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Organizational Dynamics and the Evolutionary Dilemma between Diversity and Standardization in Mission-Oriented Research Programmes : An illustration

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Abstract

The American NASP programme — National Aero Space Plane — is a good illustration of the evolutionary dilemma between variety and standardization in the management of mission-oriented R&D. This dilemma relates to the trade-off between the need to explore the technological diversity in order to avoid the risk of being locked-in on the wrong technological option, and the need to share the knowledge produce through the experiments. In this regard, two main organizational designs can be considered:

— the « mainlining » strategy gathering all the partners in an « club », exploring the potential of one alternative, allowing the sharing of knowledge, and

— a network of simultaneous competing technological projects, allowing a synchronic exploration of the technological variety

The NASP programme was dedicated to the design of radical technology innovation system, and then was basically characterized by a structural uncertainty arising from the structural change it involved in the technological basis. In this case, the lack of guide mark resulted from technological discontinuities in the innovation process. Moreover, the research activities were impeded by strong indivisibilities in the research outcomes needed for the design and demonstration of an hypersonic airbreathing propulsion system. This situation was due to the specific properties of the knowledge about hypersonic technology — strong compacity, low scalability and low analogic connections with other scientific and/or technological fields. This creates a strong need for the production of new infratechnologies, instrumentalities and research infrastructures, i.e. infrastructural knowledge and infrastructure facilities. In this case, the adoption of the “mainlining approach” in the management of the programme can be justified.

Key-words : NASP Programme, Mission-oriented research programmes, Basic Research, Organizational dynamics, Diversity, Standardization, Structural Uncertainty, Hypersonic airbreathing propulsion technology, Scramjet, Infratechnologies.

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Emmanuelle Conesa

Introduction

The growing globalization of technology and the deeper integration of the world economy have generated an ongoing debate in economics focused on the role of the policy makers in the technological change dynamics and questioned the efficient design of national technological policies (Branscomb & Florida [1997], Fransman [1995]). As Branscomb [1993] stated :

> « Economic competitiveness will no longer be left to a *laissez-faire* economic policy ; government will share costs of base technology development with commercial firms » (Branscomb [1993], p. 7).

It prompts a rethinking of the rationale of mission-oriented research programmes. Mission-oriented research programmes are generally initiated by the policy maker, in partnership with the industry, in order to impulse R&D and advance technical innovation in high technology industries. Are these national technology programmes doomed to fail or can they be justified ?

The aim of this article is to examine this argument critically with reference to the American NASP programme — National Aero Space Plane — devoted to the demonstration and development of hypersonic airbreathing propulsion technologies for aerospace applications.

Traditionally linked with the post Second World War technology policies in industrialized countries such as the United-States or France, mission-oriented research programs are known to have usually failed (Ergas [1987]). The American SST - SuperSonic Transportation program and the much more controversial project of Concorde[1] are some famous examples of failures. In the economic literature on technical change, it has frequently been suggested that those mission-oriented programs are too costly and tend to survive failure. Most of the economists criticize heavily :

- their centralized organization (Cohen, Edelman & Noll [1991]);
- their high cost that encourages a narrowing in the range of the options explored (Collingridge [1991]) ;
- and their technical complexity that restricts participation in program execution to a few, technologically sophisticated agents, (see Ergas [1992], p.3).

Then, the economist faces an interesting paradox between :

- On the one hand, the policy makers' unanimity to adopt mission-oriented program as an instrument of technological policy during the last half century, and,
- On the other hand, the economists' unanimity to inform against the high probability of expensive failure (Cohen, Edelman & Noll [1991]) that can survive a long time.

However, looking at those mission programmes through the evolutionary glasses reveals that their organizational management is challenged by a dilemma between variety and standardization in the learning process (Cowan & Foray [1995]), in a context of technological discontinuities. Such a trade-off between variety and standardization corresponds to the evolutionary dilemma between exploration and exploitation (March [1991]), and between static and dynamic efficiency (Klein [1977]). Exploration is linked to experimentation, flexibility, discovery, innovation and dynamic efficiency, while exploitation corresponds to choice, selection, implementation, and static efficiency. The development of a pure strategy of exploration causes the system to suffer the costs of experimentation without benefiting from the diversity generated simply because of a lack of coordination. The risk associated with a pure strategy of

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[1] The case of Concorde is quite different from the American SST failure, as the project has been recognized to be a commercial failure, not a technical one.

exploitation is to become locked-in a suboptimal state. Thus, the stake is to maintain an appropriate balance between exploration and exploitation as a necessary condition to keep the dynamic efficiency in the long run.

To avoid the risk of being locked-in on a wrong technological option, the policy maker should adopt a network approach based on the conduct of decentralized parallel experiments in order to explore simultaneously the technological variety. But such programs deal with complex technology challenges that involved basic research activities to remove the knowledge frontier. Thus they are characterized by structural uncertainty. To decrease this structural uncertainty, it is needed to create common pools of knowledge about the radically new technology as it was the case in the American NASP programme, a mission-oriented programme dedicated to the conception of a hypersonic space plane.

Such characteristics call for clubbing together the research partners of the program in a single entity in order to produce the standard knowledge required to explore sequentially the technological variety.

Clearly in this case, the organizational dilemma between learning from diversity and learning from standardization can be solved only by taking into account the specific characteristics of the research stake.

1. The case of the National AeroSpace Plane Program

The NASP was a three-phase research, development, test and evaluation program — RDTE — undertaken in the mid of the eighties, to develop and demonstrate hypersonic and transatmospheric, single-stage-to-orbit — SSTO — technologies that will support national security and commercial applications and could provide economies in space launch costs (Augenstein & alii [1993).

Low-cost — or at least affordable — access to space satisfying NASA and US' strategic plans was the primary goal of the NASP program. The focus was then to reduce the cost of space access to hundreds of dollars per pound and provide the core research and technology needed for the next 25 years ; i.e. advanced technologies in aeronautics — transatmospheric — and space transportation systems (Barthelemy [1989]) so as to enable the American aerospace launch vehicle industry to compete in a global market.

The main purpose of the NASP was a multi-year technology demonstration effort to show that a range of technologies — including airbreathing propulsion, advanced aerodynamics, materials and structure, fuel systems, avionics, and the computational fluid dynamics — could lead to the development, fabrication, and flight testing of an experimental flight vehicle called X-30. Critical technologies concern the propulsion system.

The research effort was strongly focused on the design of a new airbreathing propulsion engine : the scramjet. This propulsion system would allow the design of a space aircraft capable to attain satellite speeds and to reach orbit, having take-off like a regular airplane, and returning to the earth once its mission is accomplished. Space station re-supply would be far cheaper than with systems requiring launching of rockets that cannot be recovered or reused. An aerospace transport vessel, in contrast, could be flown repeatedly. Another application of the technologies would have been the production of a civil transatmospheric transport vehicle travelling at Mach 8. The starting point was that a hypersonic transport might prove an attractive option for the long-distance market in the next century. It could fly at altitudes of 20 miles or higher and at five times the speed of sound or greater. Travelers would reach far distant destinations within two hours.

In order to manage the NASP program it was necessary to progress in advancing structures, thermal protection systems, propulsion, and vehicle technology in the primary area of advanced reusable transportation technologies. The NASP programme was supported by a strong optimism : «We have the capability to integrate these technologies in the experimental X-30, which should begin validation in actual flight by the early 1990's » (Executive Office of the President, OSTP [1987], p.10).

The requirement to fly forces advanced vehicle concepts and related technology efforts, such as the X-30 lifting body and propulsion system to become more integrated and to place additional focus on system technology demonstration.

1.1. Inflexible Technology and the Risk of Costly Failure

But some people criticized this ambitious enterprise as Collingridge [1990] who claimed :

> « The national aerospace plane will do nothing to promote the technical diversity of the space programme. It is likely to repeat the errors of the Shuttle, having such large sunk costs that it will have to be used intensively » (Collingridge [1990], p.197).

According to him, the adoption of technologies involving large-scale shifts away from the *status quo* cannot permit low-cost control of technology or lead to successful performance in policy.

The NASP has all the indicative properties of what he describes as complex inflexible technologies (Collingridge [1992]). As he has sought to explain, the degree of inflexibility of the technology allows to account of its non-incremental nature. This inflexibility, which makes the development of the technology peculiarly prone to costly errors, has been shown to obtain where the technology in question possesses at least the following four characteristics :

(1) large-unit size;
(2) long lead-time;
(3) high capital intensity;
(4) dependence on specialized infrastructure.

Despite some unique technical and functional merits, such as recovering satellites from orbit while at the same time being reusable, NASP might proved a hugely expensive failure because (I) it represents a large change from anything that existed earlier, and (ii) the risk of failure results in its development and operation under a decision making process of considerable centralization. Whatever the route chosen, many regard such an organization as positively inimical to fostering the flexibility that is necessary to search for an “optimal” design.

Following the doctrine of Incrementalism (Collingridge [1990]), technologies should be developed in a piece-meal, experimental way involving a series of trials. This demand is most easily met when decisions are made in a decentralized, pluralistic way. By contrast, the development of an inflexible technology involves highly centralized decision making dominated by large organizations, able to transfer risk in some way to government, thereby excluding many legitimate stakeholders. Thus, the policy maker should look for technical alternatives that are more flexible, some of which may be developed in a more decentralized way. Those arguments are consistent with the analysis of Cohen, Edelman & Noll [1991].

1.2. Organization of the NASP Program : A Matter at Issue

Cohen, Edelman & Noll [1991] described the organization of NASP as being similar to the great Japanese government-industry collaborations of the 1970s and the 1980s (see Fransman [1990]). NASP was drawn out of the American industrial-government structure to form a new entity that incorporated expertise from both the public and private sectors that, previously, had been dispersed throughout the national research system. Innovative partnership has been formed that strengthens the alliance between industry and the government, thus enabling the costs and risk sharing. The members of the “club” were Mc Donnel Douglas, General Dynamics, Rockwell, Pratt &Whitney, and Rocketdyne. The National Program Office, managed by NASA and the Department of Defense, assumed the function of program coordinator.

According to Cohen, Edelman & Noll [1991], NASP’s organization — described as an “innovative team” approach — had two drawbacks. First, the grouping of all available expertise into a single entity in fact narrowed the range of alternative development paths that were explored. Consideration of a wider range of technological options that could have catalyzed broader industrial involvement in the program was, therefore, inhibited.

Second, « The innovative team approach [made] the program more difficult to kill if NASP [became] nothing more than an expensive toy. Involving all of the important players in the aerospace industry eliminates short-term sources of political attack because it [picked] no winners and has no competitive external R&D effort. Involving multiple government agencies creates a stable support coalition within government » (Cohen, Edelman & Noll [1991], p.53). Cohen & alii have further suggested that the choice of organizational centralization was more a reflection of political pressure than of any attempt to achieve technical and economic optimization. They claimed that this organizational design increases the likelihood of failure, i.e. it raises the risk of missing the best design, and the cost of the event of failure.

Certainly in this case, the centralized organizational structure of the NASP program *a priori* strongly impedes the exploration of the requisite diversity. From an economic point of view, such a curtailment of diversity is bound to lead to reduce resource-allocation efficiency in the relevant research area. What can be said in response to the above evaluation?

The critique appears to be relevant : a unified organizational form increases the risk of missing the best design. It also increases the cost of a possible failure regarding the selected technological trajectory. Nonetheless, the adoption of such an organizational design can be justified, as far as it is taken into account of the effective tasks performed in the course of the research program, and in particular the need to conduct basic research activities in order to reach the radically new technological goals.

2. Exploring the Technological Diversity

The NASP program was clearly facing a situation of structural uncertainty[2] about the performance and function that the hypersonic and transatmospheric technologies could assume in the future. Consistently it would render the necessity to scan the technological variety, i.e. a large number of possible “design candidates”, before any commitment is made to a particular system design. Therefore, facing such a structural uncertainty, the rationality in the organizational management of the NASP should have led to incremental development, flexible management schedules, consistent with the exploration of different technical paths (Cohen, Edelman & Noll [1991], p.53).

This implies a form of organization that will exploit the virtues of diversity and that will promote option generation and facilitate experimentation along different trajectories. According to Ergas [1994], option generation refers to the process by which alternative design approaches are developed, tested and selected. The efficiency of option generation is greatly affected by the range of alternatives being explored and by the speed and integrity with which the results of exploratory efforts are transmitted within the technological community involved. Both contribute to the learning process of technological variety.

In the case of NASP, one should aim to create a system capable of handling multiple, decentralized projects, such procedure of investigation being one way to explore a broad range of the possible technological and functional spectra. The final orientation of the entire program toward a single, predetermined area should have been decided after the completion of many pilot projects and a broad base of experimentation (Cohen & Noll [1991], p.42, David & Rothwell [1996]). Such an organizational form is, however, difficult to establish, on one hand because of the need to arrange some form of financial compensation for those projects not selected — the technological orphans (David [1987]) — and on the other hand because the information generated by the different experiments must be *effectively shared*. In other words, such a system must include mechanisms and procedures for exchanging and distributing information produced in the course of individual projects, while, at the same time, it must be centralized enough to assess options, decide upon the timing, and select the

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[2] Drawing from Shackle [1972], Langlois [1984] established a typology of different type of uncertainty. He distinguished the parametric uncertainty from the structural one. Parametric uncertainty refers to a situation of parametric change, that is, change of certain known variables within a known framework. At the most radical extreme structural change designates change in the very structure leading to an indeterminate problem whose set of potential solutions (i.e. states of the world) is unknown, i.e. structural uncertainty.

configuration to be chosen as the standard in the program overall. In that case, it can create an incentive for agents to become "free riders" and avoid the cost of participating in any of the experiments.

Moreover, it is only when uncertainties can be overcome that is it possible to create an environment in which the experience of one agent reduces, rather than exacerbates, the uncertainties of the others and so fosters a process of cross-fertilization in which the final design will emerge.

In the case of the hypersonic programme, the structural uncertainty results from structural change. From the foregoing, here, it is useful to refer to the evolutionary economic approach of technological change dynamics.

2.1. Technological Paradigms and Conventions of Technical Change

In the framework of the economics of technical change, it is commonly referred to the existence of a technological paradigm to explain the regularity in the development of a technological path (Dosi [1988,1984]). This cumulative and path-dependent chaotic process, in which "small historical events" play a crucial role in the orientation of technological trajectories (David [1985]), is relentlessly exposed to irreversibility as a result of the market-driven diffusion process of technologies in the presence of increasing returns to adoption (cf. Arthur [1988,1989], David [1987]). This tendency seems reinforced by the emergence of self-fulfilling prophecies among the technologists, i.e. what I call a "convention of technical change", as a structure of mutually consistent expectations (see David [1994]) about the future course of technical change.

Such a convention of technical change can be described as a set of design parameters, which embodies the principles that will generate both the physical configuration of the product and the process and materials from which it is to be constructed. It refers to the notion of technological regime as described by Georghiou & alii [1986], p.34) : « The basic design parameters are the heart of the technological regime, and they constitute a framework which is shared by the firms in the industry ». It corresponds to the definition of a self-reinforcing institution as is stressed by Vanberg ([1994, p.7) :

> « […] as configurations of interconnected and mutually-stabilizing behavioral routines. They are constituted by routines practices of number of persons that are functionally interlaced and reinforce each other in a mutually-stabilizing manner.»

Such a conventional institution guides the behavior of designers of advanced new technologies, as the famous Moore's Law[3] regulating the technological path in the industry of integrated circuits since the 1960s[4]. This evolution was consistent with what Dosi [1984] called a paradigm that is to say the pattern of technical change in the semiconductor industry mixing four main directions of progress : increasing miniaturization, increasing speed, increasing reliability and decreasing costs. In the era of integrated circuits, increasing miniaturization is a function of increasing *density*, i.e. increasing number of components on a single chip.

> « […] because of the unique nature of the technology, by making things smaller the speed of the circuits increases, power consumption drops, system reliability increases significantly, and, most importantly, the cost of the electronic system drops » (Moore [1996], p.56).

But if the paradigm gives the direction of technical change, the convention specifies the oriented movement by giving precise indications about the *rhythm* and eventually about the *timing* of technical change. The convention operates as a perceived exogenous constraint that defines the orientation and the rhythm of technical change. It can be analyzed as a *self-fulfilling prophecy* : its institutional content reduces the uncertainty about the path of technical change, and its salience helps the coordination of the technological expectations of the engineers in reference to a focal point. It appears to be a powerful driving force for technological standardization.

The convention of technical change plays the role of shared cognitive maps as structures of mutual consistent expectations about the course of technological dynamics. But when an unexpected technological breakthrough appears, one can call such a disruptive evolution a paradigmatic transition. Then, the reference to the old cognitive

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[3] Moore's law is not based on any scientific demonstration. It has been infered by Gordon Moore who observed in 1965 that the number of individual components on integrated was doubling each year since 1959, when the integrated circuit was patented by two engineers from Fairchild Semiconductor Corporation. In an article, published in April 1965 in Electronics Magazine, he extrapolated this exponential growth for another decade and came up with an astounding projection : that the circuits of 1975 would contain some 65,000 devices. Now enshrined as Moore's Law, his prediction has continued to hold true for over three decades, though the doubling period has grown to about eigtheen months. The most advanced chips today contain millions of transistors — each with typical dimensions of less than half a micron. According to Moore, this trend has been reinforced by the specific properties of the technology : « A unique aspect of the semiconductor industry is that prices for products tend to decrease over time. […] Not only does the price fall for a given integrated circuit, but as the complexity of the chip increases, the price per electronics function decreases from product generation to generation as more and more functions are integrated into a single structure » (Moore [1996], p.56).

[4] The semiconductor industry began in 1947 with the invention of the transistor at the Bell Telephone Laboratory. A transistor is the building block of digital logic and memory circuits (Moore [1996], p.55). An integrated circuit (IC) is a device performing more than one function on a single ship, i.e., it embodies more than one component, either active or passive — for example, several transistors connected through patterns 'written' on the chip (cf. Dosi [1984], p.23).

framework is no longer possible. As the technological goal induces a paradigmatic shift, a new set of cognitive and social practices has to merge which are different from those that govern the old paradigm. The economic agents have then to deal with what may be call radical uncertainty.

2.2. New Priorities for Space Launch Vehicles and the Need for Hypersonic Airbreathing Propulsion Technology

In the case of the NASP program, what is important to stress is the fact that the technological objective of building an aerospace plane lies beyond the limits of the performance criteria that have hitherto governed the technological evolution of space launch engines (McLean [1985]).

Since the beginning of the sixties, the development of space propulsion technologies has been shaped through the emergence of a “stable” orientation for technological progress on the basis of criteria associated with cost and industrial implementation. These criteria emphasized the need to deal primarily with the problems of acceleration speed and orbital access. This orientation, rendering the existence of “standard operating procedures to generate technological change”, was compatible with the technological option of rocket engines. Moreover, the incredible magnitude of federal R&D expenditures in aerospace industries combined with the concentration on military-oriented R&D performed by government agencies like NASA in partnership with the Department of Defense, allowed to neglect the industrial potential of aerospace transport systems (Pace [1990], Macauley [1986]), and therefore, make it possible to ignore issues of reutilization, operability, and payload mass.

However, in the case of the hypersonic program, the advanced research objective clearly marked the end of this kind of technological change convention. The challenge was to try to reconcile the different and possibly contradictory sets of performance requirements that were previously applied exclusively, either to aeronautic or to space systems, by trying to unite in a single propulsion system, the economic advantages of airplane engines — cost, ratio to mass, operability, maintenance — and the performance criteria of rockets, in terms of flight speed and orbital access. The development of reusable launch vehicles appeared to hold great promise as the key to unlocking the vast potential of space business exploitation. Unfortunately, while a great improvement over current systems, the cost per pound delivered to orbit for currently proposed systems would still be greater than that needed to exploit space for many business uses. One of the limiting factors in potential reductions for chemical rockets is the Ips limit — specific impulse.

The change in the performance criteria has involved the conviction that the technology portion of the program should concentrate on airbreathing propulsion technologies. The ultimate success of this project depended, first, on solving the propulsion problems associated with the use of airbreathing engines[5].

Airbreathing propulsion technology offers substantial advantages for hypersonic flight, notably :

- The use of airbreathing engines holds potential for very significant increases in Ips which could result in a significantly lower cost per pound to orbit : an improvement in mass ratio of the order of 3 to 5 in comparison with the mass placed into orbit by non-airbreathing propulsion — rocket engines —, made possible because airbreathing engines utilize air as the combustive agent, removing the need for mass-loaded oxygen for combustion in rockets ;
- The possibility of vehicle reutilization, thus eliminating costly replacement or in the best case, the recovery of the space vehicle which means — as for the American Space Shuttle — reconfiguring and refurbishing the vehicle after each flight — the so-called refurbishment phase ;
- The ability to take off and land horizontally as well as the elimination of auxiliary solid fuel rockets and other types of launch support ;
- Adaptation of maintenance practices that are closer to the airplane ones than to the rocket maintenance, therefore requiring less retraining of the ground staff.

This contrasts with a rocket-powered vehicle’s operational penalties — such as large infrastructure requirements—, and its need to transport its own oxidant for combustion exacts large payload penalties. As a result, airbreathing propulsion is an essential ingredient for sustained endoatmospheric hypersonic cruise applications such as “global reach” vehicles, and can significantly improve the performance of space launch vehicles.

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[5] The aerobe or airbreathing principle is distinguished by the utilization of oxygen from air — taken up from the atmosphere — as the combustible agent, whereas rocket propulsion requires the loading of both fuel and combustible agents — anaerobe or non-airbreathing principle.

At the sought-after flight speeds — beyond Mach 5 — supersonic combustion becomes necessary[6]. In other words, the pacing airbreathing hypersonic technology is certainly the scramjet engine. The tricky problem of developing the existing propulsion technology — called ramjet — into a fully functioning supersonic combustion ramjet — called a “scramjet” — is thought to require a critical combination of the airbreathing principle with the hypersonic speeds. Nevertheless, airbreathing ramjet/scramjet — supersonic combustion ramjet — engines could improve mission effectiveness by reducing on-board propellant load in favor of payload and by increasing operational flexibility.

2.3. Presumptive Anomaly as the Dynamo of Paradigmatic Shift

According to Constant [1973], a paradigmatic change is precipitated by the intuition of the occurrence of what he calls a “presumptive anomaly”. A presumptive anomaly arises in the existing analytical framework from the scientific evidence that conventional technologies cannot perform some new missions and/or reach new levels of performance. This means that attempts to extend the existing paradigm to a new set of problems are expected to fail to provide “satisfactory” answers. In time, this generates the “presumption” that the existing paradigm is fundamentally flawed and stimulates a search for new ways of looking at things.

In the case under consideration, the anomaly is expressed in terms of a growing conviction —or evidence — that conventional airbreathing propulsion systems will not function at hypersonic speeds. The existence of this presumption prompts the expectation of further possible technical/functional anomalies in design that also creates pressure for a new paradigm.

Historically, the fastest airbreathing engine-powered airplane, the SR-71, can cruise just above Mach 3, about 60% of the Mach 5 transition to the hypersonic regime. Ramjet powered vehicles have flirted with the hypersonic threshold. History’s only hypersonic airplane, the Mach 6.7 X-15 of the 1960s, used only rockets — as have all space flight launch vehicles to date, the expendable ones and the reusable Shuttle alike.

It is argued by some that there is no point in trying to design a hypersonic jet on the basis of a technology — airbreathing propulsion — when existing science suggests that the principle cannot be applied at hypersonic speeds.

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[6] Hypersonic speed is obtained from supersonic combustion, just as supersonic speed is obtained from subsonic combustion in a ramjet.

Thus, the presumptive anomaly, though it arises primarily within science, brings in its train a technical/functional anomaly and that affects, adversely, progress in design and development. Despite the fact that there is no functional failure here, the presumption of a theoretical anomaly spills over to the design dimension and constraints the full momentum of development in the hope that analytical work will one day be able to clarify the situation.

In brief, such a complex change implies discontinuities, both scientific and technological. The two traditional supports for the elaboration of new technological designs — scientific models and the design experience of preceding technological generations, the supersonic "ramjet" — cannot be used effectively here, because they provide only certain, very limited guidance. These can be seen in the case of hypersonic flight — first, in the difficulty of developing predictive models and second, in the inability of previous experience with "ramjet" to compensate for the absence of these models. Progress, apparently blocked on both fronts, spurs on the search for a new paradigm.

The challenge depends not only on the allocation of financial or human resources, as important as they are. The main difficulty lies in the lack of a sufficiently robust analytical framework to guide both research activity and technological design. As a matter of fact, designing a hypersonic aircraft requires the exploration of a new paradigm to solve the propulsion issues.

It is now described in greater detail why the design of a scramjet requires a fundamental change in technological paradigm.

3. Technological Discontinuities as a Source of Structural Uncertainty

On the basis of the above mentioned considerations, the NASP program was clearly facing a situation of structural change as the building of a hypersonic space plane renders the transition to a new technical change convention. In turn, this transition through new performance criteria has induced a paradigmatic change regarding the technological basis.

The structural uncertainty of the NASP program was due to the indetermination faced by the research partners that concerned both the ways one should carry out the research as well as the finality of the research in terms of application of the results.

3.1. Difficulties with Experimentation and the Lack of Scientific Data

In the case of supersonic combustion — Mach 5-6 —, the first difficulty is that it is almost impossible to produce the ground-based scientific data needed in order to validate the “scramjet” concept and predict its performance in a particular vehicular form. Indeed, ground-based test capacities and experimental installations – i.e. technological infrastructures — do not yet exist for vehicles flying beyond Mach 8. There are no installations capable of reproducing the combination of speeds, pressures, and temperatures necessary to stimulate hypersonic flight. In addition, ground-based experiments are of extremely short duration. For example, hypersonic wind tunnel tests generally last less than a few seconds because of the great quantities of energy required. Suitably sized installations are needed for the experimental verification of propulsion and aerodynamics concepts beyond Mach 8 (US GAO [1988], Sullivan [1991], Piland [1991]).

This weakness in experimental apparatus can be partially overcome by using computational simulation methods. Here, however, the scientists faced two difficulties : the absence of predictive law for the modeling of turbulence in the study of laminar flows and the difficulty of solving the supersonic combustion equations (Harsha & Waldman [1989], Bogue & Erbland [1993]). The latter requires substantial computer power because of the long calculation times involved. All simulations, therefore, need to make a significant number of approximations, but even if theses can be justified they do not eliminate the need for experimental tests. Nonetheless, simulations do enable researchers to limit wind tunnel tests to those precise areas where simulations alone are either too difficult or do not provide sufficiently precise results. Simulations may reduce the quantity of experimental work necessary, but they do not eliminate it altogether.

In the final analysis, the current difficulties of ensuring synergy between simulations and real tests reveal that science is still far from being able to provide predictive models on which a design configuration might be based. If further research is blocked by lack of theoretical guidance, could not this weakness be, at least partially, overcome by using other sources of information, such as concepts and design ideas inherited from previous technological generations?

3.2. The Gap between the New and the Previous Technical Regime

The required paradigm change is driven in part by the fact that the results obtained at the threshold of Mach 5 are no longer valid beyond Mach 5, For example, certain physic-chemical laws are reversed as velocities pass from the supersonic to the hypersonic domain (Barthelemy [1989]). Beyond Mach 5, air no longer behaves as a perfect gas ; beyond Mach 8, properties dependent upon temperature and even dissociation phenomena become dominant : « as a result of kinetic chemical phenomena of increasing significance, simple extrapolation parameters no longer exists which can be applied to the domain of supersonic combustion.» (Barthelemy [1989]).

Here it is useful to refer to the notions of homotopic and non-homotopic mappings — or correspondences —, analogic links, and technological lumpiness developed by David, Mowery & Steinmueller [1992] for the purpose of assessing the potential of "transferability" of knowledge generated by one basic research program to another, not necessarily basic research program (Conesa [1997]).

The economic analysis of the payoffs of basic research outcomes, due to David, Mowery & Steinmueller [1992] explores the implications of the R&D externalities in the symptomatic case of the physics of high energy particles, and tries to assess the role played by the spillovers in basic research. Such a framework focuses on the informational outputs of basic research and the connections among these outputs, applied research and innovation. It emphasizes the interaction between basic and applied research activities « […] as the ultimate source of the economic benefits of basic research» (David, Mowery & Steinmueller [1992], p.80).

They started from the assumption that the number and richness of links between the knowledge generated by basic scientific projects and other scientific and applied research endeavors are important determinants of the potential economic returns from discoveries in a specific discipline.

They distinguished between two types of links, "homotopic mappings" and "analogic links". The first ones refers to scientific information that is potentially applicable to problems quite far removed from those of concern in the original inquiry. Such information is said to be homotopically mapped to different scientific or applied research problems. The conclusion is that once a theory exhibits such homotopic mappings, progress in other fields of basic and applied research can focus on issues of practical implementation rather than on the discovery of new phenomena. This notion helps to anticipate the pace and impact of progress within a scientific field in which the examination of a portion of an entire system of interrelated phenomena provides useful generalizations and applications in other areas. The analogic links between knowledge from basic and applied research « are based on the surmise that nature is conservative in the use of concepts and structures, and posit that physical regularities in one field underlie other natural phenomena » (David, Mowery & Steinmueller [1992], p.85)[7].

Thus, the existence of "homotopic mappings" or "analogic links" help to delimit the area of application of the results obtained in basic research. Such an economic analysis highlights the existence of discontinuities , beyond a certain threshold, in the validity of knowledge produced in the study of physical phenomena. The lack of scalability[8] resulting from the absence of homotopic correspondences, means that the results obtained can not be extrapolated to another range of size. Each of these notions has implications for the empirical examination of mission-oriented basic research programs. The indivisibility of research activities, what David, Mowery & Steinmueller [1992] called the property of "lumpiness", is an additional characteristic of basic research projects. It may influence the formation of these homotopic mappings and analogic links.

First, in the transition to the hypersonic domain, the homotopic correspondences[9] between the concepts developed at different velocity levels are weak. This means that extending existing concepts cannot bridge the discontinuity between the supersonic and the hypersonic domains by additional, modest improvements in existing facilities and human resources. Further, the "analogic links" between older rockets and the newer airbreathing propulsion technologies are relatively insignificant (Conesa [1997]). This means that there was only a limited number of opportunities to transfer practical design and development experience from one domain to the other : Harsha & Waldman [1989] emphasized that « The installation requirements for aerodynamic experimentation and propulsion systems appear to be quite different depending upon whether they concern the development of a shuttle or a scramjet demonstrator.»

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[7] The concept of symmetry, applied in mathematics and physics, as well as chemistry and crystallography, is a good example of an analogical link allowing for the extension of theoretical results from one domain to another (cf. David, Mowery & Steinmueller [1992]).

[8] Scaling is a way of dealing with different levels of aggregation. The main implication of scalability is that it is possible to move between different levels of aggregation.

[9] The methods and results may or may not be extrapolated to every size of range. The notion of "homotopic correspondence" comes from topology : two correspondences are said to be homotopic if one of them can be deformed continually within the other. This, in mechanics, a theory predicting the reaction of a physical object to attraction by an external force will be true for any object of greater mass. The relationship between force and mass is unaffected by changes in the mass parameter (cf. David, Mowery & Steinmueller [1992]).

Finally, weaknesses in both the homotopic correspondences and analogic links imply that new facilities will be needed, and this creates a degree of "technological lumpiness" and new, large-scale investments in facilities and information are bound to alter the expected economic returns from the program. The property of "lumpiness" is derived from the fact that the production of new results requires the prior resolution of a greater or lesser number of sub-problems in the research area. This lumpiness may be either informational — the minimum of sub-problems to solve — or material — the minimum of required experimental installations. This property is no doubt particularly pronounced where the homotopic correspondences are weak (cf. David, Mowery & Steinmueller [1992]).

4. The Need for Technological Infrastructures and Organizational Integration

The fact that a problem is perceived in terms of paradigmatic change implies that researchers believe that accumulated knowledge and experience on its own provides insufficient guidance as to how proceed. As a consequence, experts can argue that because the existing science has no guidance to offer in the new domain, it is imperative to push further the scientific agenda before exploring the hypersonic technologies' applications. In this way, it effectively puts a block on both further experimentation and organizational innovation.

It can be argued that, before the bottleneck can be broken, there is a need to build up a new technological base — that is to gather data and develop the methods of investigation, i.e. the infratechnologies needed, and to produce the techniques and research infrastructures and instrumentation that will form the basis for establishing the research agenda before choosing a design configuration.

4.1. The Very Nature of the Research Outputs : The Production of Infratechnologies as a First Priority

Indeed, the situation outlined in the previous section highlights the need for new infratechnologies (Tassey [1991,1996]), experimental methods and instrumentation in order to make progress on the design and development of a hypersonic airplane. Following Tassey [1991], the infratechnologies can be defined as the instrumental basis of R&D, including :

> « the scientific data necessary for operations of measurement, test control, and trial ; methods and research instruments, techniques, and knowledge. Infratechnologies are the basis of technological development in that they enable precise

> measurements and furnish scientific and technical data, evaluated and organized, necessary to the understanding, characterization, and interpretation of pertinent research results. Infratechnologies are linked to the basic units of measure. In addition, infratechnologies incorporate the concepts and techniques of measurement and testing which allow for increased quality.»

The absence of this technological basis makes it extremely difficult to identify which particular strategic research or design questions need to be addressed. This, in turn, creates what is referred to as a situation of uncertainty within the program itself. In other word, the instrumental basis for R&D within a new paradigm is completely lacking. In the case of the NASP programme De Meis stated : « Lots of things need to be measured that we do not know how to measure » (De Meis [1990], p.34). Thus, analyzing the content of the research carried out enables us to define the hypersonic programs as “oriented toward the production of adequate infratechnologies and instrumentalities” required for the achievement of the exploration of the hypersonic propulsion area.

According to Rosenberg ([1992], p.385),

> « Scientific instruments may be usefully regarded as the capital goods of the scientific research industry. That is to say, the conduct of research requires some antecedent investment in specific equipment for purposes of enhancing the ability to observe and measure specific categories of natural phenomena.»

This phase of research is crucial and has to precede whatever basic and applied research activities will eventually be undertaken. It contains a strong technological dimension. As a result, the appropriate organizational form should be subject to the requirements of collective production of the infratechnologies and research infrastructures needed to create what amounts to the “conditions of possibility” of taking the project forward. Because these conditions will influence both subsequent research and design considerations, they also constitute the “collective” dimension of the project.

Then, the difficulty is to produce simultaneously infrastructural knowledge of two type : the first type deals with the production of infratechnologies and instrumentalities required by the paradigmatic shift. To be infrastructural, this type of knowledge must be collectively used and, thus, has to be public and diffused in “codified” form. The second type deals with the information generated and diffused by the different experiments. To be infrastructural, this second type of knowledge must be shared and possess a strong “public good” aspect, that is to say being persistent[10], and exhibiting non-rival and non-exclusive properties.

4.2. Creating Common Pools of Knowledge

It is clear that the NASP program with its strong technological composition has been — effectively — in just such a preliminary phase (Bogue & Erbland [1993]). How then does one produce infratechnologies and instrumentation that will constitute the new technological base, given the weakness of scientific support, and the discontinuities marking the transition from the supersonic to the hypersonic domain? The first objective has been to push back the frontier of experimentation “on the ground” rather than “in the air”[11], so that to produce the experimental infrastructures required to pursue the ground tests. Second, it has been necessary to develop computer simulations — numerical simulation or computer modeling — for fluid dynamics to enable the prediction of the performance and flight characteristics at speeds beyond ground-experimentation capacities. However, vehicle performance calculated in this way can vary and is greatly dependent upon the hypotheses embodied in the computer codes. Thus, a first task was to verify vehicle design methods, using the correlation between simulation and experimentation[12] (Bogue & Erbland [1993]). As a result, the eventual NASP engine — the experimental vehicle X 30 — would not — yet — have been a prototype or even an “R&D instrument”. Rather, it can accurately be described as a demonstration vehicle or “basic” research instrument enabling the production of infratechnologies and instrumentalities necessary for further research and development. No possibilities existed for incremental research and step by step approaches. The weakness of homotopic correspondences and analogic links both precluded this and, at the same time, revealed the need of lumpy technological — and — research projects. Thus, the production of infratechnologies has to be based on an experimentation-simulation relationship and should result in the design of demonstration vehicles for the production of flight data. It is only on the basis of such data that the conventionally

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[10] Knowledge that plays an infrastructural role in industry needs to persist long enough that it can be recognized and exploited by the organizations not directly involved in its creation (See Steinmueller [1995]).

[11] After determining that existing Air Force, NASA, industry and university engine test facilities were not capable of testing scramjets above speeds of Mach 8 for sustained periods, the NASP program awarded two contracts in October 1986 totaling U.S. $9.6 million for two engine test facilities. These facilities were expected to provide the capability to test full-scale scramjets up to speeds of Mach 8 (US GAO [1988]).

[12] For example, government efforts led by NASA-Ames have provided an understanding of how to safely contain hydrogen, especially during the NASP’s high-temperature flight (Korthals-Altes [1987]).

described “research and development” phases can be undertaken with a minimum of acceptable efficiency in resource allocation.

What is the most appropriate type of organization to generate such infrastructures and infratechnologies, given that the chosen organizational form needs to reflect the “collective” nature of infratechnologies and research infrastructures?

It can be argued that the production of infratechnologies and instrumentation and, hence, of the related structural flight data, requires the establishment of a specific organizational form. Because, the arguments runs, infratechnologies constitute the basic procedures and routines that enable measurements to be collected and compared across projects carried out at different sites, a high degree of standardization is essential. Infratechnologies are more than the sum of the experimental routines developed by the participants and their production cannot be left to the participants alone. Infratechnologies promote collective research in a complex project such as the development of the hypersonic aircraft. Infratechnologies are collective goods in that they require investments that none of the participants individually will feel inclined to pay for. As technological standards, they have no significance outside their collective usage in the research process. Yet the generalized diffusion and adoption of this structural knowledge is essential for the particular program to go forward. This is consistent with the collective mode of knowledge production in a specific context of application.

4.3. Infratechnologies and Organizational Integration : Lessons from the NASP

Indeed, a set of factors would support the formation of a single entity. One is derived from the need to produce a collective technology infrastructure gathering the research instrumentation and the infratechnologies necessary to support the R&D activity in the hypersonic scientific area. The infratechnologies consist in standards of measurement, experimental methods and shared modes of comparing and checking research results that are produced collectively and underlie collective experimentation. The need for these infratechnologies suggests the desirability of forming a single entity to produce the required structural knowledge and facilitate its diffusion throughout the program. However, their “public good” aspects inhibit some, mainly private sector, participants from investing their resources in technologies from which they cannot capture direct benefits. Moreover, if it is known that such infrastructural knowledge (Steinmueller [1995]) has to be widely shared, there will be an incentive for agents to become “free-riders”, and thus avoid the cost of participating in any of the experiments. In other words, there is a risk of information being retained by the competing projects and teams. Besides, the experience cannot be easily shared as it can exhibit some tacit character

This bottleneck can be broken either directly by means of government investments in program infrastructure or indirectly *via* the formation of technological club — such as the consortia of firms and public agencies. Thus, a mixture of public and private investments is used jointly to develop the technological infrastructure. The latter is consistent with the integration and the coordination of dispersed public and private sources of expertise (Kandebo [1990]).

In the case of NASP, the objective of creating such an entity was then also to facilitate the sharing of technical results and to enable the formulation of a single technical design, drawing as much as possible on the research experience of a variety of individual firms (Cohen, Edelman & Noll [1991], p.51). It was also intended to establish NASP’s identity clearly and quickly, making it extremely difficult for the new entity to dissolve into its former, dispersed state. Formally, this was accomplished by producing specific codes and developing specific communication channels to guide flows of information in the nascent organization of NASP. For example, an electronic communications team was created from the beginning with the objective of developing networks within the contracting system composed of subgroups of independent firms[13]. This action brings to mind Arrow’s idea (Arrow [1974]) that codes and information channels are forms of irreversible organizational capital. Indeed, this strategy imposed an irreversible character upon organizational investments. A third organizational feature was the unprecedented level of commitment of public agencies in the research enterprise. For those government agencies, the integration process — known as “mainlining” — involved going beyond traditional generic tasks to include research and experimental instrumentation : « […] mainlining brings the government-run facilities into positions often played by contract research labs or subcontractors » (Kandebo [1990]).

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[13] « The team has already developed an unclassified network to develop scheduling and other plans, and is now working on a classified system to handle electronic transfer of drawings and other data » (Kandebo [1990]).

Conclusion

The aim of the NASP program was to develop and demonstrate hypersonic and transatmospheric single-stage-to-orbit — SSTO —, technologies that will support future national security and commercial applications and provide economies in space launch costs. It consisted in a diverse range of specialists to work in teams on problems in a complex applications-oriented environment. The challenge was to reduce the technological risk — to become locked-in the wrong technological path — without impeding the experimentation of the foreseen technological designs.

I claimed that before any organizational and managerial issues could be addressed, the very nature of the research and technical problems facing the NASP program has to be analyzed in more detail. In this respect, I attempted to specify in what respect the existing scientific base and evidence breaks down, and why it appeared necessary to scientists and technologists to explore the hypersonic area and then to develop infratechnologies. As far as this investigation can be conducted, the discussion suggests what kind of organizational problems to be solved it raises for the achievement of the technological program's goal.

The rationale behind NASP's particular choice of organizational design reflects an attempt to resolve the arising organizational dilemma, trying to balance the two imperatives of diversity and standardization. The result was that it favored the latter. At the time the decision was taken, the supporters of NASP seemed to attach greater importance to the production of a collective research infrastructure than to the broad exploration of the technological and functional dimensions of possible design configurations. Investments in the production of the research infrastructure were critical, despite the great uncertainty attaches to the potential returns from the following individual projects conducted on the basis of the infrastructural knowledge generated. In that case, the option of "clubbing together", i.e. the formation of a single entity grouping all agents — the partners of the program — in a particular sector — in a central laboratory — was proving consistent with the need for the rapid creation of irreversible organizational capital, and with a strong commitment of public agencies in the production of infratechnologies and technological infrastructures. As a consequence, there was a preference for the innovative team approach within a unified organizational form, as opposed to the management of multiple decentralized and "distributed" experimental and exploratory projects. In brief, this is what happened.

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