Space Policy 30 (2014) 83–90

Contents lists available at ScienceDirect

Space Policy

journal homepage: www.elsevier.com/locate/spacepol

Space science innovation: How mission sequencing interacts with technology policy

Zoe Szajnfarber

George Washington University, United States

A R T I C L E I N F O

Article history:
Received 11 July 2013
Received in revised form
26 March 2014
Accepted 26 March 2014
Available online 29 April 2014

Keywords:
Innovation policy
Exploration vs. exploitation
Space technology management

A B S T R A C T

Innovation is fundamental to a space agency's mission. Yet, the industry's current dominant approach to new technology development – concerted investment in step-changes in capabilities to support a particular application – contradicts the conventional wisdom of innovation theory. In order to understand why, this paper uses a unique empirical case study, in which exogenous historical circumstances created unexpected additional opportunities for technology investment, to explore the merits of this approach. The value of follow-on periods of R&D is assessed in terms of simple marginal returns, implications for workforce dynamics and the interaction of mission sequencing and technology strategy. The analysis reveals an important contingency between mission paradigm and the value of follow-on investment. Specifically, while marginal performance improvements can be achieved at lower costs, their value depends on the availability of an appropriate mission opportunity. In the current paradigm, the risk of obsolescence is high compared to the potential benefit. However, if a new small mission, frequent flights, paradigm were to take hold, there may be great value in refocusing R&D strategy on later round improvements.

1. Introduction

Core to a space agency's mission is the mandate to push the limit of what is technically possible.[1] With fewer and fewer mission slots available in the space sciences,[2] there is a perception that future missions may not even be considered if they cannot promise performance specifications of at least an order of magnitude better than the previous generation. As a result, hundreds of millions of research and development (R&D) dollars are being invested, over the course of decades, to support these revolutionary improvements in performance even though they may only be used once.[3] By the time the next functionally similar mission is approved, the incumbent capability will have been superseded by the outputs of the next major investment.

While this approach to technology-enabled scientific discovery can yield impressive step-changes in capability, it contradicts the conventional wisdom of innovation theory, and may thus be ineffective in leveraging the value of the agency's investment. Outside of the space sector, it is well understood that sustainable technology development requires a combination of both exploration (uncertain search for novel solutions) and exploitation (process oriented improvement along an existing technology trajectory) [5]. In most industries, firms tend to err on the side of too much exploitation, eventually stagnating when the limits of the particular technology trajectory have been reached. However, exploration to the exclusion of exploitation, like that done in the space sector, can be limiting too, because of its inherent inefficiency. This is because the first prototype is, almost by definition, going to be more expensive, on a per unit basis, and have lower performance than will future iterations. As a result, if second, third and *n*th units are never produced, there will be a) no basis for averaging down R&D investment costs across the full production run and b) no benefit accrued from marginal production improvements.

But how inefficient is it really to fly space science missions with (scientifically) revolutionary capabilities only once? Are the between-generation step-changes in performance that are associated with the "Exquisite Science" model the only way to achieve scientific returns in the current budgetary environment, or would

---

E-mail address: zszajnfa@gwu.edu.

[1] For example, NASA seeks "*to pioneer the future in space exploration, scientific discovery and aeronautics research*" and ESA's purpose is to "*promote […] cooperation among European States in space research and technology and their space applications, with a view to their being used for scientific purposes and for operational space applications systems.*"

[2] Compare for example the plans outlined in subsequent NRC decadal surveys Refs. [1–3].

[3] Based on multiple interviews with experienced scientists and NASA and ESA (c.f. [4]).

http://dx.doi.org/10.1016/j.spacepol.2014.03.005

structured sequences of less ambitious increments be more cost effective? This debate over the merits of flying a few large missions vs. many small missions has taken several forms over the years. It has been discussed in terms of risk and survivability (e.g., as part of "responsive space" and more recently the "disaggregation movement"[4]), and also, from the perspective of reliability and technical obsolescence (c.f., [7]). Arguments on each side of these debates have typically been supported by a combination of logic and stochastic models, rather than empirical evidence. To truly push the discussion forward, hard data, which can be openly debated, must be sought.

To that end, this paper presents a detailed case study that provides insight into one part of the discussion: it unpacks the value associated with second and third round R&D investments in a particular technology trajectory. Specifically, the 3-decade history of X-ray microcalorimeter development at NASA offers a unique *empirical* setting through which to examine the above questions. While this case is certainly not representative of all technology development in space science, attributes of its history make it ideal for unpacking the specific dynamics of interest: originally developed under the exquisite science model, two mission failures (exogenous to the technology investment decisions) created second and third opportunities to continue development activities. These events provide a rare lens into the counterfactual: enabling us to ask what would have happened if the mission opportunities had been structured differently.

This study asks, from the perspective of investment in technology development, whether it is better to a) expend concerted development effort to support exquisite science on one-off flagship missions or b) seek more continuous improvements that are realized over a sequence of individually less ambitious missions. Working from a single case study, it would be inappropriate to provide a definitive answer. Rather, this paper contributes a rich, empirically grounded, discussion of the tradeoffs inherent in the two approaches. It finds that "value" and "efficiency" of technology investment in this context depend strongly on the mission paradigm. As long as the current paradigm of infrequent, extremely ambitious, missions persists, step-change investments are much less inefficient than innovation theory would predict. However, if the trend towards smaller, more frequent, flights takes hold, there may be enormous value of a renewed emphasis on exploitative technology investments.

The remainder of this paper is structured in five sections. Section 2 reviews the relevant literature, building a theoretical basis for the analysis. Section 3 describes the methodological approach taken. Section 4 highlights the core dynamics observed in the central case history. Section 5 discusses the implications of these dynamics for how technology investments should be structured to support innovation, and Section 6 concludes the paper.

2. Theoretical context

One of the fundamental concepts underlying the discipline of innovation management is the notion of s-curve shaped growth [8], illustrated in Fig. 1. Mathematically, an s-curve embodies exponential growth, constrained by some physical limit. Theoretically, it captures the concept that initial progress is slow, however, as related knowledge and infrastructure accrue, the pace of improvements increase exponentially until some inflection point, when the facility of improving along the particular trajectory is outweighed by the limits of remaining potential improvements; at this point, the curve plateaus [9]. This phenomenon of constrained exponential growth has been widely documented in diverse industries, noting that while particular curves stagnate, functional areas seem to grow without bound [10]. In this framework, the only way to continue progressing in technical performance is to move to another s-curve, associated with a radical innovation. Since the "next" s-curve will undoubtedly follow the same characteristic slow-fast-slow trajectory, and often yield worse performance initially as its momentum picks up, firms often fail to recognize the need for strategic reorientation, until it is too late [11] (i.e., they have stagnated on the plateau, while competitors working on the next s-curve are reaching their inflection point).

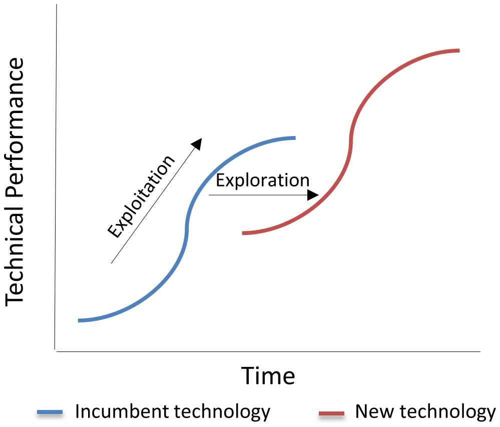

Fig. 1. Notional S-shaped improvement in performance.

It is well accepted in the literature that sustained growth (at the industry level) requires both exploration and exploitation [5,12–14]. Exploration involves basic R&D that is essentially searching for the next s-curve. Exploitation involves structured improvements along a known technology trajectory, moving up the current s-curve [5,15]. However, studies suggest that characteristics of a firm which enable exploration tend to limit exploitation, and vice versa, [16] since they are mutually contradictory and self-reinforcing pursuits [5]. While it is difficult to both explore and exploit within the same organization, failing to do so guarantees failure, either through stagnation (too much exploitation), or lack of cash flow (too much exploration).

Two strategies for combining exploration and exploitation have been proposed in the literature. So-called ambidexterity [14] advocates for combining of exploration and exploitation through loosely coupled organizational sub-units integrated by top management. Punctuated equilibrium, on the other hand, suggests that the contradictory functions of exploration and exploitation can be balanced through temporal sequencing (e.g., long periods of exploitation, followed by short bursts of exploration) [17,18]. At the project level, punctuated equilibrium can also be conceptualized as cycles of convergence and divergence [19]. Despite extensive study, there remains limited consensus on how the competing forces should best be balanced.

While these dynamics apply to the NASA context, as a monopsonist (single buyer in the relevant market), the agency is not subject to the same forces of creative destruction [20] experienced in more traditional markets [21], with implications for the dynamics it faces. The agency must still innovate (both through exploration and exploitation) to stay relevant, but is not at risk of being disrupted by a new entrant.[5] As a result, market forces will

---

[4] See for example Ref. [6].

[5] This is becoming less true in some market segments (e.g., launch vehicles), but is certainly still true within the science directorate.