | E_2) \leq 0.26$  is very unusual. It means that this tool is very unreliable when it comes to detect unprofitable projects.

Then, we can try to identify the values of  $P(U_1 | E_1)$  and  $P(U_2 | E_2)$  that enable to obtain  $P(E_1 | U_1) = P(E_2 | U_2) = 0.80$ .

In the case of  $P(E_1 | U_1)$ , equation (2) implies that

$P(U_1 | E_1) = \frac{0.75}{0.05} \cdot P(E_1 | U_1)$ . This means that for  $0 \leq P(U_1 | E_1) \leq 1$ ,  $P(E_1 | U_1)$  is such that  $0 \leq P(E_1 | U_1) \leq 0.07$ . In other words, it is impossible that  $P(E_1 | U_1)$  reaches 0.80.

In the system of Bayesian equations (1), (2), it is impossible to find a solution couple  $\{P(U_1|E_1) ; P(U_2|E_2)\}$  such that

$$P(E_1) = 0.05 ; P(E_2) = 0.95 ;$$

$$P(U_1) = 0.75 ; P(U_2) = 0.25 ;$$

$$P(E_1 | U_1) = P(E_2 | U_2) = 0.80 ; P(E_1 | U_2) = P(E_2 | U_1) = 0.20$$

So the remarkable outcomes of Airbus portfolio of incubation and acceleration projects cannot be attributed to any mechanism of uncertainty reduction, even an outstandingly performing one.

### **Answer to Research Question 1**

Our statistical tests reveal that neither gambling nor uncertainty reduction can explain what happened in Airbus' portfolio of incubation and acceleration projects. Decision theory is in contraction with the quantitative elements which describe the case. So Airbus case falls beyond the domain of applicability of decision theory. This suggests that unknown underlies the value featured in Figure 3 and that in Figure 3,  $d_3$  could be renamed the 'unknown exploration' decisional way. Our answer to RQ1 is affirmative: the outstanding profitability of Airbus portfolio of acceleration and incubation projects can be attributed to unknown exploration and structuration.

After having shown that, we still need to find an explanatory theoretical framework for Airbus case (since decision theory does not apply). Design theory, which can extend decision theory to the unknown (Le Masson et al., 2018) is a good candidate theoretical framework which could account for what happened. So we make the assumption H2 that Airbus portfolio of incubation and acceleration projects could fall into a design-theoretic investment decision model and that the theoretical investment decision model we have hypothesized at the end of our literature review (i.e. Model 2) could be this very model. We test this assumption in the subsection (4.2) dedicated to RQ2: *How are project selection and unknown exploration carried out in the frame of Airbus portfolio? Do these practices confirm (A1), (A2) and (A3)?*

In Subsection 4.2, we would like to test whether the real-world design-theory based investment decision model that underlies Airbus portfolio of projects is consistent with Model 2. In particular, we will test whether the AIC

- has established a capability that systematically diagnoses, detects, identifies the projects that contain unknown (A1)
- has deployed capabilities, methodologies and managerial approaches enabling to rigorously explore and structure the unknown (A2)

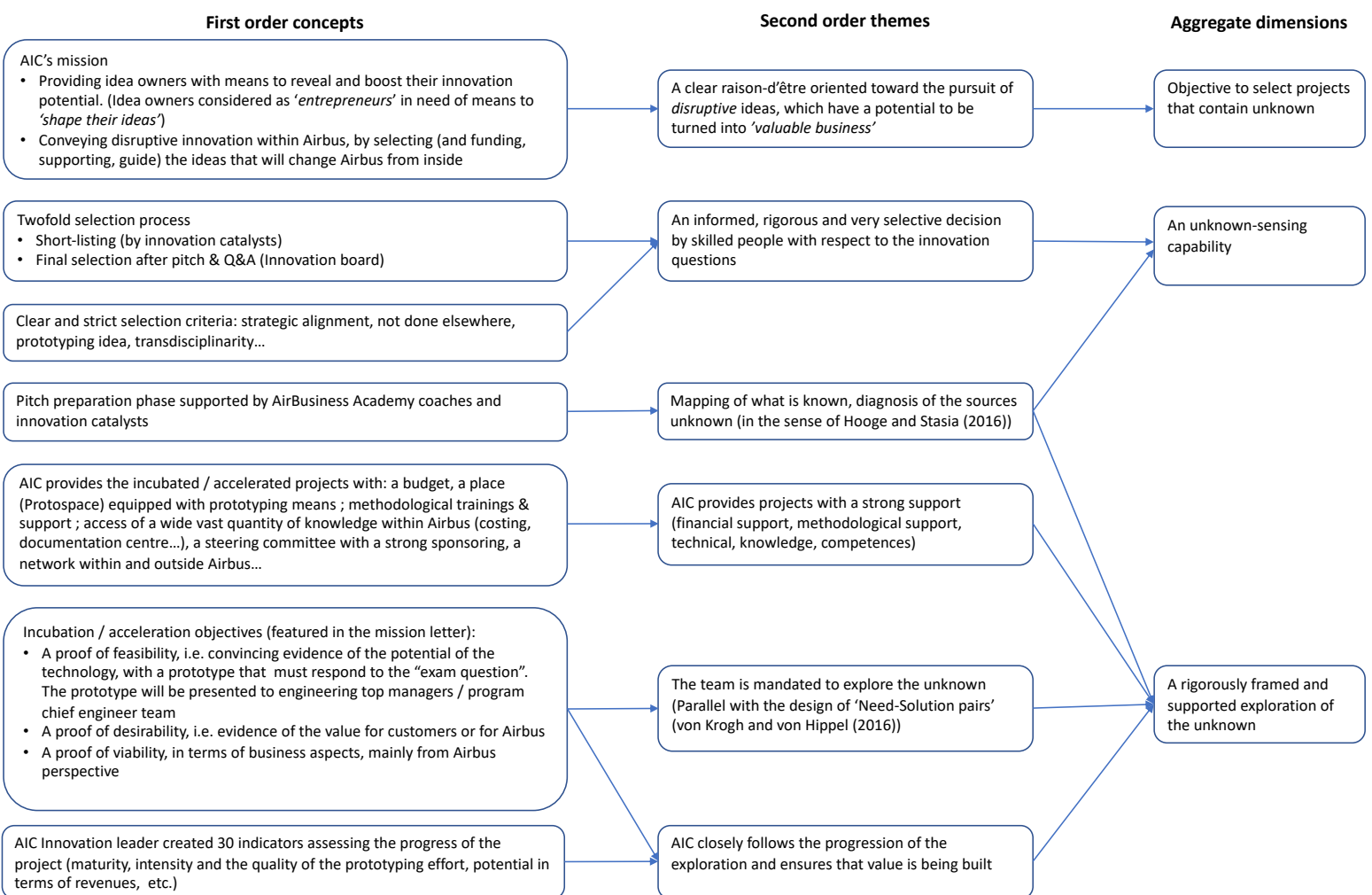
To that end, we use a case study.

Besides, we will discuss whether Airbus case confirms the possibility of developing a quantified technique to rigorously assess the value of unknown exploration, in the same fashion as decision-theory based investment

decision models rigorously assess the value of uncertainty reduction (A3). To that end, we will come back to our statistical method in Subsection 4.3.

## 4.2 Addressing RQ2 with an single-case study

In order to address RQ2, we structured the data describing the AIC functioning according to Gioia et al.'s (2013) methodology. The structured representation of the data is given in Figure 4 below. In what follows, we detail our findings regarding the AIC's mission (subsection 4.2.1), regarding how the AIC selects (subsection 4.2.2) and manages (subsection 4.2.3) projects.



**Figure 4. Gathered data structured according to Gioia et al. (2013) method**

### 4.2.1 The AIC's mission

Over the recent years, the commercial aircraft industry has been increasingly concerned with challenges related to industrial ramp-up, mass production, etc. In this context driven by time, costs, operational performance and profitability, the eventual disruptive ideas of Airbus collaborators are perceived as too risky – and traditional

organizations (Engineering, Manufacturing, Customer Services, R&T...) are unlikely to allocate them the resources for being tested and developed. In the face of this, the role of the AIC is to ensure that Airbus overall portfolio of projects does not get too unbalanced toward incremental innovation. It aims at providing *'fresh air for disruptive innovation'*.

Its mission is to detect the disruptive ideas which have *'a promising innovation potential'*, and to provide their owners with resources and facilities to *'shape'* and accelerate their ideas. In particular, the AIC provides a budget, a place (the Protospace, a prototyping lab in Toulouse, which is part of a network of 10 other Protospaces located at the different international sites of Airbus, and which gives access to a vast network of knowledge and competences within the Group) and a *'guidance package'*, co-developed by the AIC and AirBusiness Academy) which offers methods to frame the incubation / acceleration of the ideas. The AIC 2019 brochure states that it aims at *'revealing your full innovation potential'* and *'help you shape your ideas'*. In addition, the AIC seeks the ideas which are not only disruptive, but which also have *'a potential to be turned into valuable business applications'* and which *'will change Airbus from the inside'*. Thus, the ideas that enter the AIC's portfolio of incubation / acceleration projects also need to be aligned with Airbus strategy (this is one of the selection criteria, which will be detailed later on). These numerous constraints lead to a very low selection rate (36/1500 in 2018).

#### 4.2.2 Selection of the projects to be incubated or accelerated

##### **Selection process**

The AIC brochure describes a selection process involving two main steps. The first one is called *'Trendification'*: it is a continuous phase of technology watch, both within Airbus (innovation events ; presence of local innovation catalysts in the competence centers) and outside Airbus (benchmark and scouting, trend analysis...). This phase enables to reach and identify potential applicants for an incubation or an acceleration project. During this phase, a first selection filter applies at the level of local innovation correspondents (innovation catalysts) who short-list the best candidate ideas within their competence center and take them up to the AIC. The AIC Innovation Leader notes that *"the innovation catalysts are familiar with the innovation problematics and well-placed to assess the appropriateness of the topics for the portfolio"*.

The short-listed projects are invited to enter the second phase of the selection process which involves a pitch in front of a selection board (attended by top managers, (e.g. head of competence centers, as potential sponsors of the project). If the project successfully passes this step, the team will frame its project with the methodological support of AirBusiness Academy coaches, before receiving an official mission letter.

Interestingly, a coach from AirBusiness Academy reports an additional phase which does not explicitly appear in this process: *"We do not only provide methodological support once the project owners have pitched and been selected. We also help the teams prepare their pitch"*

### **The pitch preparation phase**

In order to help the candidates maximize their chance of successfully passing the pitch, AirBusiness Academy coaches encourage them to gather all the knowledge that already exists (especially within Airbus) on their topic and to make a solid project definition based on what exists in the different competence centers. This is all the more so important as one important selection criterion, called *'Not done elsewhere'*, requires that any engineer, any expert, any department etc., who is already working on the candidate topic within Airbus joins the application project: if he or she is not onboard, the project will not be selected to be incubated or accelerated. AirBusiness Academy coaches also help the candidates demonstrate how the project is in line with the strategy of the Airbus, so that 'strategy-oriented' selection criteria. This can be interpreted as an effort to comprehensively map what is already known, what is uncertain, and doing so, start discussing the sources the unknown, as recommended by Hooge and Stasia (2016).

The coach who first mentioned this phase to us intervenes in innovation cells in other firms, and that to her knowledge, this preparation phase is unique to Airbus. One reason is that it consumes resources: *"Firms are not necessarily willing to dedicate so much means to their innovation cell. The level of resources that Airbus dedicates is quite exceptional [...] The preparation effort facilitates project run and increases the likelihood of a successful outcome. It plays a key role in the performance of the portfolio."* Our field observations show that the members of the board precisely aim at assessing the level of preparedness of the team. For instance, one question submitted to the applicant was: *"Are you aware of this technology X which actually seems similar to what you propose? How is your proposition different?"*. One attendant also remarked that *"we rapidly detect those [the applications] which are not solid."*

### **Selection criteria**

All the interviewees from the AIC (innovation leaders, support team) stress that the selection process is *"demanding"* and *"highly selective"*. In 2018, the AIC selected 36 out of 1500 applicant projects. According to the AIC Innovation Leader, *"We only select the very best ideas. This is decisive in the performance of the portfolio."*

The criteria for a project to be selected are clear and the applicants are informed of them. Besides, they are strictly enforced, as we detail below. The projects must necessarily be novel in the sense that what is proposed has 'not been done elsewhere', especially in Airbus. If an expert, an engineer, a team is working on a similar topic, he must join the candidate project. Otherwise the project will not be selected. In 2018, 100% of the selected projects met this criteria. All acceleration and most incubation projects must also be transdisciplinary (multi-competence-centers, multi-functional or multi-business-units). In 2018, 80% of the projects selected met this criteria. The tackled issue must be strategic or roadmap-oriented, because the AIC seeks topics which can lead to a business application. In 2018, 100% of the selected acceleration projects and 80% of the selected incubation projects met these criteria. The AIC selects the projects which propose to address complex, difficult and long-lasting problems, the solving of which would represent an important business potential. A project

selected in 2018 which related to a design change implemented at the level of one system illustrates it. The design change was particularly interesting, because it promised significant increases in aircraft performance. But it entailed that one essential subsystem could no longer be integrated on the system. The project owner explained that: *“We had been trying conventional solutions for 2 years, and it didn’t work. We were at a limit of feasibility. We needed a brand new solution, both disruptive and feasible [...] We have applied for a SPRINT [i.e. an acceleration project] proposing a very aggressive solution. A priori, our answers to the ‘exam questions’ were blurred. We could set no targets, especially in terms of potential costs. But our sponsors were ready to pay a lot, because the challenge was the achievement of the major change at the system level, which represents a genuine performance gain on existing aircraft ”*

The AIC selects projects which involve both a large knowledge gap and a large conceptual gap. He has the final say on the selection decision observes that *“he favors non-consensual topics (i.e. the topics that are attributed heterogenous marks by the members of the board).”*

**In summary, the AIC’s investment decisions are extremely documented, informed and structured. The pitch preparation phase can be interpreted as a mapping of what is known and a diagnosis of the unknown. During the selection process, the AIC mobilizes numerous stakeholders, numerous competences (innovation catalysts, top managers and high level sponsors, methodological coaches, experts etc.) and enforces clear and strict criteria in order to detect and select projects which are genuinely innovative and which have the potential to be turned into “real business applications”.**

**All these elements support the assumption according to which the AIC has developed a kind of unknown-unveiling and unknown-sensing capability. This validates A1.**

#### 4.2.3 Management of the selected projects

##### **Mission letter**

Once the project has been defined, the team receives a Mission Letter prepared by the Innovation Leader mandating it to:

- Assess technical feasibility of the topic, by identifying risks and showstoppers: the team needs to determine the assumptions under which the investigated concept could be a working concept
- Build a prototype enabling a convincing demonstration which will be presented to engineering top managers / program chief engineer team
- Provide first business information: costs, savings, lead-time...

In terms of deliverable, the Innovation Leaders expects *“A proof of feasibility, i.e. convincing evidence of the potential of the technology, a proof of desirability, i.e. evidence of the value for customers or for Airbus and a proof of viability, in terms of business aspects, mainly from Airbus perspective.”*

The mission letter recalls the issue that the team members have proposed to address during their pitch. But it neither specifies a pre-defined target, nor a well-define problem, as illustrates this quote from a project owner: *“[The project was related to an industrial assembly sequence] Usually, we have a model which describes how the parts move with one another. According to this, we define hypothetical assembly sequences that we experiment. Here, we had no such model. The traditional way to address the problem was not valid.”* Specifying the problem which the topic will eventually address(or re-specifying it, since the traditional problem is ill-defined) is part of the incubation / acceleration objectives.

Besides, as mentioned above the team members are expected to explore the *“feasibility, desirability and viability”* of solution that they have proposed using the pitch. But they might need to explore beyond this solution. For instance, the team (mentioned in the previous subsection) who had proposed the ‘aggressive’ solution (in an attempt to integrate the subsystem of the system the design of which was changed) has found out after a week of acceleration that its idea would not work. In spite of that, it has re-oriented the project, tested alternative ways with 4 (less disruptive) prototypes which have generated important knowledge regarding technological bricks that make today the object of further investigations. The Innovation Leader considers this project as an exemplar acceleration.

We can make a parallel with the design of “Need-Solution pairs” theorized by von Hippel and von Krogh (2016).

### **A strong level of support during project run**

The selected projects start with a one-week methodological training, to learn C-K theory, Agile, Design thinking, Learn UX, ... During the course of the project, team members benefit from the full availability of a coach from AirBusiness Academy. These latter support them with the methodological aspects.

The AIC ensures that all the relevant existing knowledge is available to the project team. On the one hand, this is made possible thanks to the multidisciplinary of the project (and by implication the multidisciplinary of the team). On the other hand, one person from Airbus information center is dedicated full-time to take in charge any documentary research that the team would need in the frame of the project. Regular steering committees also enable the project to benefit from the view and the knowledge of the heads of the competence centers (which can help unblock some problems). Besides, one person from Airbus costing department provides support regarding the economic questions. One coach from Air Business Academy notes that such a level of support is *“massive in comparison with what is done in the other firms”*.

### **A strong level of monitoring during project run**

In terms of management, the innovation manager in charge of the portfolio requires a demanding reporting from the project team. In order to follow the creation of economic value, he keeps track of them with the use about more than 30 indicators (even 99 if we do not consider that some are mutually exclusive), which summarize the progress of the project, its maturity, which characterize the prototyping effort, the economic figures...<sup>58</sup>

**In summary, the AIC Leader stresses that “To drive innovation teams forward to disruption, a high level of sponsorship, technology, trend analysis, cost and impact assessment, have proved helpful.”. He considers that the projects pay off thanks to “the rigor we give them: a place, with prototyping resources, a training and a steering committee with a high level of sponsorship”. The projects are well equipped, supported (in terms of methods, tools, competences...) and rigorously managed (a high level of reporting) to investigate a topic that calls for an exploratory approach. This supports A2.**

#### 4.2.4 Is it possible to assess the value of unknown exploration? (i.e. testing A3)

With our statistical test in subsection 4.1.2, we have seen that the figures featured in Figure 3 are inconsistent with the Bayesian formula that account for uncertainty-reduction mechanisms.

Thus, the decisional way  $d_3$  can by no means represent to an “uncertainty-reduction decisional way” which would describe the decision-makers’ decision to invest in a pre-investment trial in the perspective of reducing uncertainty. This paper supports the idea that  $d_3$  plausibly corresponds to an “unknown exploration way”.

Interestingly, although the statistical values represented in Figure 3 do not respect the Bayesian equations, nothing prevents us from making the traditional *expected utility* calculation for the  $d_1$ ,  $d_2$  and  $d_3$  way with the decision theory-based formula. We remind the formula below.

- expected utility associated with  $d_1$  (investing in the projects):  $0.05 \times 3.x - 0.95.x$
- expected utility associated with  $d_2$  (rejecting the projects): 0
- expected utility associated with  $d_3$  (investing in an incubation or acceleration):  
 $0.75. \text{Max}\{0.80 \times 6.x - 0.20.y ; 0\} + 0.25. \text{Max}\{0.20 \times 6.x - 0.80.y ; 0\},$

---

<sup>58</sup> It is interesting to note that these indicators include monetary indicators, which enable follow the creation of value during the course of the incubation / acceleration. And they also include scored / quasi-quantitative indicators (e.g. those related to the intensity and the quality of the prototyping effort) that the innovation manager in charge of the AIC has created. This creation of indicators reminds some works which we have classified as ‘Model 1’ and which call for enriching the sets of investment decision criteria with new criteria better adapted to radical innovation. We will discuss this aspect in subsection 5.2.2.



The real values of  $x$  and  $y$  cannot be disclosed for confidentiality issues. But if we suppose that the amount of the initial investment to incubate / accelerate the projects was 10 (i.e.  $x = 10$ ), and that the outcomes of the incubations / acceleration led to dedicate a three-time-larger investment to give rise to subsequent projects (i.e.  $y = 30$ ), we can compute the following expected utilities:

- expected utility associated with  $d_1 = -8$
- expected utility associated with  $d_2 = 0$
- expected utility associated with  $d_3 = 27 > \max(-8, 0)$

If the figures fitted with the Bayesian formula, +27 (i.e. the difference between 27 and  $\max(-8, 0)$ ) would correspond to the value of uncertainty reduction, i.e. to the value that the decision-maker can expect to earn if he or she decides to make a pre-investment trial before deciding on a final investment

In Airbus case, we have demonstrated that the figures do not fit with the Bayesian formula and that we are under design theory. So **these +27 can be seen as the value generated by unknown exploration during incubation / acceleration.**

**This validates A3: under Model 2, it is possible to rigorously assess the value of unknown exploration, in the same fashion as decision-theory based investment decision models rigorously quantify the value of uncertainty reduction**

These +27 m€ correspond to the value generated by the AIC Group. They are lower than the +60 (i.e.  $x6$  rate of return) published in the economic assessment of the year 2018: the reason is that the AIC economic assessment only *qualitatively* describes the residual uncertainty or risk that remain after incubation / acceleration. The +27 correspond to a 'promise' of benefits, provided the projects launched by traditional organizations effectively prove successful. Beyond this, Figure 3 *quantitatively* includes in the expected utility calculation the residual uncertainty that the traditional organizations which invest in the post-incubation / acceleration projects will have to reduce.

It is interesting to note that the fact that there remains residual risk or uncertainty after incubation/acceleration is not a predicament, it is perfectly normal. Indeed, traditional organizations are supposed to be well-equipped to reduce and eliminate uncertainty. They own the methods to do so. What is of upmost importance is that the incubated / accelerated projects that are handed over to traditional organizations *do not contain residual unknown* (because the emergence of residual unknown during the course of the project would probably not be in line with what these organizations expect to do with the projects and because traditional organizations are unlikely to own the methods that handle the unknown). This highlights that when a project contains both unknown and uncertainty, the sequence of processing these two forms of lack of knowledge is crucial: one needs to first structure the unknown, *before* handing over the project to an organization that will reduce the remaining uncertainty in a second time. Doing it the other way round is due to be underperforming, because the initial uncertainty reduction effort will be undermined when the residual unknown emerges. Apart in a

‘corrective effort’, if a project has mistakenly been launched with an reducing uncertainty approach while it contained unknown, it does not make sense for a project manager to explore the unknown after having reduced uncertainty.

## 5 Conclusion and discussion

### 5.1 Summary of the contributions

*Answer to RQ1: Are the remarkable profits of Airbus portfolio of incubation and acceleration projects attributable to unknown exploration?*

With a statistical test, this article reveals that the projects selected by Airbus AIC for being incubated or accelerated contain unknown. During the incubation / acceleration process, the project do not go through an uncertainty reduction process. The unknown they contain is explored, structured. Statistically, the structuration of the unknown is the possible consistent explanation for the remarkable economic outcomes achieved by the portfolio: neither chance nor uncertainty reduction could have led to such outcomes.

*Answer to RQ2: How are project selection and unknown exploration carried out in the frame of Airbus portfolio? Do these practices confirm (A1), (A2) and (A3)?*

With an empirical method, we have found that

- Airbus AIC owns an ‘unknown sensing capability’ (i.e. has established a kind of pass-unknown filter) which enables it to exclusively select projects that contain unknown within its incubation / acceleration portfolio. (A1 confirmed)
- Airbus AIC has developed very rigorous methodological, managerial and organizational means to efficiently explore the unknown, which follows the generation of value during the incubation and acceleration phases. (A2 confirmed)
- it is possible to rigorously assess the value of unknown exploration, in the same fashion as decision-theory based investment decision models rigorously assess the value of uncertainty reduction (A3 confirmed)

These findings provide empirical evidence for the existence of a design-theory based investment decision model in the unknown which predicts that it is possible to intentionally design a lot profitability in the unknown, without gambling, provided one is able to detect the presence of the unknown and to efficiently manage this unknown.

In summary, our answers to (RQ1) and (RQ2) confirm that in the same fashion as there is a genuine rupture in what we deal with depending on whether we face basic uncertainty or unknown, there is a rupture in the profitability dynamic when one moves to the unknown. Under basic uncertainty, expected profitability obeys decision-theoretic rules: it decreases as the level of uncertainty increases. But in the unknown, expected profitability can follow a design-theory-based behavior: this paper provides empirical evidence which illustrate that it is possible to intentionally design a lot profitability in the unknown, provided one is able to detect the presence of the unknown and to efficiently manage this unknown.

Consequently, investing in the unknown may be less risky and more profitable than investing under basic uncertainty, provided the decision-maker's economic reasoning falls into a design-theory based investment decision model (Model 2), and meets its assumptions in terms of capacity to detect the unknown (A1) and capacity rigorously manage its exploration (A2). Within this model, the decision maker can even assess the value gained by structuring the unknown (A3).

In what follows, subsection 5.2.1 discusses the counterintuitive nature of these findings. Then, subsection 5.2.2 reconsiders and re-discusses the elements of distinction between the research works which we have characterized as fitting with a Model 1 investment decision model (i.e. investment decision model based on lightened economic management and enriched sets of decision criteria) and Model 2 (i.e. investment decision model based on design theory). Finally, subsection 5.2.3 notes that the findings of this paper represent a call the acquisition of design based methodologies enabling to detect and explore the unknown and their application over a range of situations even wider than seemingly unprofitable projects. This could be very beneficial to this firm which does so. But subsection 5.2.3 also concludes on a warning: the sole acquisition of design based methodologies such as the ones of the AIC can by no means be considered as a magic bullet which will enable to generate systematic returns out of the unknown. This paper has definitely not clarified all facets of the generation of economic value in the unknown, and there remain some part of 'mystery' in this regard.

## *5.2 Discussion*

### *5.2.1 Is it really counterintuitive to say that investing in the unknown may be less risky and more profitable than investing under uncertainty?*

A design-theory based investment decision model (Model 2) which predicts that one can generate a lot of profits by investing in the unknown, without necessarily gambling and taking extreme risk is definitely very counterintuitive in terms of economic reasoning under uncertainty. However, in the light of prior works in the management literature and in the entrepreneurship literature, this finding may not be so surprising.

Firstly, we can make a parallel between the counterintuitive nature of this finding and Kleinschmidt and Cooper (1991) observing in the conclusion of a study dedicated to the relationship between innovativeness and firm performance:

« *The assumption that highly innovative products are too risky and have a negative performance is incorrect. On average, they do very well!* ».

In 1991, Kleinschmidt and Cooper characterized their finding as '*provocative*' (and hoped that it would encourage managers to be less reluctant to dedicate efforts to radical innovation).

Beyond noting that the counter-intuitiveness of our findings reminds the provocativeness of Kleinschmidt and Cooper's (1991) result, one can make the two following remarks:

- in the light of numerous subsequent works, Kleinschmidt and Cooper's (1991) is no longer '*provocative*' in the management literature in 2020. Firstly, it is widely acknowledged that in a contemporary context of intensive innovation, investing in the development of radical, disruptive innovations is essential for firms' long-term profitability and survival (e.g. Veryzer, 1998 ; Chao et Kavadias, 2008). In order to prepare its future and its survival, any firm is willing to invest in radical innovation. Secondly, and most importantly, over the last decades, research works in innovation management have dismissed the view of innovation as the simple output of an 'innovation black box' and stress that innovation covers a set of well-organized collective activities, capacities of action which are essential to obtain successful innovative results (Le Masson et al., 2017): this means that a firm which develops successful, profitable radical innovations is not a lucky gambler, but an exemplary case of a firm which owns and is able to manage its innovation capabilities, capacities of innovative action, etc. The idea that it is possible to create a lot of value out of the unknown, provided one is able to detect and efficiently explore the unknown is consistent with this.
- other studies (e.g. Cooper et al., 2001 ; Green et al., 1995, etc.) found out that radical innovation projects are more likely to fail or to be killed before completion, contrary to Cooper and Kleinschmidt's (1992) observation. In the first instance, this seems contradictory with the idea that one can create a lot of value by investing in the unknown (all the more so as these studies often assume that the high rates of failure could be due the risky and long-term nature of radical innovation projects). However, the works in the management literature dedicated to the organization, the development, the exercise of the capacities of innovative action mentioned in the previous paragraph provide alternative explanations for why radical innovation projects often fail: they stress that innovation which calls for dedicated organizations, specific processes, specific capabilities. In other words, innovation is an activity in its own right and the structures (organizations, processes, competences...) which frame it need to be adapted to the specificities of radical innovation (e.g. Le Masson et al., 2017 ; Le Masson, 2001). For instance, the development of radical innovation may involve 'concept shifts' (Seidel, 2007) not only in the first phases of the development process (as in the case of incremental innovation), but at any phase during the development process: thus, the development process of radical innovation needs to be adapted to this specificity of radical innovation, i.e. this involves designing a development process model that is specific to the radical innovation (Seidel, 2007). Engaging in the development of radical innovations with the traditional and ill-adapted processes is very likely to lead to failures.

Thus, the failures in radical innovation may be symptomatic of firms which do not own action capabilities that are specific and adapted to radical innovation (rather than to the risky nature of innovation). And this fact that the failure rate is high is not so surprising. Indeed, establishing such organizations, capabilities, processes... specific to radical innovation is also a demanding task and is not straightforward. Thus, many firms may not own them, which would explain a high rate of failure. Similarly, establishing a capability to detect (A1) and rigorously explore (A2) the unknown is demanding and not straightforward task, which would explain why cases as remarkable as the one of the AIC are scarce.

#### 5.2.2 Re-discussion of the elements that distinguish Model 1 investment decision model (i.e. investment decision model based on lightened economic management and enriched sets of decision criteria) and Model 2 (i.e. investment decision model based on design theory)

When studying the practices enforced by the AIC to manage the exploration of the unknown, we have observed that the Innovation Leader in charge of the AIC rigorously tracks the progress of the incubated / accelerated projects. To that end, he has created numerous indicators (about 30) which enable follow the creation of value during the course of the incubation / acceleration. These indicators include monetary criteria. And they also include scored / quasi-quantitative indicators (e.g. related to the intensity and the quality of the prototyping effort ; to the maturity of the projects ; its multidisciplinary ; its sponsorship, etc.).

We can make a parallel with research works which call for creating distinct sets of indicators, depending on whether evaluates incremental or radical innovation projects (e.g. Schmidt et al., 2009). In our literature review, we have classified as 'Model 1' these works which call for enriching the sets of investment decision criteria with new criteria better adapted to radical innovation, and which put aside the monetary criteria during the early phases of a radical innovation project. And we have considered that they remain in a decision-theoretic paradigm, since they circumvent the profitability criterion and aim at collecting information on other dimensions which can be more easily characterized in explicit terms and used to inform the investment decisions.

The fact that Airbus AIC investment decision model falls into design-based investment decision models, but creates new decisional criteria as recommended by Model 1 invites to question whether Model 1 decision frameworks falls so neatly with the decision-theoretic framework. Indeed, it would be interesting investigate whether the creation of new indicators could be seen as a design practice and investigate the extent to which this could lead to re-discuss / refine Table 3.

### 5.2.3 A call for developing capacities of detecting and rigorously managing the unknown... but which should not be misunderstood

#### **A call for developing capacities of detecting and rigorously managing the unknown... and largely applying them...**

In this paper, in the frame of Airbus case, we have seen that being able to sense, detect the unknown and rigorously explore it are essential capabilities which can enable to generate a lot of profitability out of an investment in the unknown.

One can add that recent works in design theory have revealed that the occurrence of radical innovation is less rare than one might initially think: quite the contrary, in the case of several consumption goods (the smartphone, but also vacuum cleaner, the toothbrush, etc.), radical innovations are occurring on a permanent basis (El Qaoumi, 2016), . This phenomenon is called functional expansion. It means that products the evolution of which seemed manageable with basic incremental innovation efforts actually require more radical-innovation-oriented design efforts. In the first instance, this can look daunting for the engineering departments which develop these products. But the results of this paper which highlight the possibility to generate a lot of profitability by intentionally investing in the unknown, without necessarily gambling are news for these engineering departments. Indeed, they suggest that the unknown could be a promising resource for developing radical innovations without taking excessive financial risk. And they suggest that when one considers the development of a radical innovation, there may be an interest engaging in genuine unknown which may be less risky than basic uncertainty, and more profitable. For the engineering departments interested in such a perspective, the findings of this paper represent a call to develop the capabilities of detecting a rigorously managing the unknown.

More generally, we can ask whether the capability to detect the unknown and the capability to rigorously manage its exploration could also helpful under investment decision situations other than ‘investment decision situations in the unknown’.

In the specific case of Airbus AIC, these capabilities have enabled to turn projects that were due to be all but profitable (and that were rejected by the traditional organizations of the Group) into profitable projects. In that, they have generated interesting returns on investment for Airbus. So the question may be asked whether these capabilities could help generate profits in other situations. The outstanding performance of the portfolio of incubation and acceleration project reveals that the AIC is able to detect ‘positive unknown’, i.e. the unknown which offers room to design states of the world which will positively contribute to the generation of profitability. Is it also possible that the AIC’s unknown sensing capability detects the presence of ‘negative unknown’, i.e. the voids which are likely to be filled by unwelcome states of the world that could emerge during the course of the project? Such a ‘negative-unknown capability’ could be very useful to check that the projects selected by traditional organizations do not contain undesirable unknowns, but only uncertainty to be reduced. Conversely, we could investigate whether the absence of an unknown-sensing step in traditional

organizations could explain the unexpected failure of projects that were launched (by traditional organizations), that were perceived as low-risk and were due to be successful, and which, against all odds, have failed. This could open interesting perspectives for further research.

**However, the acquisition of design-based methods can by no means be considered by firms as a magic bullet which enables them to generate systematic returns out of the unknown**

In Airbus AIC's case, we have seen that the articulation of (i) an unknown-detection-capability and (ii) a capability to efficiently manage and explore the unknown has enabled to turn projects that were due to be all but profitable into profitable projects. However, many aspects about *how* this articulation generates value remain mysterious and call for further research.

Firstly, as noted above, generating value by investing in the unknown does not simply require to detect the unknown, it requires to detect the 'positive unknown' which offers room for the design of opportunities and to reject 'negative unknown' which will involve unwelcome events. Then, even within the 'positive unknown' category, all forms of unknown do not have the same potential. Some will embed a higher potential of value creation through exploration than others. Some projects might only contain a low positive potential of unknown, the exploration of which will generate little additional value, while others may contain a higher positive potential of unknown. In the same fashion as decision theory characterizes the intensity of the uncertainty involved in a project, we could imagine a scale which could locate projects depending on how high their potential of 'value generation through exploration' is. Incorporating (implicitly or explicitly) such a scale in one's unknown sensing capability may be very important in order to avoid the selection of projects with the potential of positive unknown that will be insufficient to reach the firm's profitability criteria. So far, we know little regarding such a characterization / such an assessment of the unknown (positive) potential. Even in Airbus case, it remains quite mysterious.

Secondly, from one team to another, from one firm to another, the capacity to apply design-based methods and efficiently explore the unknown may be more or less performing. The exploration of the same potential of positive unknown by two different teams, or firms will not necessarily generate the same value of unknown exploration.

This suggests that there is a challenge for firms to be aware of the 'power' of their unknown exploration capabilities, and to be able to detect and select projects which contain a potential of positive unknown that is *'high enough' with respect to this exploratory power*, so that they will indeed create profitability when exploring the unknown.

In other words, the articulation between (i) an unknown-detection-capability and (ii) a capability to efficiently manage and explore the unknown, which seems to enable the generation of profitability out of the unknown is

all but straightforward. Further modelling efforts and empirical research are necessary to understand more deeply and specify the conditions and the mechanisms which can allow this articulation to effectively render value.

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La théorie de la décision offre des techniques de calcul qui quantifient la valeur de l'information dans l'incertain et permettent de guider des décisions en conséquence. Ces techniques sont très pertinentes pour le calcul économique dans l'incertain. En revanche, elles négligent la valeur de l'inconnu. Et il est largement prouvé que de telles techniques sont inappropriées, voire induisent en erreur dans des situations qui contiennent de l'inconnu. Cela signifie-t-il que dans l'inconnu, les calculateurs économiques devraient renoncer à toute forme d'approche calculatoire, et guider leurs décisions en s'appuyant sur leurs intuitions? Ou pourrions-nous envisager une approche de calcul qui isolerait et évaluerait les effets économiques dans l'inconnu? Ce travail de recherche offre quelques réponses à ces questions, en étudiant des phénomènes économiques observés dans le secteur de l'aviation commerciale, où le calcul économique est massivement utilisé (études de rentabilité, analyse des coûts...), et en mobilisant la théorie de la conception qui rend compte de l'émergence de l'inconnu.

Un premier axe de recherche concerne les impacts de l'inconnu en conception réglée : nous cherchons à identifier la mesure dans laquelle l'inconnu peut impacter les coûts de développement d'un produit, en simulant la relation entre expansion fonctionnelle et dynamique d'augmentation des coûts de développement avec le temps. Nos simulations révèlent que l'expansion fonctionnelle génère de véritables chocs dans l'architecture d'un produit, dont l'impact sur les coûts peut être considérable. Et nous étudions comment des techniques classiques de réduction des coûts (modularisation, sur-investissement dans de l'apprentissage anticipé) pourraient réduire les coûts de l'expansion fonctionnelle. Cela amène à discuter comment une utilisation à mauvais escient de techniques classiques de réduction des coûts en situation d'expansion fonctionnelle peuvent générer des catastrophes industrielles. Un deuxième axe s'intéresse à des situations d'investissement dont les rendements économiques semblent incompréhensibles dans le cadre de la théorie de la décision. Nous démontrons que l'inconnu a joué un rôle dans la génération de ces rendements. Et nous montrons que des raisonnements, méthodes et des principes managériaux relevant de la conception innovante ont été utilisés pour explorer rigoureusement l'inconnu et générer de tels rendements.

Les résultats de ce travail suggèrent que l'inconnu peut être géré comme une variable économique à part entière, ce qui soulève d'autres questions, notamment au sujet des types d'organisation qui pourraient déployer du 'calcul économique dans l'inconnu'.

## MOTS CLÉS

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Coûts de développement ; décision d'investissement ; inconnu : expansion fonctionnelle ; générativité ; apprentissage

## ABSTRACT

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Decision theory provides computational techniques which quantify the value of information and guide decisions under uncertainty. These latter are very relevant for economic calculation under uncertainty, but they ignore the value of the unknown. It is now well-proved that such techniques are unsuitable, even misleading in a situation that contains unknown. Does this mean that in the unknown, economic calculators should give up any form of computational approach and only rely on intuitions? Or could we contemplate a computational technique that isolates and assesses economic effects in the unknown? This research work provides some insights to these questions by studying economic phenomena in the commercial aircraft industry, where economic calculation is massively used (profitability analyzes, costing analyzes...), and by mobilizing design theory models which account for the emergence of the unknown.

A first research axis investigates the impacts of the unknown in rule-based design. We seek to understand the extent to which the unknown may impact a product development cost evolution dynamics by simulating the relationship between functional expansion and development cost escalation. We find that functional expansion introduces shocks in product architecture, the cost impact of which may be dramatic. And we study how well-known cost management techniques (modularization, anticipative overinvestment) could reduce costs under functional expansion. This leads to discuss how the misuse of classic cost reduction techniques under functional expansion could generate industrial catastrophes. A second axis focuses on investment cases whose huge economic returns seem incomprehensible in a decision-theoretic framework. We demonstrate that the unknown has played a role in the generation of these returns. And we show that specific methods and managerial principles were used to rigorously explore the unknown and generate such returns.

The outcomes of this work suggest that the unknown can be managed as an economic variable in its own right, which raises further questions, namely about the kind of organizations that could deploy and operate 'economic calculation in the unknown'.

## KEYWORDS

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Development costs ; investment decision ; unknown ; functional expansion ; generativity ; learning