The essential tension in the social sciences: Between the “unification” and “fragmentation” trap *

By

Christian Knudsen,
Copenhagen Business School,
Department of Industrial Economics & Strategy,
Howitzvej 60
DK 2000 Frederiksberg
Denmark
e-mail:ck.ivs@cbs.dk
JEL Classifications: A12 and B40

ABSTRACT

A new framework is presented that suggests that scientific progress requires a balance between exploitation of existing research programs (normal science) and exploration of new research programs (revolutionary science) Too much pluralism can be as destructive for scientific progress as too little pluralism. In order to make progress in an intellectual field one need to uphold what Thomas Kuhn described as an essential tension between tradition and innovation. In the framework presented here, this implies balancing on a knife-edge trying to avoid falling into either a “fragmentation trap” or a “unification trap”. The “fragmentation trap” is a self-reinforcing process where the exploration of new theories completely comes to dominate the exploitation of existing research programs, while the “unification trap” is a self-reinforcing process where the exploitation of an existing research program completely comes to dominate the exploration of new research programs. A number of strategies for avoiding both the “fragmentation trap” and the “unification trap” are presented and discussed in relationship to management studies and economics, respectively. The framework is finally used to discuss the type of traps that faces different social sciences and the way they are organized as discussed by Richard Whitley in his comparative analysis of intellectual fields.

* Forthcoming in Hans Siggaard Jensen et al. (eds) 2002: **THE EVOLUTION OF SCIENTIFIC KNOWLEDGE.** Edward Elgar.

1. Introduction

The purpose of this paper is to define which intellectual structure best promotes the advancement of knowledge within the social science disciplines. A conceptual framework will be proposed that can analyse different intellectual structures and appraise how they perform in promoting scientific progress. By the term "intellectual structure" I refer to the distribution of activities that go on within a scientific field at a specific point in time. In this paper I will especially focus on the distribution between activities aimed at refining existing research programs (normal science) on one hand and activities aimed at searching for new research programs (revolutionary science) on the other hand. The question that I will explore is: What mix of the two types of activities best secure sustainable growth of knowledge in a field?

In answering this question I will put forward a framework that on the conceptual level is analogous to Schumpeter's thesis in the field of industrial organization (cf. Schumpeter, 1992). According to this thesis, neither perfect competition nor monopoly are optimal industrial structures for promoting (technological) progress. While perfect competition is too fragmented and monopoly is too concentrated, Schumpeter argued that oligopoly, by securing an optimal balance between static and dynamic efficiency, would be the market structure that best promotes (technological) progress.

In analogy with this Schumpeterian thesis, it will be argued in this paper that in order to make sustainable progress over a long period of time, a scientific field needs to secure some balance between the generation of new theoretical alternatives and the selection and retention of them. As a consequence we may find intellectual fields with either a too low or a too high degree of theoretical pluralism that each are confronted with a specific set of problems. Fields with too little theoretical pluralism run the risk of being caught in a unification trap (monopoly), while fields with too much theoretical pluralism runs the risk of being caught in a "fragmentation trap" (perfect competition). In the first case the elaboration, modification and extension of an existing research program tends to drive out the search for new research programs in a self-reinforcing process. In the opposite case a field will go on searching for new programs replacing one theory with another, without ever establishing ongoing research programs that lead to a coherent and cumulating body of knowledge. Securing a balance between tradition and innovation therefore implies establishing a field with a few competing research programs (oligopoly) that are constantly confronted with new tensions that drive the field towards new solutions and ultimately progress.

The paper is organized in the following way. In the next section I describe how productive research often emerges from essential tensions either within one research program or between two research programs. An essential tension is defined as a problem that cannot be solved within an existing research program but demonstrate the need for a more encompassing program. By viewing advancement in science as a process of creative destruction, it is argued in section 3, that it is necessary to uphold an unstable "essential tension" equilibrium in a field in order to avoid falling into either a "unification trap" or a "fragmentation trap". Different strategies for avoiding or getting out of the unification and fragmentation traps are discussed in section 4, using economics and management studies, respectively, as main examples. Which organizational structures of intellectual fields are exposed to the two traps is discussed in section 5, using Richard Whitley's (1984) comparative sociology of science framework.

2. The essential tension: scientific progress as a process of creative destruction.

What is productive or fruitful research? Though disagreeing on many other things, philosophers such as Popper (1972), Kuhn (1970,1977), Lakatos (1970), Laudan (1977), Hegel, etc. seem to converge on the view that productive research starts from some tension, inconsistency, opposition or paradox to stimulate the development of more encompassing theories or research programs. Since all programs are constraining a theorist's field of vision, path-breaking research will consist in removing such tensions, thereby creating new research programs that expand the explanatory capacity of the field. Tensions, inconsistencies or oppositions may exist either within a single research program or as an opposition between two or more research programs in a field.

In fields composed of only one research program, Kuhn argues that scientific revolutions would be unthinkable without the laborious puzzle-solving activity of normal science. The reason for this being that it is only through the activity of normal science that the anomalies that eventually contribute to replacement of the old paradigm with a new paradigm can be identified. There exist therefore what Kuhn calls an essential tension between tradition and innovation in science. If there is too much normal science, in the sense that normal scientists ignore anomalies by developing a "trained incapacity" to appreciate aspects not mentioned in the paradigm, no anomalies will be taken seriously and no new research programs will be forthcoming. If on the other hand there is too little normal science or exploitation of an existing research program, the research community will just replace one theory with another, without establishing any research programs. Or as Kuhn (1970) argues: "By ensuring that the paradigm will not be too easily surrendered, resistance guarantees that scientists will not be lightly distracted and the anomalies that lead to paradigm change will penetrate existing knowledge to the core. The very fact that a significant scientific novelty so often emerges simultaneously from several laboratories is an index both to the strongly traditional nature of normal science and to the completeness with which that traditional pursuit prepares the way for its own change" (1970:65). While researchers try to solve a problem within a research program they may end up creating solutions that destroy their original paradigm. Such a pattern of (scientific) development where a research program contain the seeds of its own destruction, closely resembles the "creative destruction processes" studied by Schumpeter in his work on the transformation of capitalist economies.

Hayek (1948) has, for instance, argued that the "knowledge problem" in economics – i.e. the problem of explaining how perfect rational agents can obtain enough knowledge about each others actions in order to reach an equilibrium solution – may be such an "essential tension" or paradox in the maximization paradigm. The paradoxical nature of this problem is caused by the fact that any solutions to this problem necessary leads to a destruction of the maximization program, because we need to introduce some kind of evolutionary mechanism working behind the back of the agents, i.e. the question of how equilibrium comes about cannot be posed in fully orthodox theoretical terms because it leads to a self-reference problem that makes it impossible to explain how economic agents in a interdependent system can acquire perfect knowledge about each others decisions. This implies that trying to solve this problem within the framework of the maximization framework destroys the framework pointing in the direction of an evolutionary-institutional research program (cf. Christian Knudsen, 1993).

While Kuhn with his predominantly mono-paradigmatic view of science mainly focused on tensions or paradoxes within a single research program, other philosophers of science have directed our attentions to multi-paradigmatic situations, where there exist tensions or oppositions between different research programs in a field. However, just as in the mono-paradigmatic case above, we may also have too much tradition or too much innovation depending upon how the community of researchers decides to deal with tensions and oppositions between theories (J. Holmwood & A. Stewart, 1994 and Poole & Van de Ven, 1989). If the researchers in a field with several research programs follow an isolationist strategy by continuing the internal development of their own research program ignoring what goes on in other research programs, the researchers choose to live with the tensions and oppositions instead of seeing them as opportunities for creating new ways of looking at the world that may lead to new synthesis and new insights. However, we may also commit the opposite mistake by thinking that these oppositions or contradictions between research programs may be resolved very easily by just testing the theories against the same empirical data in order to find out which one fits the data best. As Imre Lakatos (1970) reminded us, there are no such thing as a "crucial test" that immediately will decide between two competing theories or research programs.

Neither of the two strategies above seem, however, to uphold an essential tension that can drive the field through a creative destruction process by extending the boundaries of existing theories by building more encompassing theories or syntheses. Or as stated by Van de Ven & Scott Poole (1988) in relationship to organization and management theory "There are many ringing denunciations of opposing viewpoints, but too few attempts at bridging or synthesis. Hence, addressing organizational paradoxes is an exciting and challenging effort. It is an issue on the cutting edge of organization and management theory, and one that will spawn new ideas and creative theory. Looking at paradoxes forces us to ask very different questions and to come up with answers that stretch the boundaries of current theories" (p.25).

This Kuhnian view of productive science as approaching an essential tension between tradition and innovation, initiating processes of creative destruction, is in accordance with the Correspondence Principle of the Danish physicist Niels Bohr. Popper gives the following explanation of this principle " I suggest that whenever in the empirical sciences a new theory of higher level of universality successfully explain some older theory by correcting it, then this is a sure sign that the new theory has penetrated deeper than the older ones. The demand that a new theory should contain the old one approximately, for appropriate values of the parameters of the new theory, may be called (following Bohr) the "principle of correspondence" (1972:202). A "correspondence view" of scientific advance can be interpreted as dialectic in the sense that all problems in research emerges from tensions, contradictions or oppositions either within a single research program or between two or more research programs.

According to the correspondence principle, tensions, contradictions or paradoxes may emerge because the conceptual framework used in a theory T1 is too narrow to understand a specific phenomenon. In order to remove this tension or contradiction in theory T1, we may try to broaden the conceptual framework of the old theory to include what was excluded before, thereby constructing a more general theory T2. A scientific advance will therefore consist in the establishment of a correspondence relationship between the two theories. Such a relationship will exist if T1 is a satisfactory approximation for T2 within the domain D1, but T2 correct the explanations/predictions of T1 outside that range, i.e. in D2 – D1.

Proponents of an evolutionary research program in economics such as Schumpeter (1992), Winter (1975) and Nelson & Winter (1982) have all used the "correspondence view" of scientific advance as an argument for replacing the standard neoclassical research program with an evolutionary theory. Like Hayek's "knowledge problem", the dilemma or paradox that confronts the neoclassical research program and leads to the necessity of constructing a broader evolutionary research program, has been described in the following way by Nelson & Winter. " Thoroughgoing commitment to maximization and equilibrium analysis puts fundamental obstacles in the way of any realistic modelling of economic adjustment. Either the commitment to maximization is qualified in the attempt to explain how equilibrium arises from disequilibrium or else the theoretical possibility of disequilibrium behaviour is dispatched by some extreme affront to realism." Since the neoclassical program cannot solve the adjustment problem, this problem then leads, according to Winter, to an evolutionary research program, the purpose of which is "…to develop a more fundamental theory that explains both the range of validity of the approximations and the phenomena that lie outside that range" (1975:96). In accordance with the "correspondence view" of theoretical advance, the Evolutionary theory is used to determine the limited domain D1 of the neoclassical theory as well as studying new phenomena outside this range D2-D1: "The qualitative predictions of orthodox comparative static may well describe the typical pattern of firm and industry response in the dynamic, evolving economy of reality. However, evolutionary analysis probes more deeply into the explanations for these patterns and warns of possible exceptions. Also the explicit recognition of the search and selection component of adjustment brings a whole new range of phenomena into theoretical view" (1982:175)

3. Between the "unification" and the "fragmentation" trap.

Building upon the discussion in section 2 intellectual fields often experience problems of securing an essential tension between tradition and innovation. But what is worse, besides having problems with securing such a balance, intellectual fields are also constantly exposed to traps that may drive them into either a self-reinforcing spiral of elaborating upon existing programs or into a self-reinforcing spiral of search for new research programs.

In both cases, the possibilities of keeping an optimal balance between extending existing research programs versus searching for new programs or shortly securing an unstable "essential tension" equilibrium will be upset. In the following section I will try to explicate the mechanism driving each of these traps as well as discussing how these two traps interacts to upset any balance between them.

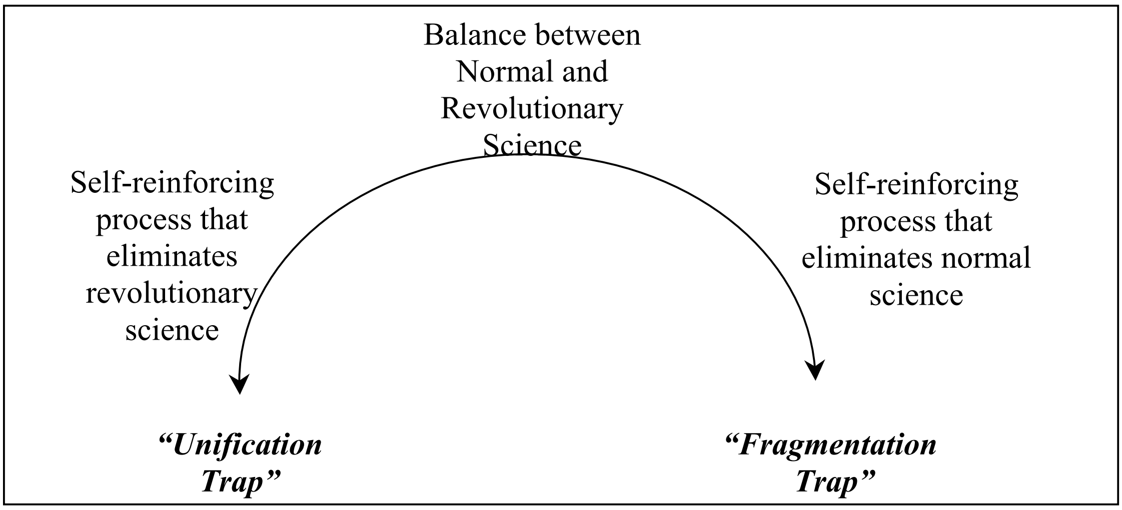

Fig. 1. The unification and fragmentation trap

The Unification trap and the case of economics in the post WW II period

The unification trap is present when normal science drives out revolutionary science and the activity of elaborating, modifying and extending an existing research program gradually comes to dominate the search for new research programs through a self-reinforcing process. As researchers in a field develop better and better skills in using the problem solving heuristic of an existing research program, this program will be even more used to solve new problems, etc. thus further increasing the strength of the positive heuristic and the opportunity costs of searching for new research programs. Acquiring competencies to solve problems within one research program therefore leads to more and more specialization within that program, making it more and more difficult for alternative research programs to compete. This self-reinforcing process may lead to a “unification trap” where all research activity in the field goes on within a single research program instead of being distributed between several competing research programs. The unification trap therefore emerges because the exploitation of already existing research programs gives a faster and safer return than the experimentation with new and uncertain research programs. The unification trap consequently implies a scarcity of exploratory activities that in the long run undermines the flexibility of the field by reducing its ability to adapt to new and unpredictable situations

Within the social sciences, economics is probably the only field that has been caught in a "unification" trap for an extended period of time during the hegemony of the maximization paradigm after World War 2. With the development of this program, economists developed a more and more refined mathematical heuristic that made it more and more attractive to use the neoclassical research program and its positive heuristic and less and less attractive to switch to any alternative program's heuristic. This self-reinforcing process of the "unification" trap lead, however, to an imbalance where heuristic progress (i.e. the development of the positive heuristic/problem solving methodology) came to dominate the empirical problem solving activity in the field. For economists that believe in efficient markets such a conclusion seem rather unlikely. Or as Grubel & Boland (1986) states:

"Economic knowledge and human capital are sold in markets. For most economists this implies a strong presumption that both are priced correctly and produced efficiently. Any university using too little or too much mathematics-teaching economists should find that its graduates are at a competitive disadvantage; its training program should shrink and finally disappear. Similarly, knowledge that contains inappropriate amounts of mathematics should lose out in the market and its production will contract or cease"(p.421).

But how is it possible that mathematical modelling of economic phenomena within the neoclassical research program during the post WW II period became the high prestige area in economics while applied research gave a much lower return in terms of reputation? According to Grubel & Boland (1986), this may be explained using a model of rent seeking behaviour. In such a model it is assumed that there are two groups of economic theorists: mathematical and applied researchers, that both attempt to generate economic rents for its members. The relatively lower rate of return to the rent-seeking behaviour of applied researchers is explained in the following way. First, since mathematical economists have fewer employment opportunities outside academia than applied economists, mathematical economists as group will have greater incentives to seek rents in the university environment. Second, since it is easier to formulate objective tests of competence in mathematical than within applied economics, mathematical researchers will find it easier to build barriers to entry and therefore to defend rents than any other group of researchers. Third, since mathematical economists are not facing any direct market tests like applied researchers, the demand for their products can be stimulated by themselves, leading to higher rents and reputation than other groups of researchers. Fourth, due to the universality of mathematics as a language, mathematical economists can build coalitions with other natural scientists, statisticians, econometricians and of course, mathematician, thereby achieving a higher degree of internationalisation than other subgroups in economics. The result is that mathematical economists through network externalities will earn higher rents and reputation that other group of researchers. Fifth, this dynamic process of reinforcement of mathematical economics was initiated in the early 1950s and 1960s. It received stimulus both from great optimism about the usefulness of natural science methods to the social sciences and the government support for the training of mathematical economists in the post-Sputnik era.

While economics was caught in a “unification trap” with the elaboration and extension of the positive heuristic of the neoclassical research program during 50s, 60s and 70s, the field seems to have escaped the trap during the latter part of this century. While there - with the exception of the Old Institutional Economics - were no heterodox traditions in economics in the period after the WW II, things started to change around the later part of the 1970s and the 1980s. At that time a whole set of theories – many with roots in the pre-war period and earlier - were marketed as new heterodox research traditions in the economic profession. Among those were Transaction Cost Economics, the Evolutionary Research Program, Austrian Economics, Post Keynesian Tradition, Property Rights Economics, Information Economics, etc. This signalled that economics had managed to get out the unification trap and that heterodox tradition managed to influence the type of problems taken up by mainstream economics. We will return to this subject in section 4 and 5.

The Fragmentation trap and the case of Management and Organization Studies

The second trap is called a fragmentation trap and is present when revolutionary science drives out normal science and the search for new research programs comes to dominate the elaboration, modification and extension of existing research programs.

There are several reasons why a scientific field may end up in a fragmentation trap. First, most new scientific ideas will be worse than the existing pool of ideas. Second, it takes a lot of time and experience before the positive heuristic of a new research program can be developed enough so that normal scientists can successfully exploit it. Even the research program that turns out to be most successful will normally perform rather badly to start with. Due to a lack of persistency in the scientific community many theories may therefore never be investigated well enough to become programs for research, before new theories have been proposed and have replaced them. The real potential of a theory to become a new research program will therefore never be discovered. And when the process - that drives new theories to be introduced in a field without replacing older theories - takes on a self-reinforcing character, the field ends up in a "fragmentation trap" with no chain of coherence through time or accumulation of knowledge.

In some cases the "fragmentation trap" is due to a "fad & fashion" mentality that implies that new approaches are introduced into a field at a faster and faster speed. In management studies Harold Koontz (1961) talked early on about "The Management Theory Jungle". Nineteenth years later he concluded, "the jungle appears to have become even more dense and impenetrable" (1980:175). In Organization Studies, Lex Donaldson (1995) argues, "since around 1967 at least fifteen new paradigms have been launched…on average a new paradigm is offered every second year" (p.7-8). Such a process of proliferation will typically start when a "value of novelty" (Pfeffer, 1993) or a " uniqueness value" (Mone & McKinley, 1993) that favours new ideas rather than integration and consolidation becomes dominant in a field. According to Mone & McKinley (1993) such a value has emerged within the fields of Organization and Management Studies as documented from statements of leading authorities, editors of leading journal such as Administrative Science Quarterly, Academy of Management Review and Organization Science. This value "prescribes that uniqueness is good and that organization scientists should attempt to make unique contributions to their discipline" (Moone & McKinley, 1993). Similarly, Pfeffer states that: "Journal editors and reviewers seem to seek novelty, and there are great rewards for coining a new term. The various divisions of Academy of Management often give awards for formulating 'new concepts' but not for studying or rejecting concepts that are already invented (1993:612).

However, the introduction of new approaches in both Management and Organization Studies – due to the "uniqueness value" - leads according to Moone & McKinley (1993) to a problem of "information overload". The more and the faster new paradigms are introduced into Organization and Management Studies, the less intellectual capacity will be available for exploiting and appraising the existing paradigms. This implies that paradigm proliferation will shift resources from normal to revolutionary science in a self-reinforcing manner. This leads to a fragmentation trap as formulated by Donaldson: "With the constant rush to the next paradigm the consequences is half-finished research programmes, as exemplified by structural contingency theory, where decades of research have left a literature widely perceived as containing unresolved theoretical problems and empirical inconsistencies…Reference to such problems is a standard argument for embarking upon the next new paradigm, but this argument can be self-defeating, precluding the completion of any research programme" (1995:10).

As argued by Zammuto & Connolly (1984) and Van de Ven (1997) the problem of "information overload" – while being caught up in a fragmentation trap - may be especially problematic to handle for new doctoral students. They will be confronted with a bewildering diversity of theories that they will have no chance of digesting. The background knowledge of the field will be "a morass of bubbling and sometime noxious literature. Theories presented are incompatible, research findings inconsistent, and the general body of knowledge indigestible" (p.32). The combination of the "uniqueness value" and a very fragmented knowledge structure will make it tempting for many doctoral students to learn only some of the newer and more exciting programs at the expense of the old. In this way, the field may end up in a situation where there is no effective transmission of knowledge between the old and the new generation and where older theories are not replaced by newer theories, but just forgotten. Being caught in a "fragmentation trap" implies therefore that the historical dimension of a field will tend to get lost. Few attempts will be made to show how newer contributions relate to earlier contributions by expanding upon and correcting older contributions. Instead newer contributions will just be introduced into the field without facing any demand that they somehow should solve problems that earlier contributions had been unable to solve.

How tradition and innovation interact to undermine a healthy balance between themselves

In order to secure the unstable "essential tension" equilibrium, scientific fields may be seen as constantly trying to avoid getting locked into either a self-reinforcing "unification" or "fragmentation" trap. However, there exist very complex interactions between activities of exploiting an existing research program and activities of searching for new theories that will tend to undermine any kind of balance that may exist between them.

Elaborating, modifying and extending an existing research program tends to undermine extraordinary science by discouraging attempts of finding new research programs and problem solving heuristic that are essential for the long-term survival of a field. Researchers in the field therefore either tend to stick to one (currently progressive) program and its problem solving heuristic to such an extent that there is little exploration of other programs. Or they fail to stick long enough to one (underdeveloped and currently degenerating) program long to determine its "true" problem solving capacity.

In a similar way, revolutionary science undermines normal science. Efforts to promote revolutionary science encourage impatience with new theories and make the development of new problem solving heuristics very unlikely. Theories are therefore likely to be abandoned before enough time has been devoted to develop them into research programs with a specific heuristic. The impatience of revolutionary science therefore results in unelaborated discoveries and a fragmented knowledge structure. As a result of the way normal and revolutionary science interacts, most scientific fields will have difficulties maintaining a healthy tension between them. This tendency to undermine each other raises the problem of what strategies scientific fields have in fact used in order to keep a balance between them, thereby avoiding both the unification and fragmentation traps. In other words what kind of "competition policies" have been implemented in different scientific fields?

4. Strategies for avoiding the "unification trap" and the "fragmentation trap" in the social sciences.

Introduction

By arguing that self-reinforcing processes and traps characterize intellectual fields several important policy issues may be brought forward. For instance, if positive feedback loops might lock in a dominant research program such as neoclassical economics for long periods of time, regardless of the intellectual progress actually generated by it, is it then sensible to talk about an open market for ideas? What can be done about the substantial sunk costs and high entry barriers that confront unorthodox competitors? Conversely, what policies may be recommended in case a field is exposed to a feedback loop that leads to its fragmentation? When one theory is introduced after the other in rapid succession, it will be impossible to evaluate which ones are good and which ones are bad, thereby destroying the possibility of having a cumulating body of knowledge. In this case, we may investigate policies to reduce the speed with which new alternatives are introduced into the field.

Before entering this discussion about different strategies to avoid dilemmas and traps in a scientific context, I will reflect upon how these discussions should be conducted? Firstly, I will suggest that debates about appropriate strategies should be conducted in a comparative context so that we avoid policy conclusions that only rest upon experiences from a single field, but use data from a broader set of fields. In this way we may avoid the "Panglossian bias" of many researchers who view the structure of their own field as the only natural/possible way to organize a field. Secondly, policy debates should preferably be conducted on a constitutional level, which implies that the discussions about what rules (conventions, norms, etc) should govern a specific field preferable should be conducted behind a "veil of ignorance" (Rawls, 1971) or a "veil of uncertainty" (J. Buchanan & G. Tullock, 1962); that is, without knowing what implications these rules may have regarding the choice between specific theories or research programs.

The unification trap and the case of economics

In the following section we shall discuss some strategies that a research community may suggest in order to avoid a self-reinforcing unification trap using economics as the main illustrative case. These strategies include a) Promoting the isolation of young heterodox research programs during their maturation b) Building heterodox traditions around core anomalies in mainstream economics and giving priority to a strengthening of their positive heuristic and c) Changing the composition of research styles in an intellectual field.

a) Promoting the isolation of young heterodox research programs during their maturation

In his “Open Society and its Enemies” Karl Popper (1945) argued that the more ‘open’ a scientific field is in terms of accepting competing research programs, the tougher the competition and the better the chances for a scientific break-through. For the same reason, the scientific community should be very lenient towards new research programs, in order to make sure that they get enough time to mature, before being exposed to the fierce competition of older and more mature research programs. In accordance with this position, Imre Lakatos argued that ”we must not discard a budding research program simply because it has so far failed to overtake a powerful rival. As long as a budding research program can be rationally reconstructed as a progressive problem shift, it should be sheltered for a while from a powerful established rival” (1970:157).

In economics, for instance, the maximization program has been elaborated, modified and extended for many years. A lot of sunk costs have been spent on this program and it is, therefore, very unlikely that a competitor with a better problem solving capacity or a stronger heuristic to suddenly emerge. New research programs such as the behavioural program, the evolutionary program, the new institutionalist program, etc. should therefore be protected during their infancy in order to make sure that they get time to develop and strengthen their heuristic before a verdict can be made. In fact, this argument is analogue to the “infant industry” argument of Friedrich Litz recommending that new firms should be protected from outside competitors until they have grown strong enough to be exposed to the fierce competition of the world market from older and more mature foreign competitors.

Terence Ball (1976) has raised a similar argument in political science. He argued that Marxism and functional analysis as scientific programs might have been killed off prematurely, because their protagonists lacked the necessary tenacity and their critics the necessary tolerance required to give these programs a fighting chance. He concluded:

“We political scientists have not, I fear, treated our budding research programs (or traditions) very leniently. On the contrary, we have made them into sitting ducks; and, in a discipline which includes many accomplished duck hunters, this has often proved fatal…if we are to be good sportsmen we need to take Lakatos’ scheme seriously” (p.34)

This implies that we should be more tolerant when criticizing a research program than assumed by Popper’s methodology of naive falsificationism. Purely negative and destructive criticism, like a refutation will never be enough to kill a program. We will only be able to reject a program when we have a new and better theory or program. This leads us to the second strategy.

b) **Building heterodox traditions around core anomalies in mainstream economics and giving priority to building a strong positive heuristic**

Though economics has often been portrayed (mostly by other social sciences) as having a completely unitary structure, today the field includes several heterodox traditions that during the last 20 or 30 years have influenced mainstream economics in fundamental ways. In comparison with the older heterodox traditions such as the Old Institutionalism, some of the new heterodox traditions have not been marginalized in the same way. Consequently the field has recently been able to move out of the “fragmentation trap”, getting closer to an ”essential tension” equilibrium with a better mix of normal and revolutionary science. The main reason being that the new heterodox traditions seem to have followed a strategy that on the one hand focuses on the core anomalies in the mainstream paradigm and on the other hand gives priority to strengthen the positive heuristic of these newer research programs.

New heterodox traditions such as Transaction Cost Economics, the Evolutionary Research Program and the Knowledge-Based program start from the assertion that a purely negative critique will be insufficient in order to replace the mainstream tradition. What is needed is that the new heterodox traditions are able to identify some core anomalies in the orthodoxy and to show how a solution to this problem will lead to the replacement of the mainstream tradition with a newer research program. An example of such a core anomaly is the so-called "knowledge problem" that potentially may be solved by switching from the mainstream tradition to an Institutional-Evolutionary research program. The argument is that you will never "beat" a research program just by identifying some anomalies within it. In order to do so you need a new and better research program. It is for the same reason that several new heterodox traditions have allocated so many resources towards improving the problem solving capacity of the new research programs by strengthening their positive heuristics.

c) Changing the composition of research styles in an intellectual field.

When studying scientific fields most philosophers of science tend to view science from a typological rather than from a population perspective (cf. E. Mayr, 1976). A typological view of a scientific field assumes that all researchers within a field follow the same rule of rationality, converging to the same uniform decision, and therefore approximates the same common underlying ideal type. Consequently, variety or the co-existence of a plurality of approaches will either be very difficult or even impossible to understand from such a perspective. For instance, from the view of Imre Lakatos MSRP, continuing the work on an old degenerating research program when a new progressive program emerges can only be understood as involving a "mistake" or "irrational behaviour" on behalf of the researchers staying with the old program.

Variety or the co-existence of several research programs takes centre-stage in a population perspective. In this case, we switch to a truly system (population) level of analysis by asking what distribution of individual research strategies may be rational (conducive to scientific progress) for the field as a whole (cf. P. Kitcher, 1993). Let us assume that the research community may be described by different styles of research.

First, there is the “orthodox normal scientist” that will stick with the paradigm, whatever happens. Second, there is the “standard normal scientist” that will be ready to reject an old paradigm when there are clear indications that a new paradigm will supersede the old paradigm. Thirdly, there is the “essential tension researcher” that exploits anomalies in the old paradigm(s) in order to create new research programs. And finally, there is the “fashion-driven” researcher that value new research programs more than old research programs just because they are newer.

The existence of an uneven distribution of research styles in a field, with a dominance of the first mentioned, will lead to a “unification trap”. In this case, a strategy for getting out of this trap would consist in trying to create a more even distribution by favouring the last mentioned research styles when recruiting to the field. This may be done by consciously promoting entry of new heterodox researchers into the field.

The “fragmentation trap” and the case of management/organization studies.

In the following section we shall discuss 3 strategies that researchers may use in order to avoid a self-reinforcing fragmentation trap. The illustrations will come from the field of management/organization studies. These strategies include a) Increasing the persistency in the research community 2) Focusing on tensions and oppositions between theories and research programs and 3) Condensing the knowledge structure in order to increase the absorptive capacity of an intellectual field.

a) Increasing the persistency in the research community

An important reason why a field may end up in a fragmentation trap is that there is too little persistency and too much impatience in the scientific community. Since new theories and approaches need a lot of refinement and development before their true value in terms of heuristic power (problem solving capacity) can be determined, the high rate of “turn-over” will imply that the selection mechanism will function very imperfectly. As we argued earlier the individual researchers as well as the research community will be confronted with a problem of “information-overload” (Mone &

McKinley, 1993), since new theories are introduced into the field at a rate that makes it impossible to identify the theories with the best heuristic or problem-solving capacity. In other words the variation mechanism that produces new theories will completely outperform the selection mechanism. As a consequence, theories will just succeed each other in an endless cycle of failure and change without any real accumulation of knowledge. That is, they find themselves in a typical fragmentation trap.

Fields that ends up in a fragmentation trap do rarely have a good sense of their history because the researchers are too busy to keep up with the newest "fad & fashion" to look back upon the historical roots of their field. Or as Michael Reed (1992) has argued in relationship to the field of Organizational Studies:

"Any sense of historical continuity and narrative coherence is lost in the clamour of voices announcing the 'end of history' and extolling the virtues of root and branch transformation from the 'old' to the 'new organization theory" (1992:246)

Furthermore, as we shall discuss below the knowledge structure is too fragmented and diffuse in order to be of any help to researchers in their current problem solving activities. Introducing a "conservative bias" towards the status quo in such fields would therefore be a strategy that can reduce the high "turn over" rate of new theories, thereby securing a better balance between continuity and change. One way that such a "conservative bias" strategy could be implemented is to demand that only theories that build upon and correct older theories should be taken as serious candidates for being included in the background knowledge structure of the field

b) Focusing on removing tensions and oppositions between theories and research programs in the field.

In cases where a field is caught in a "fragmentation trap" any attempt to isolate theories or research programs rather than confront them with each other will be seen as counterproductive. For instance, both in management studies and organization studies there are many supporters of an eclectic research strategy (cf. Mintzberg, Ahlstron & Lampel, 1998). However, supporters of this strategy often seem unaware of the negative implications that such a strategy may have regarding the knowledge structure of a field by moving the field away from an “essential tension” equilibrium.

An eclectic research strategy will often lead to a high degree of theoretical conservatism rather than theoretical radicalism. The reason for this is that adherents of the eclectic strategy are willing to accept and live with very varied – and in some cases even contradictory – theoretical perspectives. This implies that inconsistencies and tensions in the field are accepted, rather than acted upon. Consequently, tensions and inconsistencies in the knowledge structure of the field are not seen as leading to new research problems and new research opportunities, though a closer investigation of them may have revealed that they could have done so. Another implication of the eclectic type of research strategy is that the members of the research community will find it more and more difficult to communicate with each other the more theories that are introduced and the further they get stuck in the fragmentation trap.

The only way to reverse this tendency towards a fragmentation trap is therefore to give up the eclectic research strategy and instead focus on removing tensions and opposition between theories and research programs. This necessitates continuous investments in research that tries to solve all kinds of conceptual problems that emerge as contradictions or tensions between the different parts of the knowledge structure. Since these conceptual problems (and the kind of foundational research that is implied) are often looked upon as second order problems, they are not considered as important to solve as empirical problems.

c) Condensing the knowledge structure in order to increase the absorptive capacity of an intellectual field.

A continuous accumulation of new theories and methods in a field will sooner or later create a complexity crisis, because the knowledge structure will become too diffuse and disintegrated. Adding new layers of knowledge in a field, without at the same time trying to “condense” the knowledge structure by ordering/mapping the different theoretical contributions, will sooner or later lead to severe inefficiencies that reduce the absorptive capacity of an intellectual field. Or as Jeffrey Pfeffer (1982) states in relationship to the field of organization studies: “There are thousands of flowers blooming, but nobody does any manicuring or tending” (p.1). In a field that is exposed to such a “fragmentation trap” the researchers will therefore experience that they have difficulties in “standing on the shoulders of their predecessors”, because there is no real structure of knowledge to start from.”

One way to reverse this self-reinforcing tendency toward a “fragmentation trap” is to make investments in structuring the background knowledge of the field. That is, changing the balance between continuity and change by moving resources from the latter to the former. This may be done, for instance, by using resources to map the existing theoretical contributions within a field, by identifying the main dimensions along which they differ, by specifying the formal and substantial relationships between individual contributions, by identifying important oppositions between different contributions, etc. In organization and management studies, examples of such contributions are Burell & Morgan (1979), Astley & Van de Ven (1983), Pfeffer (1982), W.R. Scott (1982), etc. Structuring the background knowledge of a field in this way will make it more likely to be used in future problem solving activities. It will therefore be a strategy for increasing the absorptive capacity of a field. It will also increase the chances for finding more encompassing theories that may further reduce the fragmentation of a field.

Working on “condensing” the knowledge structure in a field may have significant efficiency implications, since this determines to a large degree the production time for new knowledge contributions. If the knowledge structure is very complex, consisting of many layers of old knowledge that has only been condensed to some degree, the production time of new contributions is relatively high. One direct way to measure or estimate this “production time” is to find out how much time a newcomer to the field needs to recapitulate the history of the field, in order for him or her to make a new knowledge contribution. The idea that a new member of a scientific field “recapitulates” in a condensed version the evolution of the field goes back to the thesis that “ontogeny recapitulates phylogeny” in biology. This thesis states that every new member of a species recapitulates in a condensed way the evolution of the whole species. In a scientific context, according to Herbert Simon (1962), this thesis states that every new member of a scientific field will recapitulate – in a very condensed way – the historical evolution of the field, in order to make a new knowledge contribution.

In fields where a lot of resources have been spent on formalizing and thereby condensing the knowledge structure, the production time and the age of new contributors may be lower than in fields where this is not the case. However, investments in removing tensions and oppositions, thereby keeping the knowledge structure relatively simple and manageable are primarily done in order to increase the absorptive capacity of the researchers within the field (cf. Cohen & Levinthal, 1990); that is, to secure long-term scientific progress in the field.

5. The "unification" vs. "fragmentation" trap in fragmented adhocracies, polycentric oligarchies and partitioned bureaucracies.

In the following section I will show how the discussion of the unification trap vs. the fragmentation trap fits into Richard Whitley's (1984) discussion of the intellectual organization of scientific fields. Whitley's argues that it is possible to identify very different modes of how scientific fields are organized as reputational systems based on the following two dimensions: 1) degree of interdependency and 2) degree of task uncertainty.

The degree of interdependency refers to how many researchers in a field are dependent on each other to obtain reputation. The more applied a field is, the more open it will be towards its environment and the less interdependency there will be. Conversely, the more basic a science is, the more researchers have to rely on each other for obtaining reputations. The degree of task uncertainty refers to the degree of uncertainty a researcher faces when trying to solve a specific problem. It is normally claimed that the main function of science is to produce new knowledge. What is accepted as new knowledge depends to a large extent on the background knowledge of the field. As I argued in section 4, the more systematic, exact and general this knowledge is, the easier it is to determine whether a contribution is new or not and how well this contribution fits into the background knowledge of the field. If the background knowledge is well structured, which is the case for mono-paradigmatic fields, the task uncertainty will be low. Whitley (1984) furthermore distinguishes between two different aspects of task uncertainties, technical and strategic. Technical uncertainty refers to the degree of unpredictability and variability that exist, in a field with regard to the methods and procedures for solving empirical problems. If there exists many different methods and if it is difficult to interpret the (test) results in a field, the degree of technical uncertainty is high. On the other hand, if a certain method has been canonized as being the only legitimate method in a field, the degree of technical task uncertainty is low. Strategic uncertainty, on the other hand, refers to the degree to which researchers agree upon which problems are important, less important, etc. and what goals should govern their research. In fields with a high degree of strategic task uncertainty, researchers will be confronted with many different problems, the relevance and importance of which are appraised very differently.

According to Whitley, variations in these two contingency variables make it possible to distinguish between at least seven different configurations of how scientific fields are organized. However, since we are primarily interested in the social sciences, the discussion here will be limited to the three configurations found in this area. These include the “fragmented adhocracy”, the “polycentric oligarchy” and the “partitioned bureaucracy”.

	Degree of Interdependency	
Degree of Strategic Task Uncertainty	**Low**	**High**
High	*Fragmented Adhocracy*	*Polycentric Oligarchy*
Low	*Unstable form*	*Partitioned Bureaucracy*

Figure 2: Reputational Organizations in the Social Sciences

Social science fields such as sociology, management studies, anthropology, political science, etc. have rarely been dominated by a single paradigm, as is the case for some natural sciences. We should therefore expect that these fields have a substantially higher degree of technical and strategic uncertainty. The only social science that has diverged from this pattern is economics, which for a long period, has been dominated by the (neoclassical) maximization paradigm and therefore has a substantially lower degree of strategic task uncertainty and a higher degree of interdependency than the other social sciences. According to Richard Whitley (1983), the reputational configuration of economics may be characterized as partitioned bureaucracy.

As a **partitioned bureaucracy,** economics consists of a core with pure and abstract theorizing (within the maximization paradigm) and a number of peripheral sub-fields of applied research. Due to the absence of control over the object of research and the ambiguity of empirical testing in the social sciences, any unifying theoretical framework in a social science will be under a permanent threat to be replaced. In economics, however, this problem was solved by partitioning the core of pure theory with formal mathematical modelling from the applied and empirical research in the peripheral areas. Compared to other ways of organizing social science fields, economics has a very hierarchical type of reputational organization, since research in the core of the field is viewed as much more prestigious than the applied research in the sub-fields. The term 'partitioned" in partitioned bureaucracy refers to the absence of feedback from the applied research in the periphery to the pure theory in the core, i.e. the abstract models of the maximization paradigm have been "immunized" from "potential falsifications" arising in the applied field.

The second type of organizational configuration found in the social sciences, according to R. Whitley (1984) is the **polycentric oligarchy**. Examples of this structure are classical continental sociology, British social anthropology and as I have argued in a recent paper Organization Theory in the US after 1975 (cf. Christian Knudsen, 2003). The polycentric oligarchy emerges when relatively small groups of researchers gain control over critical resources such as positions and journal access. But since the degree of task uncertainty is very high, their control can only be exercised locally and personally, resulting in the establishment of several independent centres. In Organization Theory these centres were formed around the main research programs that emerged in the late 1970s such as population ecology, transaction cost economics, institutional theory and resource dependency theory. Within each of the research centres formed around these research programs, there was a relatively strong hierarchical reputational organization due to a consensus of what was the basic framework to be used, what were the important problems to be solved and how reputation should be allocated within the "specialized" research community. However, there was very little coordination and cooperation between the centres, and an intense competition in order to gain control over the whole field. Consequently the field became balkanised into a set of more or less autonomous centres, each pursuing their own research agenda, with minimal interaction and communication.

The **fragmented adhocracy** that may be found in management studies (Whitley, 1984a) is characterized by a low degree of interdependency between researchers, which implies a rather loose research organization. Since researchers are facing few restrictions regarding the choice of theoretical framework and choice of method, the degree of technical and strategic task uncertainty is very high. This implies a relatively fragmented knowledge structure and the structure of much disagreement about the relative importance of different problems to be solved by the field. As a result, the problem solving activity within the field takes place in a rather arbitrary and ad hoc manner, with limited attempts to integrate new solutions with the existing structure of knowledge.

But how does the framework presented in this paper fit into Whitley's comparative analysis of intellectual fields? There seems to exist a very simple answer to this question following from the discussion above. If a field is very hierarchical in its reputational organization, which is the case for the partitioned bureaucracy (low degree of strategic task uncertainty and a high degree of interdependency), the field will typically be struggling to avoid or get out of a "unification trap". If the field, on the other hand, has a very flat reputational configuration which is the case for fragmented adhocracies (high degree of task uncertainty and low degree of interdependency) the field will be struggling to avoid or get out of a "fragmentation trap". Fields with an organizational structure that is situated between these two extremes such as the polycentric oligarchy will on the other hand be closer to maintaining an unstable "essential tension" equilibrium between tradition and innovation or a balance between elaborating existing research programs and searching for new programs.

6. Conclusion

Philosophers of science such as Karl Popper (1945) have argued that the more diverse or pluralistic a field becomes the tougher the competition will be and the better will the chances for a scientific break-through be. In this paper I have argued that such a policy prescription between the degree of pluralism and scientific progress need not be generally valid across all fields. In accordance with the Schumpeterian thesis in competition policy, more pluralism may have positive as well as negative consequences for scientific advance in a field, depending upon how far the field is from the unstable "essential tension" equilibrium. If a field is already caught in a "fragmentation trap" a policy of pluralism will be counterproductive, since it will just lead to more and not less fragmentation. If the field, on the other hand, has been caught in a "unification trap" a policy of increasing theoretical pluralism may have a positive effect. The structure of a scientific field that best seems to avoid both the "fragmentation trap" and the "unification trap" is the polycentric oligarchy.

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