

# **PLURALISM, SCIENTIFIC PROGRESS AND THE STRUCTURE OF ORGANIZATION STUDIES<sup>1</sup>**

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## **ABSTRACT**

Should organization studies aspire to be a mono-paradigmatic science as argued by Pfeffer or should we pursue a strategy of unconditional pluralism by “letting thousands flower grow”? A new framework is presented that suggests that scientific progress in organization studies will best be promoted by upholding a balance between the exploitation of existing research programs and the exploration of new research programs. Too much pluralism can be as destructive for scientific progress as too little pluralism.

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During the 1980s and 1990s organization scholars have increasingly been debating whether the coexistence of several research programs was a blessing or a curse for the advancement of their field. Two opposing positions have gradually emerged that support either a **unification** or a **pluralist** prescription. Those who support the unification position argue with reference to Thomas Kuhn (1970) and Michael Polanyi (1958) that upholding a scientific consensus is a necessary condition for the accumulation of knowledge. If researchers in a field like organization theory “were too willing to accept every unorthodox theory, method, or technique, the established consensus would be destroyed, and the intellectual structure of science would become chaotic. Scientists would be faced with a multitude of conflicting and unorganized theories and would lack research guidelines and standards” (S. Cole, 1983:135). The main advocate of this position in organization theory, Jeffrey Pfeffer (1993), argues that scientific fields that are more paradigmatically developed will tend to advance more rapidly, fare better in the contest for resources, have larger and better organized “invisible colleges”, have lower journal rejection rates, have fewer coordination problems and therefore will be more likely to take advantage of the benefits of team work than paradigmatically less developed fields. On the other hand those that support a pluralist position argue that the more research programs are advanced the better for the long-term growth of the field. Several reasons have been given to support this position. One reason is that in order to understand the highly complex reality different theories are required since most theories only highlights one aspect of this world. Another reason is that the existences of many theories promote competition and increase the chances for scientific advances. One proponent of this pluralist position in organization theory, John Van Maanen (1995a & b), therefore argues that one of the most important issues related to theory development in organization theory is to

answer the following questions. What institutional arrangements are more likely to facilitate tolerance, learning and conversations? What are the conditions that surround productive scholarly exchanges in the field? However, Van Maanen do not raise the question whether the strategy of “letting thousands flowers bloom” actually is a good or bad strategy for organization theory.

The main purpose of this paper is to define what intellectual structure best promotes the advancement of the field of organization studies. A conceptual framework is proposed to analyze different intellectual structures and appraise how they perform in promoting scientific progress. The term “intellectual structure” refers to the distribution of activities within a scientific field at a specific point in time. This paper will especially focus on the distribution between activities aimed at refining existing research programs on one hand and activities aimed at searching for new theories on the other hand. In accordance with studies of adaptive processes, it is argued that in order to make progress over a long period of time a scientific field has to secure a balance between the generation of new theoretical alternatives and the selection and retention of them. As a consequence we may find intellectual fields with a too low as well as a too high degree of theoretical pluralism that each are confronted with a specific set of problems. In fact, it is argued that both the unification strategy and the pluralist strategy may lead to intellectual structures that have sub-optimal traits by either having too little or too much pluralism. Fields with too little pluralism run the risk of being caught in a **specialization trap**, while fields with too much pluralism run the risk of being caught in a **fragmentation trap**. Both of these traps emerge as a result of self-reinforcing processes where either the activity of extending an existing research program (normal

science) or the activity of searching for new research programs (extraordinary science) get reinforced and sooner or later become dominant in the field.

The **specialization trap** - where the elaboration of a dominant research program completely comes to drive out the search for new and heterodox theories – emerges because the exploitation of an existing research program gives a much faster and safer return than the experimentation with a completely new and uncertain research program. As researchers develop better and better skills at using the problem solving heuristic of a research program they will be inclined to use this program even more in the future, thus further increasing the strength of its heuristic and the opportunity costs of switching to another research program. The consequence of this trap is a scarcity of exploratory activities that undermines the long run adaptability of the field to new and unpredictable future situations. This is the main risk of following the unification strategy in organization theory proposed by Pfeffer (1993) and later supported by Donaldson (1995, 1996a).

The second trap is called a **fragmentation trap**. Its main characteristic is that the search for new theories comes to dominate the activity of extending and elaborating a research program. The fragmentation trap will emerge when too many new theories are proposed at a too fast pace in order for the scientific community to be able to evaluate each contribution properly and to integrate them into a reasonable coherent knowledge structure. Three implications follow from this. First, single theories are typically not turned into coherent research programs with their own unified way of solving problems, because there is too little persistence in the community of researchers to “stick to” one theory in order to investigate its

potential for solving other problems as well. Second, there is not enough time to determine the relationship between the different theories i.e. whether they compete or complement each other. Third, since the relationships between old and new theories are never determined there will be no cumulative growth in the field. New theories will either just succeed – rather than replace - old theories until one or both of them are forgotten. As a consequence the knowledge structure of the field will become more and more fragmented, since new theories are just tacked onto the existing structure in an ad hoc manner rather than being integrated with the fond of existing contributions. And with a more fragmented discipline it becomes more and more difficult to use the existing knowledge structure to construct new contributions, which will undermine the chances for scientific progress in the future. This is the risk of following the pluralist strategy proposed by authors like Van Maanen (1995) and Daft and Lewin (1990) in organization studies itself.

Contrary to both the unification and the pluralist position this paper argues for a position of “limited pluralism” which proposes that upholding a balance between extending existing research programs and searching for new theories is a prerequisite for scientific progress in the field of organization studies. In fact, this “third” position should not be seen as surprising since it is just an application of organizational learning theory (March, 1991; Levinthal & March, 1993) to the field of organization studies itself.

But before proceeding any further with the analysis let me shortly describe the perspective from which the field of organization studies is viewed and the methodological position adopted in this paper. The main argument is that the relationship between pluralism and

scientific progress is a contingent rather than an absolute one in the sense that this relationship depends on the ‘social structure’ of the research community of the field. Consequently, a major part of the paper will look at the field of organization studies from a “sociology of science” perspective, with an attempt to establish what ‘social structures’ have characterized the field at different times. Being a ‘non-cognitive’ type of sociology of science though, it is closer to the classic sociology of science represented by R. Merton than to the more recent sociology of science of B. Latour (1982). And by studying which effects a higher degree of theoretical pluralism may have on scientific progress, given a specific type of social structure in the field, I assume a **non-relativist** methodological framework in which it is possible to compare different research programs. Contrary to some post-kuhnian and post-modern views, it is argued that there can be rational debates, communication and choices between the different theories and paradigms in organization studies.

**A review of the debate on the relationship between pluralism and scientific progress in organization studies.**

According to several reviewers (e.g. Anna Grandori, 1987; Michael Reed, 1992) from the late 1950s to the late 1970s organization studies has been characterized by a relatively high degree of consensus with regard to both theoretical and empirical issues mainly due to the hegemony of the structural contingency program. The emergence and later coexistence of several competing research programs in the mid 1970s such as population ecology, neo-institutionalism, resource-dependency theory and organization economics made it significantly more difficult in organization studies to agree on how to prioritize problems and

choose between methods to use in order to solve these problems. As a result, the uncertainty facing the individual organizational researcher in performing his or her research activities has increased significantly. The increase of theoretical pluralism has also produced a field in which the allocation of reputation became more ambiguous and the structure of knowledge became more complex than before.

As one may suspect the high degree of theoretical diversity in organization studies has been appraised very differently. In the period just following the break down of the hegemony of the Structural Contingency Program, many organizational researchers expressed positive feelings regarding the new state of affairs in the field. Not only were organization researchers more receptive and more willing to take on new theoretical and empirical problems, but they were also open to a range of non-positivist methodological frameworks. Burrell & Morgan (1978) were among the first to map the field according to which meta-theoretical positions could potentially be developed within the field. Like many other organization researchers, Burrell & Morgan, regarded this development of the field from a mono-paradigmatic to a multi-paradigmatic state as being very positive. However, looking upon the field from a kuhnian perspective, they came to the conclusion that the major paradigms in the field were mutually exclusive and incommensurable, since there were no paradigm independent criteria to appraise which of a set of competing paradigms should be preferred. Burrell & Morgan even suggested that the incommensurability thesis could be used by new heterodox paradigms as a way of defending themselves against the mainstream functionalist paradigm through arguing for paradigm closure and **isolationism**.

More subtle statements later replaced the early statements regarding the virtues of theoretical diversity and paradigm closure in organization science. Willmot (1990), Hassard (1993), and Reed (1985) found that Burrell & Morgan's acceptance of the incommensurability thesis and the idea that there can be no communication between paradigms in organization theory was unwarranted. Furthermore, Scott (1998) argued that there exist very different relationships between research programs or "perspectives" in organization theory. In some cases, the perspectives are "partially in conflict"; in other cases they "partially overlap" and in still other cases they "partially complement one another" (p.31). Although the different perspectives have emerged at different times, according to Scott, the later perspectives have not been able to completely supplant the earlier ones. Instead, they "continue to coexist and to claim their share of advocates" (p.31). This trend of an increasing number of theoretical perspectives in the field may, according to Scott, pose some severe problems. For instance, as more and more new perspectives are introduced into the field, the background knowledge becomes more complex and it is increasingly difficult to uphold a consensus in the field. However, as Scott argues, "the existence of multiple paradigms may reduce consensus and support, it does not thereby necessarily reduce the power of the ideas and the value of possessing multiple lens through which to observe our world" (118-9).

Recently, a more critical attitude towards theoretical pluralism has emerged. In his early attempt of mapping organization studies by setting up a typology of different research programs, Jeffrey Pfeffer (1982) noticed that due to the proliferation of middle range theories, the field had become a "weed patch" rather than a "well-tended garden". This tendency towards fragmentation of organization studies due to the proliferation of new

research programs could, according to Zammuto and Connolly (1984), be counteracted by teaching new students about the social structure of the field and about strategies for coping with the information overload resulting from the fragmentation. Mone and McKinley (1993) explained the fragmentation of the field as a result of the diffusion of a "uniqueness value", i.e. a value that emphasized the drive to be novel, original, innovative, etc. rather than a value to conform to and to extend the existing research programs. Furthermore, they argued for the existence of several mechanisms that will further reinforce or promote adherence to the "uniqueness" value, indicating that the tendency towards fragmentation will, indeed, be very difficult to reverse. Donaldson (1995) took this argument one step further by proposing that the proliferation of paradigms reflects a pathological status contest among individuals who gain a higher reputation by creating new research programs than by expanding and testing existing research programs. Donaldson complained that several of the new research programs reflect an anti-managerial bias among American organization theorists.

The most coherent statement regarding the fragmentation of organization studies so far was published by J. Pfeffer in 1993 with the title: "Barriers to the Advance of Organization Science: Paradigm Development as a Dependent Variable". Pfeffer argued that the increased theoretical diversity had several negative implications, including a break-down of the consensus among scholars that were unable to agree on core issues such as what research should be funded, what articles should be published, etc. This lack of a consensus furthermore weakened the support for the field from the universities and from the state. And if this weakening of the consensus and fragmentation of the field continued, organization studies would, according to Pfeffer, "remain ripe for either a hostile takeover from within or

from outside” (1993:618). Indeed, the field would come to look more and more like political science, which had been taken over by the "rational choice" paradigm imported from economics.

It was against this background that Pfeffer argued that organization studies needed to be much more consciously organized and managed, if it wanted to avoid the risk of being taken over by the economists. The community of organizational researchers was therefore urged to invest authority in a small well-published elite that through their control of journals, positions, resources, etc. should be able to enforce a consensus, thereby increasing the reputation, power and financial support of the profession. However, Pfeffer did not explain in any details how this could be accomplished and towards which paradigm the field should converge. Though Pfeffer was impressed by the way economics as a field had been able to obtain consensus and get plenty of external funding and support, he was very critical towards having the rational choice paradigm as **the** mainstream tradition in organizational research, considering its lack of empirical success.

It was this case for a strategy of unification that Canella & Praetzodd (1994) and Van Maanen (1995a & b) all set out to debunk. Since knowledge is socially constructed, according to Canella & Praetzodd, organization researchers will be unable to make unambiguous claims on some absolute truth. Trying to enforce a consensus upon the community of organization researchers as argued by Pfeffer will be counterproductive since it leads to a stagnation rather than scientific progress in the field. While both Canella & Praetzod had followed Pfeffer in taking a formal organizational approach to the advance of

the field, Van Maanen focussed much more on the rhetoric aspect of organizational research and how the development of good ideas can be facilitated. He argued that Pfeffer's view of organization studies were "insufferable smug; pious and orthodox; philosophically indefensible; extraordinary naïve as to how science actually works; theoretically foolish, vain and autocratic" (1995: 133). In opposition to Pfeffer, but like Canella & Praetzod, Van Maanen was confident that theoretical pluralism would facilitate the growth of knowledge in the field. Consequently, he was interested in how to increase tolerance between different approaches in order to improve scholarly exchanges and understanding within the field.

It is against the background of these very different descriptions and appraisals of how organization study as a field function that this paper will explore the relationship between theoretical pluralism, scientific progress and the social structure of the scientific community within organization study. Ever since the "fall" of the contingency program in the mid to late 1970s and the proliferation of new research programs in the late 1970s and the 1980s, the relationship between pluralism and scientific progress has been a central concern within the community of organization researchers. In accordance with the "conventional wisdom" of philosophy of science, many organization researchers have taken the position that the increase of theoretical pluralism in organization studies since the mid-1970s was positive, because it facilitated faster growth of knowledge in the field. Lately, however, an increasing number of researchers have raised warnings, questioning the "conventional wisdom" that more pluralism should lead to faster growth of knowledge. These researchers argue that organization studies may (have) become too fragmented thereby inhibiting, rather than speeding up, scientific progress.

### **On research programs, scientific progress and theoretical pluralism: Some definitions.**

Osigweh (1989) has suggested that there is a need for greater precision in concept definition. This seems especially true when rather broad concepts such as research program, scientific progress and theoretical pluralism are debated. Take, for instance, the thesis under debate in this paper that an increase of theoretical pluralism will lead to faster scientific progress. For many researchers, an increase of pluralism is often simply taken as evidence of scientific progress. Consequently, by not carefully distinguishing between the two concepts, Popper's thesis is hereby turned into a tautology. We may avoid this by carefully defining each concept independently. Let us start with the concept of "research program".

Using Imre Lakatos' (1970) Methodology of Scientific Research Programs (MSRP), a research program (such as contingency theory, population ecology, new institutionalism, transaction cost economics, etc.) may be defined as a series of theories  $T_1, T_2, \dots, T_n$ , that all have a "family resemblance". According to Lakatos, there are two reasons for such a family resemblance to exist. First, each of the theories within a research program builds on the same "**hard core**" propositions (H) that is regarded as non-falsifiable. By gradually exchanging the auxiliary hypotheses in the **protective belt** of the program (B1 is replaced with B2 and B2 is later replaced with B3, etc.), a series of theories  $T_1, T_2, T_3, \dots, T_n$ , is constructed in the following way:  $T_1 = H \ \& \ B_1$ ,  $T_2 = H \ \& \ B_2$ , etc. In the case of the contingency research program, the hard core is the 'fitness', 'efficiency' or the 'alignment' thesis, while the protective belt consists of the hypotheses describing different types of

organizational structures and different types of contingency variables. Second, when researchers construct new theories within a research program by changing some of the auxiliary hypotheses in the protective belt, they rely on the **positive** and **negative heuristic**. That is, they rely on a set of rules defining how problems should or should not be solved for them to be accepted within the program. In the contingency tradition, for instance, the heuristic consists of positive and negative advises on how to develop the protective belt, thereby formulating testable versions of the program.

So far, we have defined what a research program (such as the contingency research program) is and we have described its internal theoretical development or what Kuhn (1970) calls the normal science of such a research program (cf. Lex Donaldson, 1996a). However, not all theoretical developments have this piecemeal character, where researchers try to solve new problems by taking the 'hard core' and the 'positive' and 'negative heuristic' as given and only make marginal changes in the protective belt. In some cases, researchers do also question the most basic assumptions of their research program and the 'standard' way of solving problems, i.e. they question the hard core and the positive and negative heuristic. This happened in organizational studies during the late 1970s and early 1980s when the contingency research program was partly replaced by newer research programs such as population ecology, organizational economics, new institutionalism and resource dependency theory. In this case, both the fitness assumption (the hard core) and the method of solving problems in the contingency program in terms of the variance approach (the positive and negative heuristic) came under attack and were replaced by new hard core propositions and new heuristic rules. While Lakatos describes this kind of development as a 'shift' between research programs,

Kuhn talks about either a 'shift' of paradigm or a 'scientific revolution' in order to emphasize the dramatic character of such a change.

The second concept that is used extensively in this paper and which is in need of a careful definition is the concept of 'scientific progress'. A clarification of this concept can fruitfully take its point of departure in the distinction between the two types of research development defined above. On one hand the relatively conservative shift between two theories T1 and T2 within the same research program and on the other hand a much more revolutionary shift between two research programs RP1 and RP2.

In this paper 'scientific progress' will be defined as the establishment of a 'correspondence relationship' between two theories or two research programs. Popper has given the following intuitive understanding of this concept: "I suggest that whenever in the empirical sciences a new theory of a higher level of universality successfully explains 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 progress can also be interpreted as a dialectical view of scientific development. Scientific problems emerge from tensions, contradictions or anomalies either within a single theory or between two or more theories. According to the correspondence view of scientific progress, tensions, contradictions or paradoxes emerge because we are trying to use a theory T1 to solve problems that are in fact lying outside T1's domain of application D1. In order to remove the

contradiction or tension in a theory T1, we may try to construct a new and more “general” theory T2 that has a wider domain of application D2. A scientific progress will consist in a new theory T2 that “corrects” an older theory T1 by first clarifying its limited domain of application D1 and second by making new predictions (or explanations) outside this domain of application (D2 – D1). We can express this by saying that  $T2 \rightarrow T1$  in D1, while  $T2 \rightarrow \sim T1$  in D2 – D1. That is, by constructing a new theory T2, we learn more about the limited domain of T1 because we are able to explain why the old theory was falsified or couldn’t solve a problem outside its domain D1. In summary, we say that we have established a correspondence relationship between T1 and T2 when:

$$D1 \subset D2, V1 \subset V2, T2 \Rightarrow T1 \text{ in } D1 \quad \& \quad T2 \Rightarrow \neg T1 \text{ in } D2 - D1$$

In the definition of what a research program is, we have distinguished between two types of theoretical developments: a shift between two theories within the same research program (normal science) and a shift between two research programs (a 'scientific revolution'). Our definition of the concept of 'scientific progress' in terms of a relationship of (homogenous) correspondence is mainly linked to the first of these types of theoretical development, i.e. a shift between two theories within the same research program. This is due to the fact that we have been talking about relationships of *homogenous* correspondence where the two theories are basically using the same vocabularies. However, we can extend the relationship of correspondence to also include *heterogeneous* relationships, i.e. relationships between two theories or research programs that are using different vocabularies V1 and V2 like for instance, when researchers belong to 2 different research programs and build on different

hard core hypotheses, using different heuristics to solve the same problems. A relationship of heterogeneous correspondence between two research programs RP1 and RP2 may be defined in the following way:  $D1 \subset D2$ ,  $V1 \Rightarrow V2$ ,  $RP2 \Rightarrow T1$  in  $D1$  and  $RP2 \Rightarrow \neg RP1$  in  $D2 - D1$ . In this case we need to be able to "translate" the vocabulary  $V1$  of the first research program to the vocabulary  $V2$  of the second research program (otherwise this definition is the same as a relationship of homogeneous correspondence).

The third concept that has a central role in this paper is the concept of **pluralism**. Like so many other popular concepts in organization science, its meaning is often not very clear. To start with, we may note that the notion of pluralism can refer to many different things such as ontological pluralism, methodological pluralism, theoretical pluralism, methodical pluralism, etc. or any combination of these (cf. U Maki, 1997). This paper will mainly be concerned with **theoretical** pluralism.

An argument for theoretical pluralism will have to include an explanation for why it is desirable to have a 'plurality' of research programs. One such justification is that a plurality of research programs will promote scientific progress within the field. Another justification for theoretical pluralism is the argument that since any research program only highlights one or a few aspects of reality, it will be necessary to have a 'plurality' of research programs if our goal is to get a reasonably adequate picture of the complex reality. Researchers that use this argument are often referring to the fable about 'The Blind Man and the Elephant' (cf. Mintzberg et al. 1998). This second argument for theoretical pluralism is often used in relation to applied research rather than theory development. Consequently, it may be argued that this literature does not contribute to answer the main question of this paper.

Since we will primarily be investigating the first of these two arguments for theoretical pluralism, let us shortly explore the modern history of this argument. One of the first philosopher of science to argue in favor of 'theoretical pluralism' was Karl Popper (1945) in his "The Open Society and Its Enemies". Like later falsificationists such as Hans Albert, Imre Lakatos, John Watkins and the early contributions of Paul Feyerabend, Popper argued that the more 'open' a scientific field is towards new 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 maturer 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). This argument very much resembles the "infant industry"-argument that recommends 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.

The "valid domain" of the statement "that the more pluralistic a field becomes the more competition there will be, and the better will the chances be for a scientific break through", has never been clearly determined. Popper (1945) who was the first philosopher of science to propose this thesis had primarily been interested in physics and other fields with a mono-

paradigmatic structure. Due to the lack of variability characterizing the type of fields studied, these philosophers of science never came to question the validity of this thesis. For instance, in less mature and more applied types of scientific fields such as management studies, engineering, etc., the validity of this relationship seems highly questionable. However, before being able to say anything of the restricted domain of the relationship between theoretical pluralism and scientific progress, we need a framework that can inform us about variation between the different scientific fields. The point of departure of this paper is the comparative framework of Richard Whitley that studies how different scientific fields are organized as reputational systems. This framework will be used to describe the recent evolution of organization studies.

### **The Structure of Organization Science as an Intellectual Field since 1960: Partitioned Bureaucracy, Polycentric Oligarchy or Fragmented Adhocracy?**

According to Richard Whitley (1984a & 1984b), it is possible to identify some of the most important and distinct features of scientific disciplines by analyzing them as **reputational organizations**. In this type of organizations, the members obtain their position in the hierarchy by making contributions to the knowledge structure of their field. The more a scientific community values the contributions of a researcher, the higher return in terms of reputation he or she will get. However, fields that are structured on the basis of their member's reputation may have very different structures. Whitley (1984) argues that it is possible to identify very different modes of how scientific fields are organized based on the

following two contingency variables: 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 its external audiences) and the less interdependency there will be. Conversely, the more basic a science is, the more will researchers have to rely on each other for obtaining their reputation. The **degree of task uncertainty** refers to the degree of uncertainty a researcher may face 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. 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 fit into the background knowledge of the field. If the background knowledge is well structured, which is the case of a mono paradigmatic field, the task uncertainty of an individual researcher will be low. Whitley (1984a) distinguishes between two different aspects of task uncertainties, **technical** and **strategic**. Technical task uncertainty refers to the degree of unpredictability and variability existing in a field with regard to the methods and procedures that is accepted to solve empirical problems. If there exist 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, the degree of technical task uncertainty is low. Task uncertainty does not only have a technical aspect, but also involves a strategic (theoretical) aspect. In this case, the researchers face a different

kind of uncertainty regarding 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 are confronted with many different problems, the relevance and importance of which will be appraised very differently by different groups in the field.

According to Whitley, variations in these two contingency variables make it possible to distinguish between at least seven different configurations of scientific field's organizations. These structures include the following types with examples listed in brackets. 1) Fragmented adhocracy (management studies) 2) Polycentric oligarchy (continental philosophy and classical sociology) 3) Partitioned bureaucracy (Anglo-Saxon economics) 4) Professional adhocracy (Biomedicine, engineering) 5) Polycentric Profession (experimental physiology) 6) Technological integrated bureaucracy (Chemistry) and 7) Conceptually integrated bureaucracy (Physics).

Focussing primarily on organization studies, the discussion will be limited to the three configurations found in the social sciences. These are **the Partitioned Bureaucracy, the Polycentric Oligarchy and the Fragmented Adhocracy**. An important thesis of this article is that organization studies, since the 1960s, has adopted all these three organizational configurations or at least configurations that are close to them, consecutively. By first giving a description of each of these configurations and second showing how organization studies at certain point in time or in different areas may fit into these, the stage will be set for the next and main section of this paper. In this section, it will be analyzed how the structure of

organization studies may influence the relationship between theoretical pluralism and scientific progress.

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In general, social sciences such as sociology, management studies, anthropology, political science, etc. are less dominated by a single paradigm than the natural sciences are. This implies that we should expect to find less consensus and therefore more task uncertainty in the social sciences than in the natural sciences. The only social science that has diverged from this pattern is economics which, for a long period, has been dominated by a single 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, 1984a) the reputational configuration of economics may be characterized as a **partitioned bureaucracy**.

As a partitioned bureaucracy economics consists of a core with pure and abstract theorizing (within the optimization paradigm) and a number of peripheral sub fields of applied research (industrial organization, labour economics, international economics, etc.). 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 modeling 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 applied research in the peripheral 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 optimization paradigm have been “immunized” from potential “empirical falsifications” arising in the applied sub fields.

Though economics is often portrayed (mostly by other social scientists) as having a completely unitary structure, the field includes several heterodox traditions (behavioralism, transaction cost economics, evolutionary theory, institutionalism, etc.) that during the past 20 or 30 years have influenced mainstream economics in profound ways. Take the case of the transaction cost approach of Oliver Williamson that is of interest to organization researchers as well. In the 1970s, when Williamson turned Coase’s 1937-article into a research program, most mainstream economists for two reasons viewed the transaction cost approach as heterodox. First, Williamson didn’t follow the central maxim of not opening up the black box of the firm in orthodox theory. Instead he viewed the firm itself as an important economic institution that was in need of explanation. Second, Williamson based his research program on the hard core assumption that decision-makers are boundedly rational rather than perfect maximizers. While neither the principle of not opening the black box of the firm nor the principle of bounded rationality were accepted by orthodox theory in the 1970s, both of the principles in transaction cost economics has been adopted by the modern orthodoxy of the 1990s and 2000s.

How did the field of organization study evolve over time? During a relatively short period from the late 1950s to the mid-1970s, a single paradigm has managed to obtain a (near) dominant position within organization studies. By combining an open system view and a structural functionalist frame of explanation, contingency theorists created a paradigm for studying organizations that, for almost fifteen years, united and facilitated coordination within this very young field of study. In fact, the period from the late 50s to the middle of the 70s is the only period during its short history where organization studies has had a hierarchical form of reputation organization. Or as stated by Lex Donaldson: "The normal science that has been pursued within the contingency paradigm is probably the largest single normal science research stream in the study of organizational structure to date" (1996b:58). However, there are some interesting differences to observe between the way economics and organization studies were organized as fields due to differences between their mainstream paradigms. While the optimization paradigm offered economic theorists a "coherent" way of doing highly abstract theoretical work, structural contingency theory was more a program for standardizing empirical research and testing empirical structure-contingency relationships than for solving theoretical problems. While the optimization paradigm therefore reduced the *strategic* task uncertainty of economics, structural contingency theory mainly reduced the *technical* task uncertainty of organization studies. Unlike economics, organization studies never managed to separate the theoretical puzzle solving from empirical research by fully adopting a *partitioned* bureaucracy. This may be one of the reasons why economics has experienced a more stable intellectual structure than organization studies over time.

In the early 70s, the contingency research program came under increasing criticism both internally and externally. The internal criticism was mainly directed towards the inconsistencies and ambiguities in many empirical studies of contingency-structure relationships such as, for instance, the negative relationship between size and administrative intensity (J. Kimberly, 1976 for a review). Indirectly these criticisms were directed towards the positive heuristics of the program that had almost exclusively relied upon a variance approach using cross-sectional data to formulate and test empirical regularities. The external criticism was directed towards the theoretical and philosophical underpinnings of the hard core assumptions of the contingency approach. It included a critique of its functionalist and deterministic framework that excluded the modeling of strategic choices, power relationships, processes of social change, etc. in organizations.

In the later part of the 1970s, organization studies in the US experienced a proliferation of new theories such as transaction cost economics (Williamson, 1975), population ecology (Hannan & Freeman, 1977), institutional theory (Meyer & Rowan, 1977) resource dependency theory (Pfeffer and Salancik, 1978), etc. As a consequence, the intellectual structure of "organization studies changed," according to Michael Reed, "from orthodox consensus to pluralistic diversity" (1992:248). In the comparative framework of Whitley one may interpret this change as a transition from a bureaucratic type of organization during the hegemony of the structural contingency program to a **polycentric oligarchy**.

A **polycentric oligarchy** is characterized by a high degree of interdependency combined with a high degree of technical and strategic task uncertainty. This type of structure in a field

typically 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 centers. In organization studies these centers were formed around the main theories or research programs that emerged in the late 1970s such as population ecology, transaction cost economics, institutional theory, resource dependency theory, etc. Even though these new theories were all introduced during a period of only three years and organization studies became a fragmented field the innovation rate was subsequently dramatically reduced. During the 1980s no major innovative theories were introduced in the field. However, several of the theories introduced in the mid-70s such as population ecology, transaction cost economics and to some extent institutional theory were turned into ongoing research programs. Later during the 1990s several attempts were made to integrate some of the programs like for instance population ecology and institutional theory (Baum and Oliver, 1991, 1992). Within each of the research centers 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 centers, but rather an intense competition in order to gain control over the field as a whole. The field therefore became balkanized into a set of more or less autonomous centers, each pursuing their own research agenda, with minimal interaction and communication. According to Whitley, examples of polycentric oligarchy include continental philosophy, continental

sociology and organization studies in the period after 1975 in the US could be added as a third example.

James G. March's description of how organization studies is structured is very much aligned to the polycentric oligarchy concept of Whitley: "As the field has grown and elaborated new perspectives, it has continually been threatened with becoming not so much a new integrated semidiscipline as a set of independent, self-congratulatory cultures of comprehension. This is evident with five or the more lively subfields [research programs] of contemporary studies of organizations.... Although these subfields [research programs] have been particularly successful in augmenting our understanding of organizations.....they have exhibited persistent symptoms of isolation, engaging in intermittent internecine worldview cleansing. In the name of technical purity and claims of universality, energized subfields [research programs] have tended to seal themselves off, each seemingly eager to close further the minds of the already converted, without opening the minds of others. There is, to be sure, a certain grim necessity in the process. Exploiting interesting ideas often thrives on commitment more than thoughtfulness, narrowness more than breath, cohesiveness more than openness"(1996:280, my additions in brackets). As the structure of polycentric oligarchy gradually developed, it both reinforced and was reinforced by the diffusion of the "uniquess value" (Mone & Mckinley, 1993). This implied that the separation of the field in different centers was reinforced, making work across the boundaries of the different centers (research programs) less likely. And the insulation of the different theoretical perspectives was reinforced by a rather uncritical acceptance of Kuhn's thesis of incommensurability among the majority of organizational researchers (Andreas Georg Scherer, 1998).

The shift from a bureaucratic form of intellectual organization in organization studies to a polycentric oligarchy was partly due to the rapid growth of the field during the 1970s itself triggered by the expansion of education in management studies. The growth in positions and resources enabled new approaches to become entrenched in the reputational organizations thereby undermining the hegemony of the contingency research program. With the break up of contingency theory and the proliferation of new programs in the post 1975 period, the degree of strategic uncertainty and the degree of technical task uncertainty increased. For the individual organization researcher, prioritization of problems to solve and which empirical method to use was no longer as clear as it was during the hegemony of the structural contingency theory.

While the development of organization studies in the US may be described as a shift from a bureaucratic structure to a structure of polycentric oligarchy, the situation in Europe was rather different. While US organization researchers had constructed a set of new theories or research programs as replacements for the contingency program, European organization researchers reacted mainly by rejecting/replacing the underlying positivistic methodology in the structural contingency program with other methodologies and philosophies. Important contributions were David Silverman (1971), Burrell & Morgan (1979), Morgan (1986) and contributions within the postmodern movement (cf. Alvesson & Deetz, 1996). Much of European organization research tended to operate on a meta-theoretical level rather than on a theoretical level by discussing the ontology, epistemology and methodology of research rather than constructing new theories and research programs as their American colleagues did

in the late 1970s. The close relationship with philosophy implied, however, that European organization researchers were confronted with even more strategic and technical task uncertainty than their American colleagues. Or as Clegg, Hardy and Nord state in their introduction to their "Handbook of Organization Studies": "Gone is the certainty, if it ever existed, about what organizations are; gone, too is the certainty about how they should be studied, the place of the researcher, the role of methodology, the nature of theory. Defining organization studies today is by no means an easy task"(1996:3). That is, organization studies in Europe may best be described as being as a fragmented adhocracy.

The **fragmented adhocracy** is characterized by a low degree of interdependency between researchers, which implies a rather "loose" or flat research organization. Since the researchers are facing very few restrictions in this type of organizational configuration regarding the choice of theoretical framework and the choice of research method, the degree of technical and strategic task uncertainty is very high. This implies a relatively **fragmented** knowledge structure and the existence 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. Management studies and contemporary American sociology are mentioned by Whitley (1984a) as examples of this type of reputational organizational form.

**Towards a Process Perspective on the Organization of Scientific Fields: Avoiding the "Specialization" and the "Fragmentation" trap.**

Richard Whitley's (1984a & b) comparative study of how different scientific fields are organized builds on a static type of analysis which is adequate for understanding structural questions like: How can we describe the structure of a field at a specific time? Why are certain structures observed in specific environments? His framework, however, is less adequate for answering process-oriented questions such as: How did a certain configuration in a scientific field emerge? What forces drive the transformation of a scientific field from one configuration to another configuration? What kinds of processes stabilize or destabilize a configuration?

The strong structural bias of Whitley's framework also makes it less adequate to answer the main (process-oriented) questions of the present paper: To what extent does the configuration of a scientific field influence the relationship between theoretical pluralism and scientific progress? Is the relationship between increased theoretical pluralism and scientific progress, as implicitly argued by many philosophers of science, valid across all types of scientific fields independently of their social structure? Or is this relationship only valid for fields with some types of configurations (for instance a partitioned bureaucracy) but not for fields with other types of configurations (for instance a fragmented adhocracy) where more pluralism tend to lead to stagnation rather than scientific progress?

To answer these questions, we need a framework that not only describes the structural characteristics of different scientific fields, but also identifies the underlying "processes" and "mechanisms" that operate within the different configurations as well as determine the major

dilemmas a field is confronted with. By supplementing the structuralist and comparative static framework of Whitley with a process-oriented account, it will be possible to explain how an increase in theoretical pluralism may lead to scientific progress within some configurations, but have the opposite effect within other types of configurations.

A central thesis in this process-approach to the organization of scientific fields is that all disciplines are struggling in order to find a balance between exploitation, i.e. expanding an existing research program (normal science), and exploration, i.e. searching for new research programs (revolutionary science). Or stated in a slightly different way, that all scientific fields are confronted with finding an optimal trade-off between short run and long run activities or between continuity and change (cf. McKinley, Mone & Moon, 1999). Most scientific fields are in fact experiencing one of the following two imbalances.

The first imbalance exists when there is too much emphasis on exploiting an already existing research program and too little emphasis on exploring new theories or research programs. In this case, the researchers in the field tend to value short-term more than long-term activities, thereby reducing the adaptability of the field to new situations. The second imbalance exists when there is too much emphasis on exploring new theories in order to establish new research programs and too little emphasis on the exploitation of already existing research programs. In this case, the field is giving too much emphasis to the long-term activities of exploration compared to short-term activities of exploitation.

Finding a balance between exploitation and exploration has also been discussed by the philosopher of science Thomas Kuhn as finding an "essential tension" between tradition and novelty. New and path-breaking research will, according to Thomas Kuhn (1977), always result from a tension between working within the framework of an existing paradigm while at the same time trying to transcend this paradigm in order to overcome its major weaknesses. Paradigms will always contain the "seeds" of their own destruction since scientific revolution would be unthinkable without long periods of normal science that identified the anomalies that triggered the shift from an old to a new paradigm.

Besides having problems with securing a balance between exploiting existing research programs and searching for new research programs, scientific fields are exposed to traps that tend to drive a field into either a self-reinforcing spiral of elaborating existing programs or into a self-reinforcing spiral of search for new research programs. In both cases, the possibilities of keeping the optimal balance between extending existing research programs versus searching for new programs will be upset.

The first trap may be called a "*specialization trap*". It will be present when normal science drives out revolutionary science and the activity of elaborating and extending an existing research program gradually comes to dominate the search for new research programs. As researchers develop better and better skills in using an existing research program and its problem solving heuristic, the existing research program will be even more used by them to solve new problems, thus further increasing the strength of the research program's heuristic and the opportunity costs of searching for a new research program. This specialization trap

emerges because the exploitation of already existing research programs gives a faster and safer return than the experimentation with completely new and uncertain research programs. The implications of this trap in the long run is a scarcity of exploratory activities that undermines the flexibility of the field by reducing its ability to adapt to new and unpredictable situations. In organization theory a tendency towards such a “specialization trap” existed during the hegemony of the contingency research program with its strong adherence to a “variance approach” heuristic. With the elaboration of this program, organization researchers developed more and more refined statistical-empirical methods that made it more and more attractive to refine this heuristic, but less and less attractive to switch to alternative heuristics such as a “process approach” (cf. Huber & Van de Ven, 1995). However, being locked into such a self-reinforcing process of specialization seem to have made the field of organization studies less prepared to switch to studies of organization change when such studies came in high demand due to a more turbulent and changing environment after 1975. It is therefore not surprising that several of the new research program emerging during the late 1970s had the ambition of viewing organizations from a longitudinal rather than a cross-sectional perspective and to develop a corresponding “process approach” heuristic. Among the research programs emerging in the late 1970s, population ecology with its “inertia”-assumption seems to have been the most aware of this goal (cf. Freeman and Hannan, 1975).

The second trap may be called a “*fragmentation*” trap. It will be present when revolutionary science drives out normal science and the search for new research programs comes to dominate the elaboration of existing research programs. There are several reasons for 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 it can be successfully exploited by normal scientists. Even the most successful research programs will therefore perform rather badly to start with. Because of a lack of persistency in the scientific community many research programs may therefore never be investigated well enough, before new research programs have been proposed and have replaced them. The "true" potential of such new research programs will therefore never be discovered. In fact, no mechanisms seem to be in place to secure that progressive programs survive degenerating programs. And when the process that drives revolutionary science to replace normal science takes on a self-reinforcing character, the field ends up in the "fragmentation" trap" where one theory or research program is just replaced by the next theory or research program with minimal accumulation of knowledge.

When no less than four new research programs were introduced into organization studies in the late 1970s to replace the structural contingency program in less than 3 years, W.R.Scott (1993) issued a warning that may be interpreted as if organization studies was in danger of falling into a fragmentation trap: "These diverse and conflicting paradigms came tumbling into the placid arena of organizational studies in rapid succession – too rapidly to be properly evaluated or reconciled" (1993:63). Not being able to sort out whether the different research programs were progressive or not and what theoretical relationships existed between them threatened to lead to a fragmentation trap that in the long run would make scientific progress in the field less likely. Observe, however, that it is not the absolute number of research

programs but the rate at which new programs are introduced that determines whether a field ends up in a fragmentation trap or not. Since the 1980s may be described as a period of consolidation for US organization studies in the sense that no new programs were introduced and the field escaped falling into a fragmentation trap. Instead, during the 1980s the existing programs were all trying to build a body of empirical data to support their arguments and strengthen their positive heuristics (cf. Van de Ven, 1997).

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INSERT FIGURE 2 HERE

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In their attempt to strike a balance between elaborating existing research programs and the search for new research programs, scientific fields may be described as standing on a knife-edge trying to avoid getting locked into either a self-reinforcing "specialization trap" or a self-reinforcing "fragmentation" trap. However, there exist very complex interactions between activities of exploiting existing research programs and activities of searching for new theories/research programs that tend to undermine any kind of balance exist between them. Elaborating existing research programs requires the search for new theories and research programs in order to contribute to a scientific field's cumulative growth of knowledge. At the same time each interferes with the other. Elaborating an existing research program tends to undermine revolutionary science by discouraging attempts of finding new research programs and problem solving heuristics that are essential for the long-term survival of a field. Researchers in the field therefore 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 in failing to stick to one (underdeveloped and currently degenerating) program long enough to determine its "true" problem solving capacity.

In a similar fashion, revolutionary science undermines normal science. Efforts to promote revolutionary science encourage impatience with new research programs and problem solving heuristics. New research programs are therefore likely to be abandoned before enough time has been devoted to developing the strength of their heuristic, thereby making them progressive. The impatience of revolutionary science results in unelaborated discoveries and a fragmented knowledge structure. As a result of the ways in which normal science and revolutionary science tend to extinguish each other, most scientific fields will be struggling to maintain a healthy balance between the two.

**Unification, pluralism and avoiding the specialization trap and the fragmentation trap  
in organization studies.**

How does this "process" approach with its discussion of avoiding both the "specialization trap" and the "fragmentation trap" fit into the structural approach of Richard Whitley and his discussion of different organizational configurations in scientific fields? There seems to be a very simple answer to this question. The general rule is: If the field is very hierarchical in its research 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 a **specialization** trap. If the field, on the other hand, has a more flat organizational configuration such as the **fragmented adhocracy** (high degree of task

uncertainty and a low degree of interdependency), the field will typically be struggling to avoid or get out of a **fragmentation** trap. Compared to these two possibilities, fields that are situated in between these two extremes such as the **polycentric oligarchy** will be closer to the ideal of maintaining a healthy balance between elaborating on existing research programs and searching for new research programs.

This clarification may now help us in positioning the process approach vis-à-vis the unification position of Pfeffer on one hand and the pluralist position of Van Maanen on the other hand. From the perspective of the process model, the unification position is a less attractive alternative because it leads to a highly hierarchical structure and a self-reinforcing specialization trap that undermines the field's adaptability to new and unforeseen phenomena. The pluralist position in the form of "letting thousand flowers bloom" may on the other hand lead to a fragmented adhocracy and get caught in a self-reinforcing fragmentation trap. The process model suggests alternatively that we should look for a healthy balance between the exploitation of already existing research programs and the explorations of new theories and programs, thereby avoiding both the specialization trap of the unification strategy and the fragmentation trap of the pluralist strategy. And the intellectual structure that best supports this kind of balance is the polycentric oligarchy.

The conclusion that a polycentric oligarchy is the most suitable structure for promoting scientific progress in organization studies also find some support in the Schumpeter thesis in industrial organization. According to this thesis, neither perfect competition nor monopoly will be the industrial organization that best promote technological progress. While perfect

competition (like the fragmented adhocracy) is too fragmented and monopoly (like the partitioned bureaucracy) is too concentrated, Schumpeter argued that oligopolistic competition (like the polycentric oligarchy) would best promote technological progress by securing an optimal trade-off between static and dynamic efficiency.

To uphold the polycentric oligarchy and secure a reasonable balance between tradition and novelty in organization studies, the research community should try to follow a strategy of creative tension (cf. Poole & Van de Ven, 1989). Such a strategy direct the researchers in the field to exploit any opportunity offered by tensions oppositions and contradictions between the main research programs in order to construct new and encompassing theories or research programs. By not exploiting such tensions and contradictions between the existing research programs to construct new theories, the field risks to fall into a fragmentation trap that will make future progress less likely.

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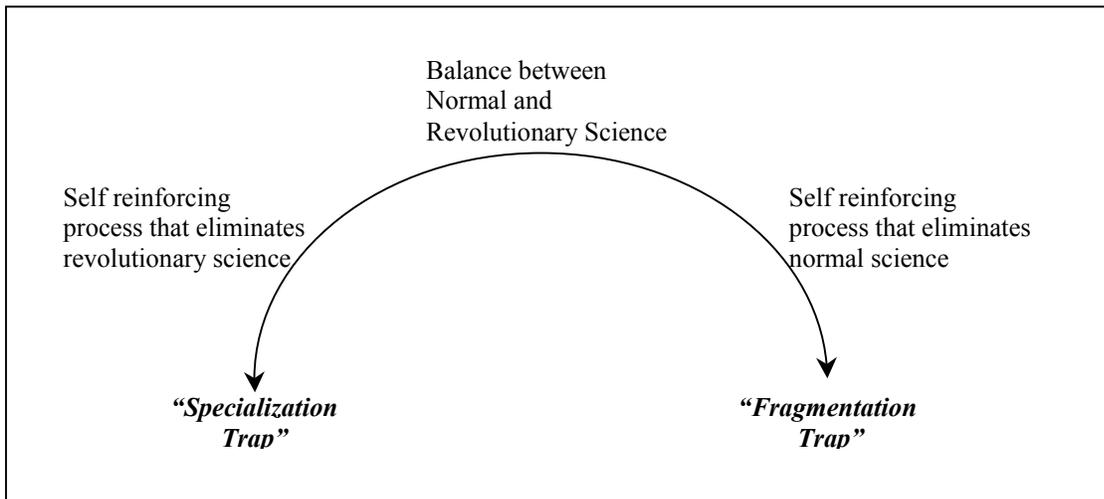
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|   |  | Degree of Interdependency   |                                |
|---|--|-----------------------------|--------------------------------|
|   |  | Low                         | High                           |
| Degree of Strategic Task<br>Uncertainty |  |                             |                                |
| High                                    |  | <i>Fragmented Adhocracy</i> | <i>Polycentric Oligarchy</i>   |
| Low                                     |  | <i>Unstable form</i>        | <i>Partitioned Bureaucracy</i> |

**Figure 1: Reputational Organizations in the Social Sciences**



**Figure 2: The “Specialization” vs. the “Fragmentation” trap.**