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Preface

The research project on *Systems Analysis of Technological and Economic Dynamics* at IIASA is concerned with modeling technological and organisational change; the broader economic developments that are associated with technological change, both as cause and effect; the processes by which economic agents – first of all, business firms – acquire and develop the capabilities to generate, imitate and adopt technological and organisational innovations; and the aggregate dynamics – at the levels of single industries and whole economies – engendered by the interactions among agents which are heterogeneous in their innovative abilities, behavioural rules and expectations. The central purpose is to develop stronger theory and better modeling techniques. However, the basic philosophy is that such theoretical and modeling work is most fruitful when attention is paid to the known empirical details of the phenomena the work aims to address: therefore, a considerable effort is put into a better understanding of the 'stylized facts' concerning corporate organisation routines and strategy; industrial evolution and the 'demography' of firms; patterns of macroeconomic growth and trade.

From a modeling perspective, over the last decade considerable progress has been made on various techniques of dynamic modeling. Some of this work has employed ordinary differential and difference equations, and some of it stochastic equations. A number of efforts have taken advantage of the growing power of simulation techniques. Others have employed more traditional mathematics. As a result of this theoretical work, the toolkit for modeling technological and economic dynamics is significantly richer than it was a decade ago.

During the same period, there have been major advances in the empirical understanding. There are now many more detailed technological histories available. Much more is known about the similarities and differences of technical advance in different fields and industries and there is some understanding of the key variables that lie behind those differences. A number of studies have provided rich information about how industry structure co-evolves with technology. In addition to empirical work at the technology or sector level, the last decade has also seen a great deal of empirical research on productivity growth and measured technical advance at the level of whole economies. A considerable body of empirical research now exists on the facts that seem associated with different rates of productivity growth across the range of nations, with the dynamics of convergence and divergence in the levels and rates of growth of income, with the diverse national institutional arrangements in which technological change is embedded.

As a result of this recent empirical work, the questions that successful theory and useful modeling techniques ought to address now are much more clearly defined. The theoretical work has often been undertaken in appreciation of certain stylized facts that needed to be explained. The list of these 'facts' is indeed very long, ranging from the microeconomic evidence concerning for example dynamic increasing returns in learning activities or the persistence of particular sets of problem-solving routines within business firms; the industry-level evidence on entry, exit and size-distributions – approximately log-normal – all the way to the evidence regarding the time-series properties of major economic aggregates. However, the connection between the theoretical work and the empirical phenomena has so far not been very close. The philosophy of this project is that the chances of developing powerful new theory and useful new analytical techniques can be greatly enhanced by performing the work in an environment where scholars who understand the empirical phenomena provide questions and challenges for the theorists and their work.

In particular, the project is meant to pursue an 'evolutionary' interpretation of technological and economic dynamics modeling, first, the processes by which individual agents and organisations learn, search, adapt; second, the economic analogues of 'natural selection' by which interactive environments – often markets – winnow out a population whose members have different attributes and behavioural traits; and, third, the collective emergence of statistical patterns, regularities and higher-level structures as the aggregate outcomes of the two former processes.

Together with a group of researchers located permanently at IIASA, the project coordinates multiple research efforts undertaken in several institutions around the world, organises workshops and provides a venue of scientific discussion among scholars working on evolutionary modeling, computer simulation and non-linear dynamical systems.

The research focuses upon the following three major areas:

1. Learning Processes and Organisational Competence.

2. Technological and Industrial Dynamics

3. Innovation, Competition and Macrodynamics

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This work is partly based on ongoing research with John Baldwin, Analytical Studies, Statistics Canada.

I Introduction

This is a rather conjectural report on the evolutionary role of decision biases - at both the level of individuals and of organizations -, and, in particular, on their importance to the processes of corporate entry and the change of industrial structures.
A growing and quite robust body of evidence highlights the pervasiveness of various types of biases in individual decision making, accounting for systematic departures from predictions of the canonical model of rational choice (see, for example, Kahneman and Tversky (1973), Kahneman and Tversky (1986), Tversky and Shafir (1992)). For our purpose here, we will mainly concern ourselves with overconfidence or optimism, which frequently leads to bold forecasts of the consequences of one's own actions. Also, by way of example, we will examine risk seeking in the domain of losses, which often yields escalating commitments in the face of failures
Interestingly, these biases appear to carry over from the level of individuals to that of groups and organizations - and, indeed, might even be amplified in the latter circumstances (e.g. Kahneman and Lovallo (1993), Lovallo (1995), and the literature discussed there). In this respect, a challenging domain of investigation - with vast ramifications into the analyses of the nature of entrepreneurship, technological change, and industrial dynamics - is that of corporate entry into an industry.

Numerous studies have shown that the vast majority of entrants fail (e.g. Dunne, Roberts, and Samuelson (1988)). Furthermore, there are significant inter-industry differences in failure rates. Evidence of high-level firm failure rates appears to be consistent with experimental data showing that typically people are unrealistically optimistic, exhibit illusions of control in even modestly complex environments, and systematically neglect the statistics of previously observed performances.

In this study, we report some preliminary results and conjectures from an ongoing investigation of entry, post-entry performances, and the collective

outcome of innovative successes and failures.
First, we propose that persistent intra-industry differences in firm performances are the joint outcome of a) heterogeneous patterns of organizational learning and b) cognitive mechanisms such as unrealistic optimism and "competitive blind spots" - areas where agents insufficiently consider the contingent decisions of their opponents.
Second, we suggest some hypotheses on inter-industry differences in relative entry rates and post-entry performances using a taxonomy of technological and market regimes. The basic idea is that knowledge and learning - concerning new products, new techniques, and new markets - are specific to distinct production activities. In turn, "technological paradigms" map expectations and corporate behaviours into diverse patterns of entry behaviours that are at least partly independent of the standard measures of profitability and risk. Of course, were we to find robust corroboration of this conjecture, it would be witness against any naive 'rational expectation' hypothesis on entrepreneurial behaviour.
Our third conjecture takes this argument a step further. We propose that micro 'irrationalities' - in terms of unrealistic optimism etc...- are likely to be a fundamental ingredient in the collective development of new knowledge bases and new industries. The development of new technological paradigms and the related emergence of new industries and new 'technological communities' might be intimately associated with seemingly wasteful mistakes, rough search heuristics, and even 'irrational' hubris, rather than sober forecasts.

Our empirical evidence is diverse. We will draw both on a few experimental studies, on the growing evidence on the economics of innovation and on what we know from some statistical surveys and longitudinal samples of firms in manufacturing industries in a various countries. (A research aimed ot test the foregoing conjectures, in collaboration with John Balwin, Statistics Canada, is currently taking shape).

In sections II to IV we briefly review the relevant evidence from behavioural decision research, identify analogous biases in organizational decision patterns, and present some experimental evidence on entry decisions. Section V discusses the evidence on entry, post-entry performances, exit and the puzzles that all this entails. In section VI we outline some elements of an evolutionary interpretation and suggest some promising links with complementary exercises in evolutionary modeling as well as some possible further developments.

II From individual biases to Organizational Errors

In economics, the use of psychological assumptions other than rationality to make predictions about organizational behaviour is relatively rare, although the company is quite good - including John Maynard Keynes, Herbert Simon, Richard Nelson, Oliver Williamson and Sidney Winter, among others -. Certainly, from an empirical point of view, there is massive evidence that individuals do deviate from the behavioural patterns prescribed by rational models. Furthermore, these deviations are systematic - the errors tend to be in the same direction - , which implies that "non-rational behaviour is often not random but predictable.

However, one of the major hurdles to incorporating alternative psycological assumptions into economic models is a healthy skepticism about how individual decision biases are likely to "scale up" to organizational outcomes.

While it is beyond the scope of this work to examine the vast literature on individual and organizational decision making, there are good reasons to believe that organizations, in many instances, reinforce rather than mitigate individual decision biases (see for example March and Shapira (1987) Kahneman and Lovallo (1993) and Lovallo (1995)).

"Escalation" situations are a very good example of the consistency of psychological phenomena in various contexts and at widely different units of analysis (ranging from individual choices under experimental conditions all the way to enormous collective tragedies such as the Vietnam war). Two basic psychological principles lay at the foundation of "escalation phenomena" , at the level of both individuals and organizations, namely : (i) people respond to changes rather than absolute levels; and, (ii) they exibit diminishing sensitivity to quantities of various items including money.

As known, drawing on these two principles, Kahneman and Tversky (1979)

constructed prospect theory , a descriptive theory of risk taking, in which individuals , due to diminishing sensitivity for absolute quantities, are both risk averse for gains and risk seeking in the domain of losses. Risk seeking preferences for losses implies that when people have not made peace with their losses they are likely to place lower than expected-value bets in order to break even. On average, these bets will fail and lead to even greater losses.
Fox and Staw (1979) show that considering an important aspect of social context - the need for accountability - enhances individual willingness to "throw good money after bad".Using managers as subjects, Bateman and Zeithami (1989b) also observe escalation behaviour. Finally, Bazerman et al (1984) find that groups escalate less frequently but more dramatically than individuals. At each point along the path from individual choice behaviour to individual choice embedded in a social context to group decision making, there is reason to suspect that also economic organizations will escalate commitments to losing courses of action . The consistency of the findings mentioned above and others (e.g. the cases that Janis (1982) and Ross and Staw (1986) recount on the Vietnam war and the Vancouver World Fair) indicate that these suspicions are valid.
Quite similar considerations apply to the widespread phenomena of overconfidence and "framing effects" in the interpretation of the available information.
For example, March and Shapira (1987) suggest that managers tend to interpret uncertainty simply in terms of 'challenges' to their abilities and commitments to the pursuit of their goals. "Groupthink" has been identified as a cause of organizational optimism (Janis (1982)). Moreover, groups are prone to use 'representative heuristics' - the tendency to formulate probabilities on uncertain events based on the similarity of the event itself with some salient property of its parent population (Kahneman and Tversky (1973), Argote, Seabright and Dyer (1986)).

There is extensive literature on overoptimism in project evaluation (e.g. for example Merrow et al. (1981)) and with regards to R&D (a discussion is in Freeman (1982)). Grossly optimistic errors are especially likely if the project involves new technology or otherwise places the firm in an unfamiliar territory. In an interesting discussion of the cause of failure in capital investment projects, as Arnold (1986) finds:

> "Most companies support large capital expenditure programs with a worst case analysis that examines the projects' loss potential. But the worst case forecast is almost always too optimistic... When managers look at the downside they generally describe a mildly pessimistic future rather than the worst possible future".

Standard operating procedures and decision methods, ranging from discounted cash-flows and net present value methods in investment evaluation to cost accounting, often involve framing effects, overconfidence, preference for confirming evidence (for a discussion, especially with regard to technological innovation, see Schoemaker and Marais (1995)).

More generally, the acknowledgment of the specificities of technological and organizational competences embodied in each firm (Teece et al. (1994), Dosi and Marengo (1993)) entails also the recognition of specific heuristics, problem-framing, and ultimately of diverse collective structures of cognition defining what the organization can do, how it does it, and where and how it can search for novel technologies and products. Clearly, competence specificity, other things being equal, will tend to strengthen an inside view in forecasts and decisions. That view - as detailed in Kahneman and Tversky (1979) and Kahneman and Lovallo (1993) - draws on knowledge on the case at hand, and constructs an ideal history of the future conditional on the sequences of actions by the decision makers. (In contrast, an 'outside view' is statistical and comparative drawing from past experiences of analogous cases).

In brief, organizational decision-making in general, and, *a fortiori*, relatively unique 'strategic' activities concerning innovation, diversification and entry - grounded in firm-specific knowledge - is often likely to involve biased assessments of one's own technological and competitive abilities (stemming from overconfidence, 'inside view', illusion of control), and inertial and escalating commitments (with neglect of potentially relevant information and 'sunk cost fallacies'). We suggest that entrepreneurial decisions of entry are no exception.

In particular, it is worth reporting some experiments by one of us (Dan

Lovallo) indicating how 'inside view' thinking is likely to lead to excess entry. We refer to the prediction that there will be excess entry as the optimism hypothesis. The experiments reported below serve three purposes.

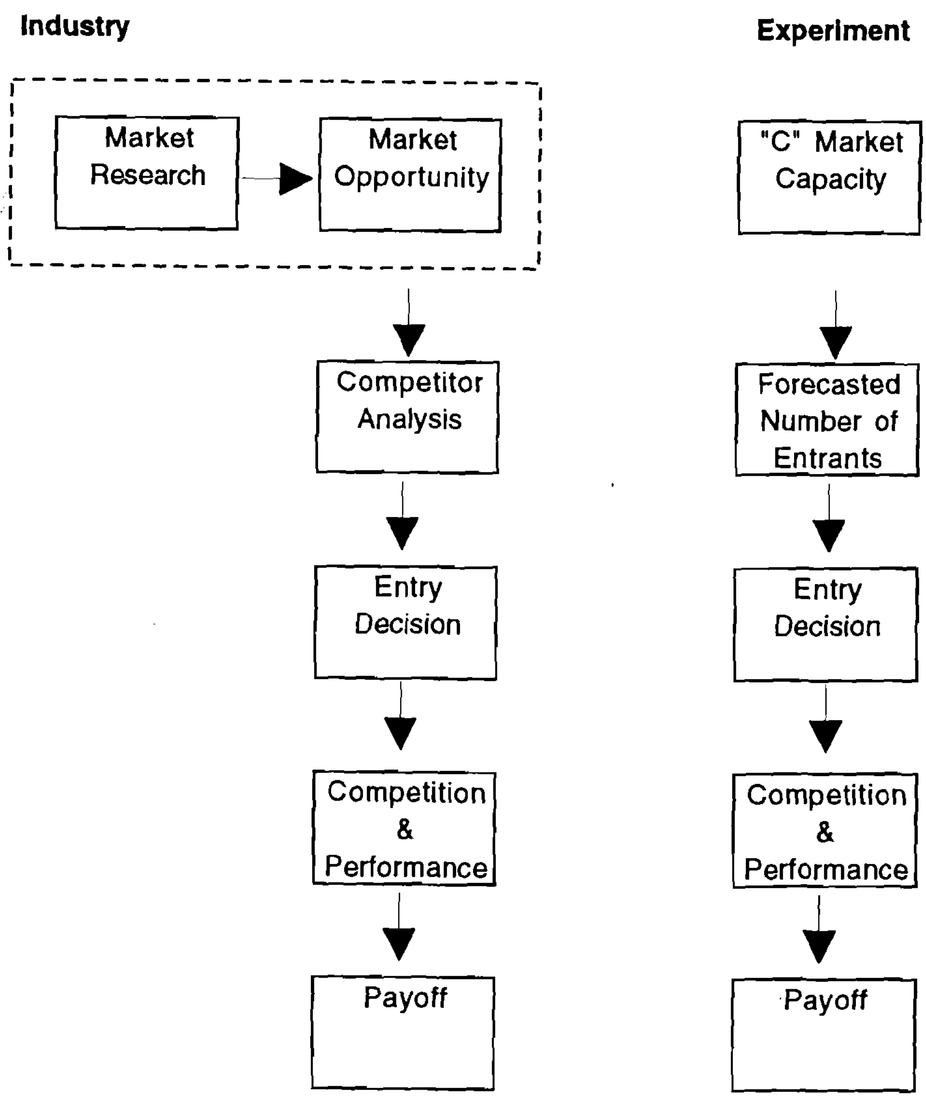
First, one would like to test whether the relative optimism that we see in non-competitive environments survives in the face of competitive interaction. Second, the controlled environment allows us to unpack the effect that various types of inside view thinking have on entry. Third, these experiments may provide clearer insights about the psychology of competition, which would lead to more informative field surveys of entrants.

III Experimental Design

The isomorphism between the experiment and the industrial activity that we model is illustrated in figure 1.

Fig. 1

Experimental Model



It is reasonable to assume that one of the first steps towards entry is for a firm to undertake some kind of market assessment in order to determine if there is sufficient opportunity in terms of probability and the size of a market to warrant entry. In the experiments, subjects are provided with information about the market capacity - the number of entrants that can earn positive amounts of money in any given period. The next step in the entry process is competitor analysis. In an industrial setting this procedure involves multiple dimensions including estimating the likely number and quality of potential entrants. In these experiments we explicitly ask subjects to estimate the number of entrants they expect to enter in each period. Implicitly, they make their entry decision, which is the next step in the process. Finally, in both environments there is competition and diverse performances which result in differential payoffs based on relative skills.

Given this broad overview of the experimental model let us be more specific about particular experiments where we manipulate several factors in the competitive environment. One of the most important manipulations is whether subjects self-selected themselves into a particular experiment or not. In some experiments subjects are recruited to participate in entry games and no particular information is given about the dimension on which the subjects will compete. In other experiments we explicitly ask for subjects that consider themselves to be above the median in terms of their knowledge of sports or current events. The subjects share common knowledge about the method in which they were recruited. In addition to the self-selection manipulation, the entry games occur in three different competitive environments: simultaneous entry without feedback, simultaneous entry with feedback, and sequential entry. In the simultaneous entry conditions, all of the subjects make their entry decisions at the same time. The feedback that the subjects receive is about the number of entrants that entered in the previous period. In the sequential entry condition, each subject is given an entry order number that remains constant throughout the experiment. For example, the subject with entry order number one makes his entry decision first, subject two goes second, etc. All of the entry decisions are public knowledge.

In all of the experiments we use an identical payoff table, which is presented in figure 2.

Fig. 2

Experimental Payoff Table

<u>**Payoff for Successful Entrants as a function of "C"**</u>

(market capacity)

	"C"			
Rank	**2**	**4**	**6**	**8**
1	33	20	14	11
2	17	15	12	10
3		10	10	8
4		5	7	7
5			5	6
6			2	4
7				3
8				2

The amounts listed are the payoffs for the successful entrants for each market capacity. Unsuccessful entrants always lose $10. Consider the following example. If market capacity is two, then the highest ranked entrant receives $33, the second highest ranked entrant receives $17, and anyone else that enters the market loses $10. In any case the maximum total possible profits in the market are $50. This means that if five more entrants in excess of market capacity will come in the total profit in the market will be $0. For example, if there are seven entrants when the market capacity is two the total profit for the entrants as a group will be $0, since the two top-ranked entrants will split $50 according to the payoff table and the third- trough seventh- ranked entrants will each lose $10. If there were 8 entrants when the market capacity was 2 total profits would be $ -10.

In each experiment there are two different ranking procedures: random rank and skill based rank. In the random rank procedure, subjects' ranks are predetermined by a random number generator. Subjects do not know their ranks prior to making their entry decisions. After all the entry decisions have been made, a tournament starts, to be played for real money. It is only at this point that subjects learn of their randomly assigned ranks. In the skill based rank condition subjects are shown examples of the types of questions on which their ranks will be based. However, they do not answer the questions until all of the entry decisions have been made. Then, subjects are given a quiz and their ranks are based on the number of questions they answer correctly. The purpose of the random ranked condition is to control for risk preferences. In games with asymmetric payoff functions such as the one described here there is no way *ex ante* to determine the equilibrium number of entrants without knowing subjects' risk preferences. Since the subjects do not change across the different version of the experiment, if we assume that their risk preferences do not change from one condition to the next, the only reason for greater entry in the skill-ranked condition is that subjects have more sanguine views of their probability of success than in the random ranked condition. It is the difference in the number of entrants in the two conditions that will be the primary measure of interest throughout the experiments. Figure 3 contains an example of the actual form subjects use to record their responses.

Fig. 3

Market Experiment A - Random Rank

NAME __________ DATE _________

Payoff for Successful Entrants as a function of "C"

	"C"			
Rank	**2**	**4**	**6**	**8**
1	33	20	14	11
2	17	15	12	10
3		10	10	8
4		5	7	7
5			5	6
6			2	4
7				3
8				2

How much would you earn if C=6, you entered, and your rank was 5 among the entrants? ____

How much would you earn if C=2, you entered, and your rank was 4 among the entrants? ____

Round	C	Expected # of Entrants	Enter	Not Enter	# of Entrants
1					
2					
3					
4					
5					
6					
7					
8					
9					
10					
11					
12					

If, for example their are 12 periods in each condition, balls numbered 1-24 will be placed in a bingo cage at the end of the experiment : the period corresponding to the chosen ball will be played for real money. The experimental procedure is summarized in Table 1.

Tab. 1 Experimental Procedure

1) **Read instruction aloud**
2) **Comprehension test on the payoff**
3) **Explanation of the two types of ranking**
4) **Subjects are shown examples of the skill question**
5) **Subjects are informed that one period will be played for real money**
6) **Subjects make their forecasts and entry decisions in the random rank condition**
7) **Subjects make their forecasts and entry decisions in the skill rank condition**
8) **After all of the entry decisions are made, subjects take the quiz**
9) **A randomly drawn period is chosen to be played**
10) **Subjects' earnings are computed and immediately paid**

IV Summary of the Results

Without going into too much detail, which can be found in Lovallo (1995a), this section summarizes the results from the experiments. There are four findings that are of interest. First, it is clear that there is excess entry in the skill condition as compared to the risk-controlling random rank condition.

This was true in each and every experiment. Furthermore, the expected value of entering in the random rank condition was significantly positive in all of the experiments, while it was significantly negative in all of the skill ranked conditions. Second, the excess entry that we observe in these experiments is not caused by "blind spots", i.e. on average subjects' forecasts of the number of entrants are accurate. This means that subjects in the skill condition are saying, "I realize that on average people are going to lose money in this market, but I'm not - I'm in!"

The next finding is the most surprising. In experiments without self-selection, we find a significant divergence between the number of entrants in the skill versus random rank conditions. However, this divergence is dwarfed by the magnitude of the divergence in markets with self-selection. This suggests that a large amount of excess entry is caused by reference group neglect (Lovallo 1995), rather than some variant of optimism. Reference group neglect refers to the tendency of people to underappreciate the group with which they are competing. It is a competitive manifestation of inside view thinking. For example, suppose that you are a phenomenally good cook and you are thinking of opening a restaurant. If you are asked to evaluate yourself as a cook in comparison to the general population, you might say that you are in the top 5% - and you might be right. However, a more pertinent question is how good of a cook are you in comparison to others in the restaurant business, almost all of which consider themselves, and probably are, in the top of 5% of cooks. Reference group neglect implies that you will insufficiently regress your prediction of your relative ability in this more competitive group.

Finally, it is useful to point out that the effect that we are discussing is robust across many different types of competitive environments. The effect is significant with feedback and without self-selection and is even more robust without feedback and with self-selection. Furthermore, the effect works in both simultaneous and sequential entry games. Indeed, there is no significant difference between the effect in these two environments, which is rather astonishing. This means that in a sequential entry environment, people are making a decision to enter knowing with probability one that the value of the game to the player as a group is already negative!

Our general conjecture is that this experimental evidence on cognitive and decision biased bears important implications also for the understanding of actual entry processes of new firms in industry. In order to argue the point, let us begin by considering some available evidence on corporate entry and entrants' performances.

V Patterns of entry, post entry performance and exit in manufacturing

Paul Geroski (1991) identifies four major 'stylized facts' on the entry process. First, "many firms attempt to enter each year, but [...] few survive for more than a year or two. The average entrant is, it seems, basically a tourist and not an immigrant, enjoying a life that is often nasty, brutish, and, above all, short" (p. 283). Second, "different measures of entry (net and gross entry measures, entry based on sales or on number of firms) are not very highly related to each other" (p.287). Third, "there are a range of different types of entrants, and some are more successful at penetrating markets or survive than others" (p.290). Fourth, "the effects of entry [on market performance] - like the lives of most entrants - are fairly modest" (p.293).

Dunne et al. (1988) examines the patterns of firm entry, growth, and exit of different types of firms over the period 1963-1982 using plant-level data from the U.S. Census of Manufacturers. For the 1967 entrant cohort, 63.8% of all new firm, new-plant entrants exited within five years, while 49.6% of all diversifying-firm, new-plant entrants exited. The difference in failure rates is similar as the cohorts age. Within fifteen years, 87.9% of the new firm-new plant exited, while 74.6% of the diversifying-firm, new-plant entrants exited. The differences between *de novo* and diversifier exit rates are substantial for all the cohorts in the sample at any of the time periods measured.

The high mortality of entrants is corroborated by longitudinal studies on industrial life cycles (see Hannan and Freeman (1989), Carroll and Swaminathan (1992), Klepper (1992)), and so are the differences in post-entry performances according to different types of entrants and the timing of entry.

Consider, for example, Lane's investigation of ATM manufacturers (Lane (1989)). The ATM market began shortly before 1969. The first firm to enter the market was Money Machine Inc. in 1967. An interesting pattern of entry develops over the life-cycle of this industry. The earliest entrants into the industry were almost all *de novo* entrants; the next group of entrants were the diversifying firms; the final wave of entry came from foreign firms. The average entry date for the three groups of firms were 1970, 1975, and 1979, respectively. The reasonably distinct partition between the entry dates for the three entrant types suggests that there is a systematic difference between the firm types that drive entry behaviour. *De novo* firms, obviously, are start-up firms without any prior production experience. All of the diversifying firms that entered this industry had prior production experience in a related domestic industry. Specifically, diversifying firms had production experience in either cash-handling products, security products (safe and vaults), or computers. The foreign firms all had prior experience producing ATMs abroad prior to entering the U.S. market, although the degree to which they were selling other products in the U.S. market varies. (The question of whether prior U.S. market experience is a significant contributor to success is an interesting one that is not addressed in the Lane study.)
Docutel, a *de novo* entrant, was the dominant firm in the early years of the industry. However, as time went on, the *de novo* entrants lost share to the diversifying entrants. In the middle to late 1970s, Diebold, an early diversifier, became the dominant firm and held that position until the end of the sample period, 1986. The rise of Diebold coincided with the period when the overall size of the ATM market grew most rapidly. Eventually, in October 1986 when Docudel exited, all of the de novo entrants were a memory. Furthermore, the average life-span for all of the *de novo* and diversifying firms that entered after the median entry date for their respective groups, except for the lone surviving firm Concord, was less than two and a half years. Whether these firms made or lost money cannot be determined from the available data. However, given that sunk costs play a significant role in this industry, it does not seem likely that such a brief visit would be profitable.

The big winners in this market in terms of market shares were Diebold, NCR and IBM - all early diversifying entrants. The experience that these firms had in safes and computers appeared to provide production advantages that increased market share and the likelihood of survival. Firm-wide production experience unrelated to the ATM industry did not confer an advantage either in terms of market share or survival. Furthermore, previous safe and computer experience with banks has greater advantages for survival and market share than non-bank related safe and computer experience.

A somewhat similar story emerges from Mitchell's account of the medical diagnostic imaging industry (Mitchell (1989), (1991), (1993)): one observes waves of new entrants (both de novo and diversifying entrants) linked with the introduction of new major technologies (nuclear imaging scanners, ultrasound equipment etc.), mortality rates especially high among new comers, and incumbents regaining relatively quickly their dominant market shares. In fact, newcomer market share fell to 10% or less by 1988 in all of the subfields except for ultrasound. Even in ultrasound where newcomers are a majority, their market share was less than 50% in 1988. In the subfields there have been waves of fluctuations in newcomer market share associated with newcomer product innovations. However, in all but the most recent upsurge of newcomer share in the ultrasound segment incumbents recovered their market position.

Incumbents were also much more likely to survive (84%) in comparison to diversifying (44%) and *de novo* firms (29%) as of 1988. The method of exit also differs systematically between the firm types. 70% of the *de novo* firms that exit do so by closing down, whereas 70% of the diversifying or incumbent firms sell their business when they exit. Even the early newcomers to industries, one of the first three newcomers in each subfield, performed relatively poorly. Only 2 of 15 early newcomers still existed in 1990 and neither of these firms was in the top three in market share. This performance stands in sharp contrast to the early incumbent entrants - 10 of 15 survived until 1990 and 5 of the 10 survivors were market share leader in 1988. In the medical diagnosis imaging industry it seems that de novo and diversifying firms' innovations contribute more to the evolution of the industry than to these firms' own success.

Other industries, however, suggest quite different patterns. In semiconductors (Dosi (1984) and Malerba (1985)), some *de novo* entrants have indeed become the industry leaders while most diversifiers from seemingly related industries have failed. Likewise, in the computer communication industry, the main actors have been new firms (Pelkey (1993)). Somewhat similarly, in the photolithographic alignment equipment, incumbents have fared rather poorly, and each reconfiguration of product technologies has been associated with the emergence of new industry leaders (Henderson (1988) and (1993), Henderson and Clark (1990).

At broader levels of description - often 2- to 4- digit industries - some intersectoral regularities in the process of entry, growth, and mortality appear to emerge. So, for example, entry, while being a very pervasive phenomenon, appears to be positively correlated with the number of incumbents, the growth of shipments in the industry and its variability; whereas there seem to be little correlation with industry profitability. Entry in concentrated industries seems to be lower in terms of number of firms but entrants tend to be bigger and have a higher life expectancy. The probability of survival of new small firms appear to be lower in capital-intensive and innovation-intensive industries. Hazard rates do not appear to be affected by scale economies in low-tech industries but they are in high-tech ones. The instantaneous effect of entry on output in terms of shares is generally low, but the medium-term one (of those surviving) is quite significant[1].

Moreover, hazard rates and post-entry performances seems to be significantly influenced by the nature of the entrant (whether *de novo* start-up or diversifying from other sectors).

Finally, other more detailed traits of the entrants (such as educational

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[1] On all these properties, see Dunne, Roberts and Samuelson (1988), Baldwin and Gorecki (1990) and (1991), Cable and Schwalbach (1991), Bianco and Sestito (1992), Aldrich and Auster (1986), Acs and Audretsch (1990) and (1991), Phillips and Kirchoff (1989), Audretsch and Mahmood (1991), Mahmood (1992), Geroski and Schwalbach (1991), Baldwin (1994), Baldwin and Rafiquzzaman (1994).

attributes of the founders and the organizational strategies) seems to influence survival probabilities (Brüderl, Preisendörfer and Ziegler (1992)).

What do we make of all this evidence on entry, performances, and mortality? How do we relate it with the cognitive and decision biases discussed in the previous section ? And what is the importance of these biases for technical change and industrial dynamics?

VI An evolutionary view of knowledge and biases in economic change

There are three major building blocks in our argument, namely: First, cognitive biases are widespread attributes of adaptation and discovery in complex and evolving environments. Second, the nature of such biases - or, more generally of decision rules - can be inferred to a large extent from the characteristics of the knowledge upon which agents draw. This applies also to entry decisions. Third, at least with regards to entry, biases might often have a positive collective effect, in that they might be necessary to trigger exploratory behaviours and contribute to the development of commonly shared 'technological paradigms' and ultimately foster the establishment and diffusion of new knowledge and new organizational forms.

Learning, competence traps and biases

One of the remarkable features of most of the evidence discussed in sections II to IV is that biases are prone to emerge also in circumstances where the decision problem is sufficiently transparent to allow the unequivocal identification of 'rational' decision procedures. A fortiori, one can expect them to emerge in more opaque and changing environments. Of course, an interpretation of such phenomena could be simply in terms of human fallibility, due for example to some underlying computational limitation, attention economizing, and inertial reinforcement of past behavioural responses. Far from denying that all these factors are at work, the line of inquiry that we want to pursue here is that, more fundamentally, these biases might be an unavoidable corollary of the ways agents form their interpretative models of the world and their behavioural routines in evolutionary environments.

It seems to us that a growing number of contributions from different camps - evolutionary economics, organization theory, cognitive psychology, artificial sciences - are starting to converge in their analyses of learning processes in all circumstances when the environment continuously changes or in any case is sufficiently complex to entail some competence gap between the skills notionally required for decision and those 'naturally' available to the agents (Heiner (1983) and (1988)). It is clearly a perspective which goes back to the research programme of Simon, Cyert , March, Nelson and Winter on the nature and implications of 'bounded rationality' and has been recently enriched by experimental evidence and computer-simulated models.

To make a long story very short, this perspective implies a radical shift in the object of analysis: rather than focusing on the signals that the environment delivers to the unit of decision, it emphasizes the inner features of the response mechanism of the unit itself and on the ways internal representations of the world are constructed.[2]

There are some quite general implications that come out of this perspective.

First, facing an essential ambiguity in the relationships between events actions and outcomes[3], agents are bound to search for appropriate categories which frame cognition and actions.

Second, action rules often take the form of relatively event-invariant routines which are nonetheless 'robust', in the sense that they apply to entire classes of seemingly analogous problems.

Third, adaptive learning , involving interrelated units of knowledge (i.e. some sort of cognitive systems), tend to lead to lock-in phenomena.

For example, Dosi and Egidi (1991) discuss this learning dynamics in the simple case of the Rubik cube and Dosi et al. (1994) show in a simulated model of adapting learning the emergence of economic rules such as marking-up prices. Levinthal (1993) studies organizational adaptation on a 'rugged

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2 Holland (1975), Holland et al (1986), Dosi and Egidi (1991), Schrader, Riggs and Smith (1993), March (1988), Dosi and Marengo (1993), Marengo (1992), Levinthal (1994), among others

3 On the notion of ambiguity as distinct from uncertainty, see Einhorn and Hogarth (1985), March (1988), Marengo (1992), Schrader, Riggs and Smith (1993)

landscape' (i.e. to a selection environment characterized by interdependent and non-linear contributions of various organizational attributes to the 'fitness' of the organization): he shows the adaptive emergence of few archetypes of organizations and behavioural patterns which - depending on the interdependence among traits - tend to lock organizational evolution even when the external environment changes in ways that are unfavourable to the existing set-ups. Marengo (1992) presents a model of co-evolution between organizational representations of the environment and its behavioural responses in a changing environment.

For our purposes here, what is important to notice is that by switching the analytical emphasis from agents as 'information-processors' to agents as 'imperfect explorers' and as 'problem-solvers', it is easy to appreciate the widespread emergence of cognitive frames and decision routines. They are in a sense the inevitable outcome of imperfect adaptation to ever-changing and potentially surprising environments, even if they appear as 'biases' whenever the environment is simple enough as to notionally allow more refined and orthodox rational decision procedures.
All this applies, we suggest, to individuals and even more so, to organizations. But seeing organizations as problem-solvers naturally leads to acknowledgement of the role of their internal knowledge, competences and 'visions' as prime determinants of their behaviours. As Levinthal puts it,

> "the ability of firms to evaluate and utilize outside knowledge is a function of their level of prior related knowledge. [The latter] confers an ability to recognize the value of new information, assimilate it, and apply it to commercial ends which ... collectively constitute a firm's "absorptive capacity" (Levinthal in this volume)

Moreover, as emphazized in Cohen and Levinthal (1989) and (1990), such absorptive capacity is path-dependent, given its cumulative nature and its co-evolution with expectation formation (see also Dosi (1988)). From an evolutionary point of view, the development of specific problem-solving competence is a necessary condition for survival but such competences are inevitably 'local', reinforced by past history but not necessarily relevant

today[4] .
Indeed, our general conjecture is that it is precisely these features of knowledge that tend to produce many of the biases discussed above.
For example, cumulative and idiosynchratic knowledge may easily imply an 'inside view' of future outcomes. Previously successful problem-solving routines can be expected to lead to overconfidence on their future applicability. And the Schumpeterian perception of the permanent existence of unexploited opportunities of innovation are likely to result in 'de-strategizing' of behaviours - i.e. actions whose outcomes depends also an interacting firms are seen on the contrary as part of a 'game against nature' (Dosi and Marengo (1993)): putting it more vividly, as once a senior officer of Intel was telling one of us when asked about their strategies, "...strategies might be a concern for our competitors; we are just better than the others and our only goal is to remain that way...".
To summarize: what we suggest here is that decision biases are to large extent the downside of competence-building and Schumpeterian processes of discovery and implementation of cognitive frames and routines apt to make sense and control imperfectly understood environments.

Knowledge bases, entry and post-entry performances

A 'knowledge-centered' view of organizational behaviours makes a nice contrast with 'information-centered' or "incentive-centered' ones also with respect to entry decisions. Drastically simplifying, an 'incentive story' on the entry process would start by the identification of proxies for expected profitabilities; make some assumptions on the information to which would-be entrants have access (rational expectations being the most extreme one) and then derive predictions on entry dynamics microfounded on rational and unbiased decision processes.[5] A similar modeling strategy can obviously be

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[4] On the notion of organizational competence and their characteristics, see Dosi,Teece and Winter (1992), Teece and al. (1994), Dosi and Marengo (1993), Teece,Pisano and Schuen (1992)

[5] More sophisticated variants of this same story would allow also for incomplete information on one's own ability relative to the other competititors, as in Jovanovic (1982)

applied to e.g. the propensity to innovate of incumbents vs. entrants (cfr. Arrow (1962), Reinganum (1983) and the critical discussion in Henderson (1993)). The major point, in any case, is that some hypothesis on an unbiased rationality and a fine perception of the 'objective' incentive structure allows the theorist to work so to speak 'backward' from future outcomes to past entry decisions.

Conversely, the 'knowedge-centered' (or 'evolutionary') story only needs to assume, on the incentive side, what elsewhere we have called weak incentive compatibility (Dosi and Marengo (1993)), that is, put very roughly, the perception, - no matter how biased, self-condescending, etc. - that '...there are some unexploited opportunities out there and if I' m good I can derive some economic benefit from them...".[6] Rather, the core of the story relates expected behaviours to some specific characteristics of the knowledge bases on which agents are likely to draw and to some internal characteristics of the agents themselves - including , of course, their problem-solving competences - . In this perspective, the predictions of the theory rests on exercises of "mapping" between a) modal learning processes approximately shared by the entire relevant population of agents, or some subsets of them; b) the institutional arrangements under which agents interact; and, c) their revealed performances.

A good deal of work has already been done along these lines , at both empirical and theoretical levels.

In terms of empirical investigation, and related "appreciative theorizing" - as Nelson and Winter would call it - one finds, for example, Pavitt's taxonomy on the sectoral patterns of generation and use of innovation (Pavitt (1984)). The basic exercise there is to identify the fundamental sources and procedures of innovative activities specific of each sector (e.g. does innovative knowledge draw heavily on scientific advances? Or is it much more informal and for example relies on tacit design skills? Is innovation mainly related to the introduction of new products or to the adoption and efficient use of inputs produced by someone else?; etc.). Next, it derives propositions on the

[6] Of course there are cases, whereby not even such weak incentive requirements are fulfilled: think for example of many features of the past Soviet innovation system, or think of circumstances with zero appropriability of innovation, such as for long time seed-related agricultural innovations (more on appropriability issues in the survey in Dosi (1988)).

characteristics of the innovating firms (whether they will be typically big or small; single product firms or diversified ones; etc.)[7] . Another exercise in a similar spirit is that by Dosi, Teece, Winter (1992) and Teece et al. (1994) who derive predictions on the boundaries of the firm - conditional on their principal activities - from the nature of the competences which their principal activities imply.

From a dynamic point of view, several studies have analyzed the typical patterns of evolution of industries following the emergence and establishment of a "technological paradigms" (e.g. Dosi (1984)) or "dominant designs" (e.g. Utterback and Suarez (1993)), often identifying some invariant features along a "technological life cycle" (Gort and Klepper (1982), Klepper (1992)).

Moreover, continuities or breaks in the process of knowledge accumulation - yielding "competence-enhancing" or "competence-destroying" technical progress - have been found to be robust predictors of the relative performance of incumbents vs. new entrants (Henderson and Clark (1990), Henderson (1993)).

At the level of more formal theory, diverse regimes of learning and market selection have been used to explain different patterns of evolution of industrial structures, including changes in industrial concentration, size distributions, turbulence in market shares, growth and death probabilities conditional on size and age (Winter (1984), Dosi and Salvatore (1992), Dosi et al.(1994)). Basically, the exercise involves some stylized representation of the learning regime - formally captured by a particular stochastic process driving the access to new firm-specific technologies - ; the analysis of the collective outcomes of competitive interactions; and their comparison under different regimes.[8]

Our general conjecture - which unfortunately we are still unable to substantiate in this preliminary report - is that the characteristics of learning regimes are also a major predictor of (i) the rates of entry into an industry; (ii) the relative frequencies of different types of entrant (e.g. new start-ups vs. diversifiers); and , (iii) post-entry performances .

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[7] For further evidence on this point, see Malerba and Orsenigo (1995), (1995a) and (1995b)

[8] A discussion of diverse corporate behaviours under different technological regimes in evolutionary models of industrial change is in Malerba and Orsenigo (1995) and (1995a).

Amongst the discriminating features of each regime the evolutionary literature has identified 1) the richness of innovative opportunities; 2) the degrees of codifiability of knowledge (vs. its 'tacitness'); 3) its serendipity vs. specificity to a particular activity; 4) the levels of 'cumulativeness' of technological and organizational learning. Well, we predict these factors to be discriminating also in terms of patterns of entry and performances. So, for example, one may derive propositions like the following:

a) other things being equal, the higher the perceived technological opportunities, the higher will be entry rates, irrespectively of post-entry performances;

b) knowledge serendipity positively affects entry rates but not necessarily survival probabilities;

c) the rates of failures of *de novo* entrants are a positive function of the cumulativeness of technological learning.

(And indeed, there is a much longer list of empirically testable propositions that can be derived with respect to corporate entry and mortality from evolutionary theories of learning and market selection).

The way these theories link up with the evidence discussed earlier on decision biases is that they fully acknowledge them and in a sense try to predict their importance and impact on the grounds of some generalizations regarding the patterns of knowledge accumulation, the sources of competitive advantage and the modes of market interaction. So, for example, evolutionary ('knowledge-centered') theories of industrial dynamics are perfectly at ease with the finding that entrants - and, most likely, also incumbents - tend to take an 'inside view' in their strategic choices ; having recognized it, they will try to predict under what circumstances the outcomes will turn out to be, with a reasonable probability, brave self-fulfilling prophecies, or, conversely, miserable delusions.

Not only that: decisions that turn out to be biased from the point of view of individual forecasting rationality might have, collectively, a positive evolutionary values.

Heroes and martyrs in the dynamics of collective exploration

Entry dynamics are most often analyzed in terms of their effects on competition - which are generally rather modest - ; of the waste of resources associated with the frequent failures - which appear to be significant - ; or of long-term impact of successful entrants on industrial efficiency - again, quite important - (on the first two points, cfr. Geroski (1991) and, on the latter, Baldwin (1994)).

Here, however, we want to look at entry from a complementary point of view, namely the collective effect of both successes and failures upon industrial learning.

A suggestive way to put the question is, following March (1991), in terms of the fundamental dilemma in evolutionary environments between "exploitation" and "exploration". Briefly, "exploitation" concerns adaptation to a given environment and efficiency. Improvements on a grounds of a given set of perceived opportunities. Conversely, "exploration" regard the discovery of novelties -e.g. in the domains of products, processes or organizational forms -.[9] It is straightforward that in a 'knowledge-centered', evolutionary view such dilemma might easily emerge. First, the knowledge bases required for "exploitation" might be quite different from those most conducive to "exploration". Second, we have mentioned earlier that learning generally entails path-dependency and lock-in phenomena into particular regions of a high-dimensional, and quite ill-defined, search space.

For both reasons, the search for novelty - and in particular, those forms of novelty which are not contemplated by the competences embodied into incumbent organizations- requires 'deviant' behaviours often associated with new start-ups.[10] As argued at greater length in Dosi (1990), the distribution of

[9] The trade-offs and dilemmas between 'exploration' and 'exploitation' carry over also to a more aggregate level, in terms of average or modal behaviours of the population of firms embedded into particular national institutions, collective competences, persived opportunities and constraints. For discussions at this broader level of notions like 'dynamic' or 'Schumpeterian' efficiency as opposed to 'static' or 'allocative' efficiency, cf. B. Klein (1977) and Dosi (1988a).

[10] It is a matter of debate to what degrees incumbents are able to internalise search for radical novelties and endogenize, in a biological metaphor, the generation of 'mutations'. It has been suggested for example that the institutional organization of markets influences such an ability. In particular it is claimed that 'market based' financial systems such as those of most anglo-saxon countries induce strong pressures to short-termism and "exploitation" ,thus relying much more on new firms for exploratory activities. Conversely, 'bank-based' systems - such as Japan or Germany - might confer incumbents a much greater room for time-consuming and uncertain attempts to search for

mutations might be heavily biased in favour of mistakes: hence search efforts are likely to turn out to be, on average, disappointing economic failures for the individual actors who undertake them. Nonetheless, collectively, they might be a crucial ingredient of change. In this sense, the biases reviewed in sections II to IV -especially overconfidence, inside-view and illusion of control - are essential to sustain exploration even when the latter is not individually rewarding.[11]

There is another, related, way in which individual mistakes are an essential part of collective learning: this occur whenever also 'mistakes' do contribute to increase collective knowledge. In that case they represent a sort of externality for the whole system.

These propositions are finding increasing corroboration in the evolutionary literature -in both domains of natural and social systems.

The general requirement of variety-generation is indeed a quite established proposition (in economics, see Metcalfe (1991) and Saviotti (1992)).[12] And it is also well established that, apart from the most restrictive cases, it is hard to identify - for the theorist and a fortiori for the empirical agents - any equilibrium distribution of 'exploratory' vs. 'exploitative' behaviours. More technically, only under highly demanding assumptions on the nature of the environment, it is theoretically fruitful to interpret such dynamics in terms of (mixed) evolutionary stable strategies (ESS). It is so for different reasons.

First, innovation, almost by definition, involves uniqueness and surprise. As a consequence it is misleading to assume that whatever strategic pattern learned in the past will necessarily be the equilibrium one also for the future.

Second, successful exploration inevitably adds to the menu of available strategies and thus deforms the shape of the 'fitness landscape' in ways that

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new trajectories of learning. For discussions, cf. Zysman (1994), Dosi (1990), Aoki and Dosi (1991).

11 Note that this argument is quite distinct from the hypothesis that 'explorers' are rational and risk-lover. Our point is that iirrespectively of whether they are risk-lovers, they certainly have also to be biased in their decision making in order to do what they do. Or, putting it in another way, given their risk preference, if they were endowed with 'rational expectations' about the future they would do otherwis

12 See also Allen (1988) and Allen and McGlade (1988) for a suggestive model on the dynamics of fishery driven by the interaction between "cartesian" fishermen (i.e. "exploiters") and "stochasts" (i.e. "explorers");

may well be unpredictable to individual agents.[13]

An illustration of the collective role of 'Schumpeterian sacrificial lambs' is presented in Silverberg, Dosi and Orsenigo (1988). There, we study the diffusion of a new technology under the assumption that learning-by-using is partly appropriated by individual adopters and partly leaks out as an externality. Well, under some parametrizations of the learning process, we show that unequivocally superior innovations might diffuse only if there are overoptimistic entrepreneurs who pay that price of the initial exploration: their failure opens the way to the take off of the industry. Somewhat similarly, one of the properties of the model in Chiaromonte and Dosi (1992) is that a necessary condition for sustained aggregate growth is some degree of diversity of microeconomic behaviours (related to e.g. to the propensity to innovate and imitate).

This theoretical argument easily relates also with the empirical evidence on the multiple contributions of a growing number of actors (quite a few firms, but also public agencies, universities etc.) to the rise of new technologies and new industries. At one level, the process can be described in some technology-space in terms of emergence and establishments of 'technological paradigms', 'dominant designs', etc. However, at a more behavioural level, the dynamics is driven by a network of diverse agents who, via their trials and errors, increasingly develop a commonly shared knowledge basis, recognizable modes of interactions, collective institutions, etc.[14]

The construction of a socially distributed knowledge base inevitably rests also upon a multitude of failed entrepreneurial efforts, in addition to a few impressive jackpots hit by the most ingenuous or the luckiest ones.[15]

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13 Interrelatedness of the contribution to "fitness" by different traits, co-evolutionary effects and non-linearities are clearly sufficient to induce unpredictability. (See Levinthal (1993) and (1994), and Dosi and Metcalfe (1991).

14 For analyses from different angles see Rip (1992), Rip, Misa and Schot (1994), Metcalfe and Boden (1991), Garud and Rappa (1994), Garud and Van De Ven (1989), Callon (1993), Nelson (1994), Appod, Harrison and Kelley (1993), Miller and Blais (1992). In general, the view presented here is highly complementary with the idea of coevolution between cognitive traits, artifacts and routines outlined in Garud and Rappa (1994) and Garud and Ahlstrom (1995).

15 This statement is in principle consistent with formal investigation of 'distributed learning models' (cf. for example Huberman and Hogg (1988), Huberman and Glance (1992)) as well as with the experimental evidence on cooperative learning in new problem-solving activities (some suggestive results are in Egidi (1993).

In all that, we suggest, the stubborn pursuits of unlikely courses of search, together with the other biases that one has discussed, might well be a wasteful, imperfect, but crucial ingredient.

VII Some conclusions

We have emphazized from the start the preliminary nature of this work. Still, if our interpretation is correct, it promises to provide closer and more coherent links among four domains of empirical investigation which so far have proceeded along quite separate paths, namely:

(i) the nature of cognitive and decision-biases of individuals and organizations;

(ii) the regularities and patterns in the processes of innovation and diffusion (associated with the emergence of "technological paradigms" and "dominant technological trajectories");

(iii) the (related) social dynamics underlying the development of technological systems and, together of communities of firms, technical societies, university discipline, etc.;

(iv) the patterns of corporate entry, exit and industrial dynamics.[16]

In a nutshell, our argument is that various forms of cognitive and decision biases are likely to be intrinsic ingredients of technological development and corporate strategies, including those concerning start-ups of new firms and diversification.[17]

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[16] The diversity between these fields and their relatively low degrees of communication with each other motivates also the choice of providing a rather extensive bibliography at the end of this chapter, which might help the reader in unfamiliar territories.

[17] Throughout the text, as a first approximation, we took a rather naive and anthropomorphic view of 'organizational decisions' (and related biases). In fact, our approach does not have any difficulty in accomodating a more complex view whereby organizational behaviour are also the outcomes of processes of political negotiation within the organization itself, grounded in the specific pieces of knowledge embodied in various 'experts' (e.g. the 'engineer', the marketing person, etc.) (See Lane et al. (1995). Also 'experts', our argument would go, are likely to display the biases discussed above. In fact, insofar as these experts share the knowledge of broader communities (e.g. software specialists, copyright lawyers, chemical engineers, etc.) they might partly curb the 'inside view' associated with each individual firm, but at the expense of bringing in the 'inside view' dominant in the expert community to which they belong.

This view easily links up with several other contributions to this volume. For example, it is certainly consistent with Richard Langlois "cognitive" analysis of corporate competences and behaviours. Indeed, the 'inside view' - with associated biases of "illusion of control", etc., discussed above - might be considered as essential corollary of cumulative and local learning, as analyzed by Daniel Levinthal. Hence, also the systematic errors of oversight of potentially rich opportunities, stubborn pursuit of past commitments or conversely overconfidence in novelty and change (cf. the chapter by Raghu Garud, Praveen Nayyar and Zur Shapira). Having recognized this sort of inevitability of errors - grounded in the very nature of individual and collective learning, and in the decision procedures of single humans and aggregates of them - there is little scope, in our view, to develop any sort of positive (or normative) theory able to accurately predict (or correct) these biases. However, we have suggested - largely in the form of a research agenda - that it might be possible to undertake sorts of taxonomic exercises mapping particular types of behaviour into particular characteristics of the knowledge bases upon which agents draw. We have outlined an example, linked to research in progress, and concerning entry decisions. Of course the first task is to show that entry patterns - as observed both cross-sectionally and longitudinally - are systematically affected by persistent decision biases. Second, we conjecture that the biases themselves (and, relatedly, post-entry performances) can be partly understood on the grounds of the learning regimes characteristic of specific industries and of their degrees of development (e.g. whether a dominant technological paradigm has emerged or not). In a somewhat similar spirit, Janet Berkovitz, John Figuercido and David Teece, in this volume attempt to map corporate strategies into characteristics of the decision problems facing the firms and the competences that they embody.

In any case, are decision biases necessarily 'bad'? At a first glance, an affirmative answer is based on the intuition that biases tend to degrade the future performances of the decision-maker, compared - as economists would easily do - with an agent endowed with 'rational expectations' and unbiases decision algorithms. However, in the final part of this work we have argued that what might hold for the individual agent (of organization) might not hold for the whole population of them, even for each of them over longer time spans. In the evolutionary interpretation we proposed mistakes, and biases that make these mistakes more frequent, are likely to be a necessary ingredient of the exploration of technological and organizational novelties. Paraphrazing Paul David (1992), collective change might generally require heros, herds and a lot of failures. And hence, biases and mistakes might be considered as a sort of powerful externality through which society learns.

We want to emphasize that there is no teleological connotation in that statement (i.e...biases exist because they are collectively useful...). Rather, this is primarily a conjecture on the collective dynamics of a particular form of social organization -call it "capitalism"- which, for reasons well beyond the scope of investigation of this paper, have been able to steadily generate these forms of "animal spirits". Indeed it might even be that, in one form or another, the strongest individual biases survive both heightened incentives and organizational processes across different cultures because they might have to do with some basic features of human cognition. This is clearly the view of the evolutionary biologist Lionel Tiger who discusses the evolutionalily useful role optimism likely played in our ancestors' ability to proceed with the hunt and find of new territory in spite of numerous dangers. He argues that

> "Thinking rosy futures is as biological as sexual fantasy. Optimistically calculating the odds is as basic a human action as seeking food when hungry or craving fresh air in dump. Making deals with uncertainty marks us as plainly as bipedalism.
> This has very practical outcomes. Is is relatively easy to cater to and exploit this 'psychological sweet tooth'. I believe that optimism,not religion, is the oppiate of the people. Religion is only one expression of the optimistic impulse. As well, exploitation based on optimism occurs in a wealth of places, not only religious ones; it occurs as much in betting shops as cathedrals and stock exchanges as confessionals" (Tiger (1979), p.35)

However, irrespectively of whether one entirely subscribe to this general anthropological view, and sticking nearer home, major implications follow from the foregoning argument, in terms of both theory and normative prescriptions.

To end provocatively on the latter: are we sure that we want to teach any sort of 'rational' decision-making in Business Schools? How can one avoid the risk that less biased assessment of any one decision environment yields more conservatism and slower collective change? Should not one emphazize the heuristics of knowledge accumulation capable of increasing the probability that biased gambles turn out to be self-fulfilling prophecies, rather than improving the 'quality' of decisions as such?

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