Rio´s International Journal on Sciences of Industrial and Systems Engineering and Management
Editor-in-Chief: Dr. Heitor Luiz Murat de Meirelles Quintella, Universidade do Estado do Rio de Janeiro

Paradigms in Management

Matthias Philip Huehn

German University in Cairo

matthias.huehn@guc.edu.eg

ABSTRACT

The paper laments the current confusion in business science with regard to its epistemology. Any scientific discipline needs a firm structural basis, otherwise research is unfocused and flawed. In business science not even the vocabulary is clear: terms like Management and Business Administration mean many things to different people. The paper suggests to replace Burrell and Morgan's matrix of sociological paradigms with a new typology which is really able to guide research and practice alike. Management scholars have argued too long without any sense of direction and managers have as a result become reserved and somewhat cynical toward Management theory.

Epistemology, paradigms in business, theory and practice, management, business science, sociology

1. Introduction

Business science is a wide field encompassing very diverse research programmes and even the usage of terms like business science, management, management science etc. differs from academic community to academic community. What the discipline lacks is a meta-structure, or, to put it provocatively, the discipline needs discipline. This paper will not make the naive and megalomaniac claim to presenting a complete and definitive taxonomy of business science. Rather the purpose of this paper is to start a debate which has been neglected for too long. Over the last 100 years business science has turned from a small formal garden into a jungle so to speak. A major restructuring effort is needed to accommodate the grown scope of that research programme. Thus, this paper wants to make an architectural suggestion how one could use what has grown in the past fifty or so years to create a garden that pleases the eye and has a recognisable structure and theme. The author is also aware that there is strong political resistance from the conservative mainstream at establishing a debate about the epistemology of business science. Many mainstream scholars refuse to subdivide the field at all and thus the four research programmes (or paradigms) suggested in this paper will face strong opposition. However, I maintain, like a formal garden, a scientific field should have some structure and academic inquisitiveness should be able to overcome attempts to quash resistance.

The paper will start with a brief discussion of Burrell/Morgan's matrix and then will suggest a different four-fold taxonomy of research programmes within business science. The two dimensions forming the newly proposed matrix will discussed in some depth, then two of the research programmes (paradigms) will be discussed. The paradigms represent the extrema of a suggested continuum (with the two other paradigms somewhere on the continuum) and are therefore especially important. One of the research programmes – I named it Traditional Business Administration – is the ideology underlying the mainstream in business science. It will be dealt with at first. Then the second extremum, which I have christened Management, will be discussed. Thus, the major aim of the paper is achieved: I would like to propose a somewhat clearer map of business science compared to what exists today in the minds of most students and practitioners so that a robust and fertile division of labour between the two (four) paradigms can be established. The paradigms all have underlying idiosyncratic strengths and weaknesses which make them useful for solving different problems. If the problems for which every research programme is particularly suited could be identified, progress in science and in practice of would be accelerated. Paradigms are assumed to be incommensurable (Kuhn 1962). While I support the view that for different problems different problem-solving methods coming from different paradigms should be applied I am less sceptical than Kuhn about the chances of a meaningful exchange. The paper argues that while incommensurability is desired (and created) by the proponents of the mainstream, because it superficially strengthens epistemological rigour within the paradigm and thus reinforces the ideology, for practice and scientific advancement incommensurability is far less attractive. Any theory should make room for debate and not take it away. Thus, the proposed differentiation aims not at creating hermetically sealed discussions *within* the different paradigms, but at injecting structure and a neutral language into a badly needed inter-paradigmatic debate. At the end I will discuss which concrete fields or problems seem to offer themselves to the two paradigms.

The paper will try to integrate ideas about the structure of the discipline by reviewing the basic business literature. The literature on the epistemology of business science – while highly relevant – will only be included when it seems unavoidable with respect to a particular discussion. This happens both to preserve the clarity of the argument and to allow those

readers who have no formal training in the philosophy of science,[1] but who are nevertheless interested in the topic, to agree or disagree with the arguments.

The reader will quickly realise that the author is somewhat biased towards the underdog of the two paradigms, Management. Having forewarned the readers I will nevertheless try to present the arguments as fairly as is possible without becoming boring.

2. Paradigms in Sociology

Many years ago two young radicals suggested a taxonomy for the social sciences. Since most business science scholars – except in the German-speaking world – tend to define themselves as social scientists of sorts, Gibson Burrell and Gareth Morgan's matrix was an instant success. We will take their matrix, which has been used in countless doctoral theses and academic articles, as a starting point for this paper and move from a criticism of their classification to suggesting a new matrix which in our view meets the requirements of business science better.

When Gibson Burrell and Gareth Morgan published their classification of social sciences in 1979 it was quickly accepted as the definitive segmentation of the different research programmes inside business sciences. Few textbooks and dissertations managed without taking recourse to their matrix with the four famous sociological paradigms. Deetz (1996: 191) talks about the "almost hegemonic character" of the grid. Taking a fresh look at the model one might ask the rather heretic question, what its contribution to the understanding of the structure of the discipline is. In business science the two maybe most important criteria should be what a theory has done for practitioners and what it has done to further academic understanding of practical problems, or, to use Imre Lakatos's (1978) phraseology and put it more generally,

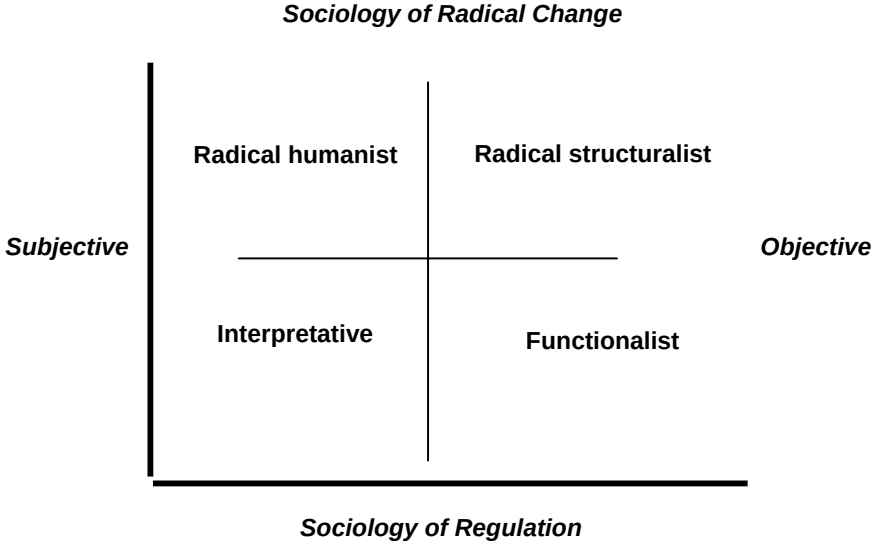

Figure 1: Sociological Paradigms, Burrell/Morgan 1979, p. 22

[1] It is quite astounding how many Doctors of Philosophy have virtually no knowledge or training in the philosophy of science.

whether the theory creates a positive problem shift. The first question is quickly answered and will probably not meet a lot of resistance. The practical value of the matrix is very close to nil. Thus, the huge success of the model must consequently rest on its enormous contribution to the theoretical advancement of business science. I would contend that notion as well for the simple reason that I consider the matrix not really an attempt to epistemologically segment the discipline into research programmes but rather a political positioning device. Deetz (1996: 191) supports this view:

> "*When the grid and discussion were published in 1979, those of us doing alternative work readily embraced it for it gave each of us a kind of asylum. While some of us were uncomfortable with the dimensions and philosophical analysis, we happily accepted the newfound capacity to present ourselves to mainstream critics as doing fundamentally different, but legitimate, kinds of research and began to work on concepts and evaluation criteria within our now produced as different and unitary communities."*

What is more, the mainstream very effectively used the matrix to stop a debate about other subdivisions in business science before it even started. A debate about essentially political issues was neither going anywhere nor would it threaten the mainstream's ideology. Thus, Burrell/Morgan's matrix was a welcome tool to avoid a real debate about the basic axioms and methodological issues and lead a harmless pseudo-debate instead.

The value of the matrix in both dimensions (practical and theoretical) is further greatly diminished by the fact that the vast majority of management scholars and virtually all practitioners can be found in one quadrant, the functionalist paradigm. I would even argue that Gareth Morgan who has become one of the most eminent writers on organisations can now be found in this quadrant. His tremendously successful and influential metaphorical approach to organisational theory, *Images of Organization*, includes scattered bits and pieces of radical-humanist/radical structuralist ideology, but really is placed firmly in the functionalist paradigm.[2] The Deetz quote also shows that in the late 70s the discussion about a segmentation of the business sciences had frustrated many researchers so much, that they simply accepted the first somewhat plausible classification that came out. In my view the situation right now is very similar in as much as there is no meaningful classification of research programmes and there is a growing frustration about that fact. Many researchers are like ships without wind in their sails, they just drift with the aimless current. Thus research itself is aimless. If that would be the case, we would have wasted a whole generation of researchers and this situation should be rectified as soon as possible.

Obviously there is a strong demand for a tool that partitions business science research into meaningful segments which help practitioners, students and management scholars alike. Practitioners, even those who took a business degree, have no overview of the field they studied, indeed they are mostly more confused after getting a business degree than before. Students struggle with different usages of the term 'management' in one and the same book[3] and fail to make connections and see differences between ideas coming from totally different paradigms of business science. The book writers, the scholars themselves, seem to be either

[2] The purpose of the book was to enable managers to make organisations more efficient and effective by allowing them to see an organisation from different metaphorical angles. The hugely successful executive edition is another indication that the book is written from within the functionalist paradigm.
[3] The grand old man of German Banking simply renamed his standard work on banking. Bank Business Administration became Bank-Management, the content stayed the same (Hühn 2000).

without a clue even where they are positioned inside the discipline or they have very backward ideas about epistemology. Positivism for instance, after Popper and his neo-positivist revolution (he called it falsificationism) widely ridiculed as naive, today is the methodological ideology of choice in many research areas and virtually the only accepted method in some (Marketing for instance). Statistics has replaced reasoning and inquisitive science has become superficial data-mining.

3. The Matrix

What I will try to outline in this paper is that there are at least four different research programmes or paradigms producing, and being characterised by, different sets of methods, philosophies and management tools. While the differences between these four segments are manifold two criteria seem to me to be of very great importance. They also have the advantage of being rather clear-cut. The two major differences are the two dimensions of the matrix proposed in this paper. Behind these two dimensions one finds a large number of characteristics which might even have the same importance as the two I have chosen. The paper will talk about these other dimensions in a discussion of the four quadrants. Thus, the two dimensions were chosen because they deliver the most meaningful quadrants, not because they are more important than other differentiating criteria.

The first criterion (and the y axis) is the role scientists see for themselves vis-à-vis the practitioners; the second dimension is whether they see organisations (the objects management scholars are interested in) as being closed or open systems. The open/closed dichotomy also incorporates what is maybe the most important single division between the extrema of the business science continuum: the acceptance or dismissal of the rational actor / economic principle. I will try to explain the two criteria and then proceed to a more detailed discussion especially of the two extrema of the business science continuum, Traditional Business Administration and Management, before at the end of the paper suggesting a division of labour between the four different research programmes which could help focus and therefore accelerate research.

3.1 The First Dimension: The Role of Science and Scientists

Management scholars can be divided into two groups by placing them at the two ends of an ideal-type continuum ranging from descriptive to prescriptive. Warren Bennis and James O'Toole have recently pleaded with American business school deans (and referees and editors) to stop modelling management to physics and accept that management methods are made for management practice not for classrooms and case studies. "Today it is possible to find tenured professors of management who have never set foot inside a real business except as customers." (2005: 101) In the German-speaking world this type of professor is in fact the norm (by law) rather than the exception. No wonder that people who live in a world of clean models and quantifiable hypotheses see themselves as the ones making the rules the practitioners of management have to follow. I spent long afternoons with the founder of one of Europe's top-ranked business schools during which this *grandseigneur* lamented the unwillingness of German professors in particular to talk to executive students because these executives actually talked back and pointed out that there is a difference between theories and reality. Thus, the

view of a large group of management scholars (I suspect it is the majority) see their role as rule makers and the role of the managers to apply these rules without fail. This is quite an interesting logical manoeuvre, which has been called the "theoretic turn", like the linguistic turn, in business science (Hühn, 2007, 284). *Reality* is seen as *theory-dependent*, i.e. theory defines what is real, or more precisely, what is allowed to be real. It is completely normal that rationality[4] – a totally unrealistic *postulate* – is assumed to be a law governing the behaviour of decision-makers (i.e. the objects under study). Management practice is viewed as a strictly controlled laboratory where new theories are tested. Managers are supposed to behave "rationally", otherwise they are bad lab rats. The managers' subordinates also behave rationally, i.e. they are motivated by only one factor: money (as an expression of material gain). F.W. Taylor's flawed and much criticised assumption is the basis for Nobel-prize winning game theory. That the axioms of game theory are completely unrealistic matters little. The most important German business journals (double-blind peer reviewed) regularly publish papers in which the authors explain how managers make decisions with the help of equations which take half a page and are full of logarithms and Greek letters. Manager who make decisions not based on these formulae are bad (non-rational) managers. This of course means that all managers are bad managers and only the theoreticians of management are good. The subsequent de-coupling of theory and practice is the price traditionalists gladly pay.

The other end of the continuum, the descriptive extremum, is made up of professors and consultants who look to practitioners to teach *them*, so that they can then identify patterns and spread these patterns among practitioners. Naturally they also create rules (theories, but rather less model-like and less exact) but they use inductive reasoning – which does not exist according to Carl Popper – instead of deducing a model from another model. Research for the descriptive faction is an iterative process of going into businesses and ruminating on what they have seen, while the prescriptive group relies on the basic theory or axioms (the rational model) to be right and build their theories on these old foundations which when being tested in practice always are refuted.[5] Thus, the difference between the two groups in terms of methodology seems not so big (it is bigger than it looks on first sight as we will show later); it is more a philosophical stance that includes humility and inquisitiveness as prime ingredients as opposed to clinical reasoning and aloofness on the other side.

[4] I contest the existence of rationality in the real world. Rationality is a mere postulate, not a physical state of affairs with a separate existence. The rationale for acting not only differs from person to person but also inside individuals at different times. The assumption of a unified *ratio* for acting based on pseudo-mathematical axioms is the basis for mainstream economics. Economist *postulate* that people act according to one rationality. What basis for a science is a postulate? A physicist who would ask atoms to behave in a certain manner and base a theory on this request would be considered a joke. An economist has a good chance to get the Nobel prize.

[5] Goshal (2005, 83) takes the very standard ultimatum game as an example. A proposer is asked to divide a gift between himself and a responder. If the responder rejects the offered gift, both players end up empty-handed. Since all players are rational, the proposer should offer one cent (or whatever is the smallest unit) and the responder will accept, because she is one cent richer than before. In experiments, that outcome is virtually unheard of. Most frequently a 50:50 split is offered, because lesser offers are considered insulting to the responder. A unified rationality is postulated by game theory, despite it being totally unrealistic.

3.2 The Second Dimension: The Systemic Viewpoint

System theory comes in two very different versions, one of which is rather new and accordingly not widely known. The latter fights an uphill struggle against what psychiatrists call “anchoring”: what one has heard first is true and other things are automatically inferior. When Ludwig von Bertalanffy, Norbert Wiener, Ross Ashby and others developed a new meta-science in the late 1940s they proposed a world made up of open systems interacting with each other in mathematically understandable ways. Later, in the 1960s, Stafford Beer (1972) and Humberto Maturana (1959) and much later Francesco Varela and Maturana (1980) together amongst others proposed that there is a second category of systems which are not open to their environments (their meta- or super-systems) but are *systemically closed*. General systems theory and open systems theory are identical because for the first thirty to forty years *any* system was assumed to be an open system.

Put very simply – with an apologetic nod to the experts – an open system is a system which turns input into output by manipulating (and adapting) internal sub-systems. Ideally the output remains constant or at least within certain parameters. An organisation’s output can be profit; the output of the human body for instance is life. Because the input is changing all the time the open system must adapt its subsystems, its internal structure, to keep the output stable. In order to structure an open system efficiently one has to manipulate its structure from the outside. This manipulation is only possible because the system is open in the sense that an outsider can see and analyse its internal structure. A mere glance is enough to conclude that tools like business process re-engineering, the Balanced Scorecard, decision trees or time based competition are firmly grounded in open system theory. This world-view allows outsiders to analyse a system. i.e. to divide it into its subsystems or parts and, most importantly, place them in a linear chain. This makes organisations manageable, predictable, programmable like machines. Michael Porter’s famous value chain is another prime example of this logic. It depicts the company as a generically structured entity with analysable components working in a predetermined linear sequence. Porter retracted his generic value chain 1996 in favour of an idiosyncratic activity system which is of course a closed system. Porter, who was maybe the most important

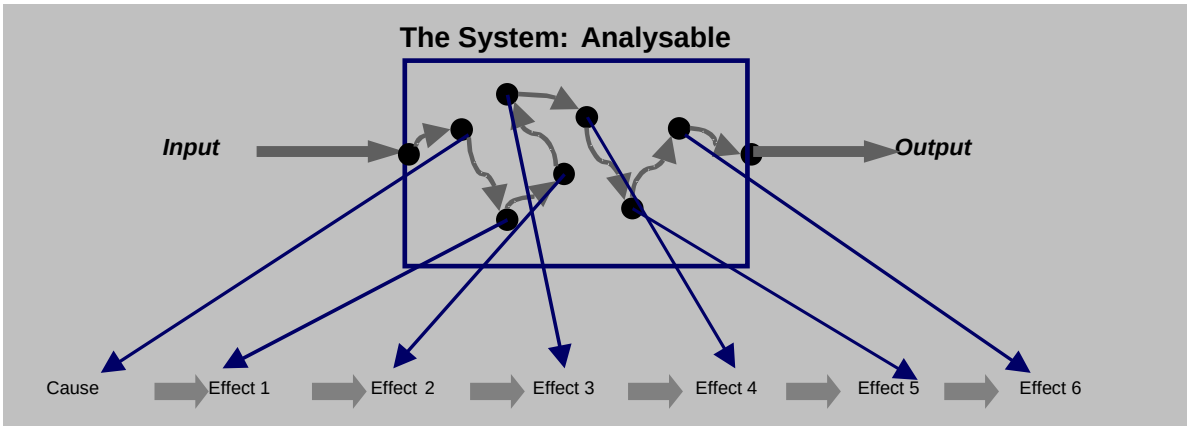

Figure 2: An Open System

proponent of open system style (strategic) management theory, has made a *Gefechtskehrtwende*[6]. Not only has his view of the organisation changed from one of a dead machine to one of a living and learning closed system. He also has changed the direction of influence. In his old thinking the company was embedded in an industry structure (which was governed by 5 forces) and had to find a position in order to survive, while his new model sees the organisation as influencing the environment. The company now is the most influential force, not the environment. A side effect is that this view makes the 5 forces model disintegrate as the market is neither a given nor governed by five independent forces but shaped by the actors in the market, the companies and the consumers. If a market cannot be analysed anymore, it cannot be predicted and thus, strategy stops being a plan and becomes an explorative learning experience. More of that later.

The system is called closed, because it is a black box into which an outsider cannot look and whose logic (which is non-linear) will always remain in the dark and cannot be anticipated with certainty. Secondly, the system is self-referential; it has its own logic and also creates its own reality, which is constantly evolving. The cybernetician uses the word "autopoietic" (Maturana/Varela) to describe a system that creates its own logic, creates its own reality and keeps itself alive. Humans are excellent examples of closed systems. The brain is a closed system which decides according to very individual criteria (self-referential) which environmental inputs are accepted (cognition is an active process), how they are processed and finally how they are interpreted. Every brain has a different microstructure, and even if all brains would have the same structure, simply because the brain is a network and not a chain, the reactions of these structurally identical brains would not be the same, perhaps not even similar. The unique connections among the subsystems (neurons) deliver an idiosyncratic image of the meta-system (the environment) and create the personality which is also unique. Every human being is a closed system with its own personal reality and real personality.[7] If humans were open systems answering to a central consciousness they would not be individuals, but automatons. Thus, humans, in the view of closed systems theory, cannot follow one rationality (principle), because they would lose what makes them human. Therefore, the open-closed dichotomy encompasses the one-rationality/multiple rationality divide in business science.

Figure 3 shows graphically why closed systems are complex: it is no longer possible to comprehend the sub-systems' relationship as a linear logical chain. This is precisely the reason why (cybernetic) management does not think that it is possible to manipulate a system (an organisation) at will. It has its own will, its own unfathomable dynamic. It is also no longer possible to predict the behaviour of a system; to see into the future. Think about yourself: can

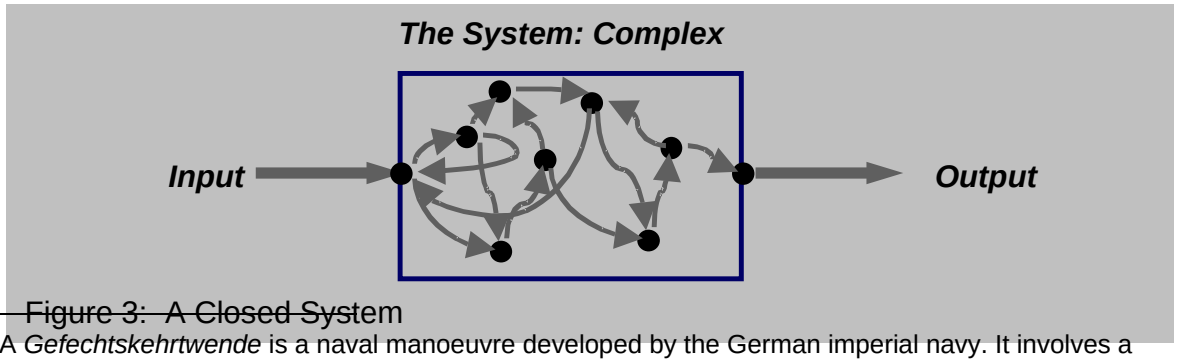

Figure 3: A Closed System

[6] A *Gefechtskehrtwende* is a naval manoeuvre developed by the German imperial navy. It involves a battle group making a 180° turn under full steam without a loss of positional integrity.
[7] That humans are unique in their perception/creation of reality is actually corroborated by the fact that many readers will violently disagree with me on this and many other points.

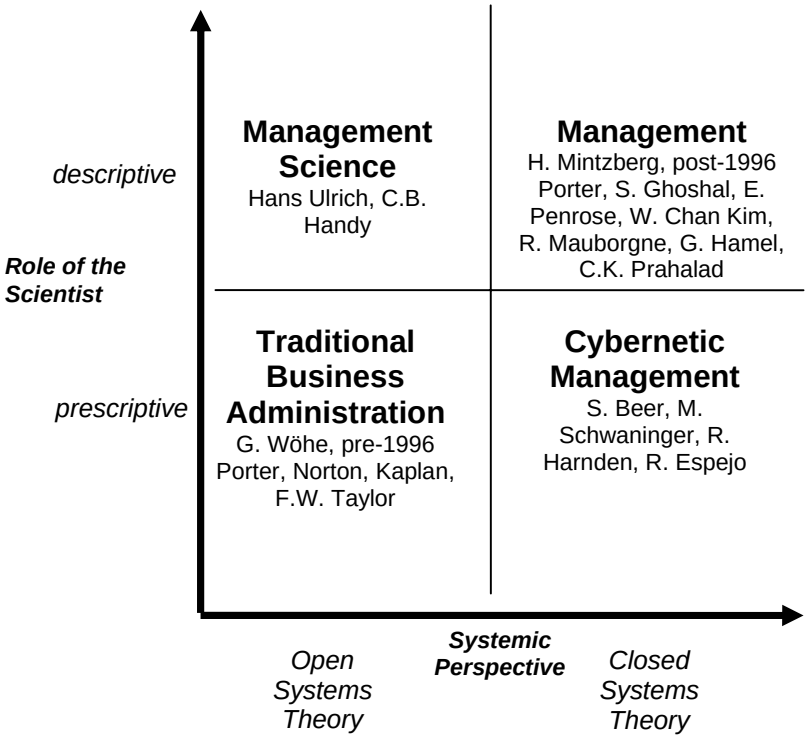

Figure 4: Paradigms in Business Science

you be sure of your reaction to an event? Managing a closed system is not as easy as re-engineering a machine; managers have to be very cautious and can let themselves be guided by looking at successive feedback loops. Again Porter may serve as an example. His *generic value chain* assumed that organisations have a certain structure and that the subsystems are working together in a predetermined way. His *activity system* on the other hand is unique for every company and the outcome of a learning process that should never end.

When the two dimensions described above are combined in a two-by-two matrix the resulting four quadrants can be filled with research programmes. The labels I have chosen are merely for convenience and readers may replace them with their own. I started by criticising the matrix by Burrell/Morgan because its relevance to practitioners and management researchers is too small. Therefore an explanation as to why this matrix is better able to act as guidance for both groups must follow.

4. Quadrant One: Traditional Business Administration

Most research is carried out in this quadrant and by and large students are exclusively taught this version. Professors understand their role as experts who give managers the tools they have to apply one-to-one in reality. The tools are based on linear logics since the assumption is that all organisations function like more or less complicated machines. Frederick Winslow Taylor's mechanistic model is, a hundred years after it was proposed, still very much the state of the art. Norton/Kaplan's Balanced Scorecard sees the organisation as a machine that has been programmed and whose parameters can be monitored and manipulated at will. Michael Porter's ideas about strategy are an even better example. Again, Porter, who was given the chair for

strategy at Harvard Business School with a background in economics, had for the first 15 years a traditional outlook. He took the old, totally mechanistic, SWOT model and did a little window dressing and came out with a slightly modified interpretation of the model. The SWOT model has three major elements/phases: the external analysis, the internal analysis, and lastly the strategic option generation. These three elements Porter restructured and renamed: the 5 forces model replaced the checklists of the *external analysis*, the value chain gave the *internal analysis* a firm structure, and generic strategies replaced the worrisome chaos of the *option generation phase*. Old Porter decoupled analysis even further from reality than was the case with the SWOT model; he made matters worse. Originally, the SWOT model was based on the assumption, that the internal and the external analysis have the same importance. That does not work with an open systems view as the linearity of cause and effect cannot be upheld: the cause must always logically and chronologically *precede* the effect, which would not be the case with a simultaneous analysis. Porter consequently stated quite clearly that the subsystem (the organisation) is dependent on the super system (the industry). Thus, the industry analysis with the 5 forces model delivered the requirements which the organisation had to meet in order to be successful. Maybe Porter's greatest (and most naïve and harmful) contribution to management was that he turned what was the most difficult part of the SWOT model, the generation of strategic options, into the easiest part. He proposed that there really are only two options for any given company. Large companies could choose between Cost Leadership and Differentiation, while small companies choose between Cost Focus and Differentiation Focus. He cut down the choices from an endless number to two. Strategy-making as child's play. Anyone who has ever involved in the strategy process knows that this is at best wishful thinking. It is one of the characteristics of Traditional Business Administration to have very simple models which are based on a number-focused operationalisation. Porter, and before him all his colleagues at Harvard, even assumed that they could calculate the future (the strategy). This is only possible within a strictly linear open system view and rational actors behaving as the models needs them to behave. Chance and entrepreneurial creativity do not exist in such a world view; a preposterous assumption as Hoppe (1997: 56) explains:

> *"First off, if our expectations (predictions) concerning our future actions were indeed as rational as rational expectation theorists believe them to be, this would imply that it would be possible to give an exhaustive classification of all possible actions (just as one could list all possible outcomes of a game as roulette or all possible locations of a physical body in space). For without a complete enumeration of all possible types of actions there can be no knowledge of their relative frequencies. Obviously, no such list of all possible human actions exists, however."*

Porter indeed assumed that the strategist had an exhaustive classification in front of him/her: the two strategic alternatives of his matrix of generic strategies. A page later Hoppe (1997: 57) shows in another manner just how logically absurd the theory of rational expectation is – which is of course the theoretical fundament of Traditional Business Administration:

> *"as far as the assumption that all actors possess identical knowledge is concerned, any proponent of this view is caught in a performative contradiction: his words are belied by the very fact of uttering them […] Rational expectations theorists, too, by virtue of presenting their ideas to the reading public, must obviously assume that the public does not yet know what* they *already know, and hence, that the public's predictions concerning the future course of actions – in contrast to their own predictions – will be systematically flawed until it has successfully absorbed the lesson of rational expectation."*

If Traditional Business Administration starts from flawed premises concerning human actions its domain obviously cannot be what is widely know as management, organising people, strategising, leading. It must be a field where predictability rules and were their prescriptive stance does not bind managers to mechanistic models which systematically deliver bad solutions.

5. Quadrant Four: Management

The upper right quadrant is created when a closed systems view and a descriptive outlook come together and I have chosen to call it Management. One would think that this research programme cannot be older than closed systems theory. However, I will argue that the so-called Human Resources/Relations Movement was the starting point and the writings of Mintzberg (from 1975 till now), whom I personally rate as the first pure Management scholar, were always firmly within the closed systems/descriptive camp.

During the Hawthorne Experiments Taylorist researchers were confounded by the unpredictable actions of the groups under study. To these scientists the organisation was a machine that could be programmed. However, they had to acknowledge that their manipulation of the working conditions (number and length of breaks, lighting, etc.) was in no way connected to the output. So they called in famous psychologist Elton Mayo[8] from Harvard University. Mayo, after 15 years of intensive studies concluded that there were two organisations, one formal and one informal. The more important to him was the informal organisation. The discovery of two intertwined organisational structures meant that simple manipulations could not work anymore as the ability to predict the actions of individuals and the group were gone. The resultant system resembles a closed system. Especially the Bank Wiring Room Experiment (the workers informally agreed on an output among themselves) describes a system that is partly closed vis-à-vis its environment. In effect Mayo's conclusion preceded constructivism by maybe thirty years. Only in the 1980s did the organisational culture movement make the assumption popular that the formal structures are not real, because they are interpreted (through different filters like organisational culture; national culture, professional culture, etc.) by individuals.

When Henry Mintzberg wrote his seminal two works on organisational structure and power (1979, 1983) and thereby founded the Gestalt approach of management, he in effect transferred the closed systems thinking from psychology to the management mainstream. Mintzberg's theory was inclusive: it incorporated all previous ideas about organisations and integrated them in one big field theory. His organisational gestalts (or forms as he later called them) were Weberian ideal types which had their own internal logic, were relatively closed vis-à-vis their environment, and created the conditions which upheld their viability. They were the very autopoietic systems described by Maturana/Varela in their groundbreaking 1980 work "Autopoiesis and Cognition: The Realization of the Living".

Not only Mintzberg operates under the assumption that their role is that of an observer rather than an *a-priori* expert and that systems can and should be designed so that they can be totally controlled like a machine. Many of the foremost management scholars share these convictions. Edgar Schein, Peter Drucker, new (and silent) Michael Porter.

Again, the icon of Traditional style strategy-making, Michael Porter, may serve as a perfect example for the general argument of this paper, namely that a solid theoretical/philosophical

[8] Mayo was not really a psychologist, he was social science expert trained and self-educated in a number of relevant disciplines.

structure/positioning is important for scientific progress. Some major errors in Porter's new theory of strategy could have been avoided easily and this may serve as an example how the absence of a meaningful structure is harming scientific advancement (more examples will given in part 6).

The value of Porter's contribution to strategy rose dramatically once he changed his theoretical stance from an open to a closed system perspective and from a prescriptive to a descriptive stance. In his seminal 1996 paper, *What is Strategy?,* he defines strategy very succinctly:

> *"What is strategy? We can now complete the answer to this question. Strategy is creating fit among a company's activities*." (Porter, 1996, p. 75)

Compare that to his 1980s approach, where strategy-making was the analysis of a company's market and the subsequent positioning of the company in the market. In his whole 1996 paper the market or market analysis are not mentioned. Porter swings from the view that the company (the system) is dependent on the market (the meta-system), to a view where the company is a closed system *creating/influencing* the market. This is a perfect turnabout. What is more, Porter could have easily avoided some major mistakes in his paper, if he would have been aware that he had switched paradigms and not merely changed from one viewpoint to another. His biggest mistake is his attempt to introduce efficiency considerations into what is now a closed system perspective and therefore should look primarily at effectiveness.[9] He suggests that managers should create a strong fit between the individual activities.[10] The highest order of fit is *optimization of effort* (1996, 72). His practical example of what he also calls third order fit is *The Gap*. Of all the company examples in the paper *The Gap* is the only company which has been experiencing (still is) serious problems. Had Porter understood that he had left open system territory, where efficiency considerations take precedence over effectiveness (as is the case in closed system theory), he could have avoided a big mistake. His explanation of different types of fit clashes with his new frame of reference, closed system theory.

In his 1996 paper Porter also moves from a Traditionalist's *prescriptive* stance to a decidedly *descriptive* view. His generic, linear and therefore largely useless model of the organisation, the value chain, is replaced by company-specific Activity Systems.[11] Not anymore do scholars like Porter dictate generic structures, but managers have created idiosyncratic structures and scholars like Porter *discover* them. This may seem like a minor point compared to the open to closed system shift, but it is possibly even more important for the Traditionalist research programme. The Traditionalist paradigm – especially the Harvard school of strategy – is based on creating cooking-recipes for the manager/cook. The managers diligently load the meat-grinder (any theory, for instance the SWOT model or Porter's five forces model) with data and they always get a strategy in the end. Then they monitor the functioning of the organisational machine with the Balanced Scorecard and keep the steamer on the charted course. Porter's

[9] Peter Drucker (1964, 5) states that "effectiveness rather than efficiency is essential in business".
[10] Strategy managers now create fit between internal company-specific activities, whereas in his old approach strategy managers created a fit between the market and a company. That means the central concept of fit is radically different from before and strategising is also not analysing the market and then adapting the company, but only managing internal activities.
[11] Porter is not really used to thinking in complex systems and it shows. The connections in his activitz systems are not complete and he mixes activities with attributes. Closed system management scholars like Markus Schwaninger, Stafford Beer and of course Peter Gomez and others have been using activity systems for decades before Porter discovered them. It is another example how isolated even major scholars are.

new view on strategy does not allow for such a simplistic bridging of the theory-practice divide. And that is extremely brave of Porter because he leaves the tradition of the most prestigious business school in the world and declares their paedagogical ideology (training cooks/managers by letting them apply theoretical cooking recipes on theoretical case studies) bankrupt.

If Porter would try to get familiar with his new theoretical basis, closed system theory and the descriptive view, he would be able to rejoin the strategy debate (he has not published anything significant on corporate strategy since 1996) and would probably be able to get back to the cutting edge.

Take another example from strategic management. In 1990 the core competence concept totally changed that field of study. Gary Hamel and CK Prahalad, in a short paper,[12] published in the Harvard Business Review (it is indicative that the HBR, a non-academic paper regularly gives space to outsiders who become founders of prominent schools of thought) flipped the strategy debate in one year from a Traditionalist to a Management approach. They took one company as an example[13] and completely changed how people thought about strategic problems. According to them core competences are the product of a cross-organisational learning process (core competences are thus knowledge); the market plays no role in the strategy process anymore other than *ex post* defining which company produced the most successful knowledge. Old Porter's positivist open system perspective with dependent and reactive/passive companies is replaced by a fully constructivist closed system view with active companies creating and shaping the meta-system (the market).

In order to keep the argument coherent and for the sake of brevity I would like to leave it at that. The suggested two dimensions – even more so if they are augmented with other dimensions – open up a new view on the general structure of research in business science. One gets a rather good overview of which research programmes are connected in which way and also understands the specific problems they are dealing with. Again, the suggested dimension and the resulting research programmes are not definitive, but I believe they can be a useful tool for practitioners and researchers.

6. A Division of Labour between Management & Traditional Business Administration

In the early 20th century Henri Fayol (1949, book first published in 1916) proposed an astonishingly far-seeing classification of business activities, which could be taken as a rough guide to assign the business activities to the suggested paradigms in business science:

[12] Prahalad and Hamel's Core Competence article is the Harvard Business Review's most often reprinted article ever (Hamel & Heene 1994).
[13] Management is happy to work with anecdotal empirical data, while Traditionalists prefer statistically valid sample sizes. These respective proclivities stem from the paradigms' descriptive/prescriptive outlook. If a researcher has a descriptive outlook it is not important whether one or one thousand companies are successful with a new approach. They are not looking for eternal truths which are expressed in models, but more for heuristics, pointers, philosophies.

- technical activities: production
- commercial activities: procurement, sales, exchange
- financial activities: search for the optimum use of capital
- security activities: protection of materiel and personnel
- accounting activities: stock control, balance sheet, costs, statistics
- managerial activities: forecasting (prevoyance), organising, giving orders, co-ordinate, control

As I mentioned at the beginning I will restrict myself to describing a division of labour only between the two extremes of the continuum of business science research programmes.

According to what was maybe the first Management scholar, Henri Fayol, Traditional Business Administration seems to be well equipped for technical activities and accounting activities, but probably not so well equipped for the commercial activities and not at all suited for the managerial activities. This follows mainly from the view of what the role of the scientist is. A prescriptive attitude needs a field of interest for which stable predictions can be made and for which a theory is a good heuristic to find a solution to a problem. That generally also includes operations research, although sometimes what seems like a straight-forward technical problem turns out to have a social component. What Horst Rittel (1969) so endearingly called a wicked problem is a nice example. The programming of three lifts in a high-rise turned out to have such a human component (Conklin 2007). The goal of programming the three lifts is to have an optimised solution, i.e. have the most optimal availability of lifts for any given demand. If theory is employed the basic starting assumption is that this theory delivers that perfect solution, i.e. that the theory is an appropriate solution generator. A perfect solution means that there is no chance to further improve the solution. Learning is not included because it cannot happen. The problem of course is that very few problems have a solution which cannot be improved on. Thus, operations research, involving the optimisation of a future production process is suddenly a doubtful application of Traditional Business Administration ideology. That is so because of the confluence of learning, a preparation for the future and human interaction, which means there are no final solutions. This very much sounds like I am preparing a case for abandoning Traditional Business Administration completely. I am not. That science is not an exact science may sound a little paradoxical, but should, 80 years after Heisenberg's Uncertainty Principle and 20 years into a constructivist debate, be widely accepted. Thus, if a research programme delivers superior outcomes (an *a priori* assumption based on experience and the rule of thumb), i.e. it is a promising heuristic tool, it should take precedence over the inferior research programme. Since Traditional Business Administration ideology is focused on solving simple to complicated problems with the help of theories and models based on mathematics and linear logics, operations research, accounting, taxes, etc. are clearly areas for which it is better suited than Management ideology. The example shows, however, that Traditional Business Administration could learn from Management and make, in this case, Operations Research more robust and give research a new open-ended path.

Management on the other hand seems to thrive on complex and open-ended problems involving an uncertain future. The readers may forgive me this pleonasm, but it is still not widely accepted that the future is indeed uncertain, despite Ludwig von Mises's (the academic mentor of Hayek) assurance that "[t]he honest historicist would have to say: Nothing can be asserted about the future." (Mises, 1985, 203) Attempts to talk about strategic problems for instance, must be based on the assumptions of the Management paradigm. The Balanced Scorecard is therefore

a futile attempt, so are the development of balance sheets for measuring organisational knowledge or culture.

Going back to Fayol's "forecasting (*prévoyance*), organising, giving orders, co-ordinate, control*"* one has a complete description of areas in which the Management would deliver more robust and effective solutions that Traditional Business Administration. The first four are more or less concerned with the unknowable future and they deal with human problems on top of that. Even the last one, control, in my opinion clearly is more in the realm of Management than the Traditionalist camp. It is in fact, as we argued previously, the issue which caused the separation of Management from the Traditional research programme. Impersonal control *creates* self-organisation in a group, as the Hawthorne Experiments showed. Despite these rather old findings, the most popular German-language textbook on Business Administration, referred to simply as *The Wöhe*[14], still holds on to quite outlandish assumptions. Leadership for instance must be rid of personal elements, it must be *neutralised* (Wöhe, 2002, 254). It is not really surprising that research in Leadership is not really flourishing on the basis of such absurd assumptions, which are only needed so that numbers, not incalculable human variables are the basis for theories. Thus leadership is a field of research where the Management ideology will produce much more fruitful results. One of the oldest discussions in leadership, whether managers are also always leaders and vice versa, can instantly be ended, because by definition a manager is only a (first line) manager if he leads people. A person who is not supervising others is not a manager and therefore also not a leader. What is more, the confusion about the terms managing, manager, and management disappears. Employees with a supervision span of 0 who write contracts or collect figures, can no longer be called contract managers or finance managers. Even worse usages of these terms, like engine management, water management, waste management, etc, are instantly unmasked as nonsensical.

Tweaking a few of the minor assumptions only makes the situation worse. Traditionalist must allow Management protagonists onto the playing field. It will do the whole field of business science a world of good. A proper basic division of labour, which gives guidance without destroying a meaningful exchange of ideas, could create focus and academic openness at the same time. Both paradigms (the other two which I neglected as well) have a lot to gain from an open discourse, not only the underdog Management paradigm. However, the discourse needs a proper grammar and vocabulary. If the terminology is not even clear, how can that discourse happen? The only way to push forward the boundaries of knowledge is the adherence to the essence of academic work, which according to Mark Blaug (1992: 42) is "remain[ing] loyal to the ideal of intellectual competition". Thus, a new delineation and typology of business science is needed. Burrell and Morgan's grid delivered neither a useful grammar nor debating platform. At the moment the situation in business science is rather perverse in that the acknowledged icons of Management are not shaping mainstream thinking because mainstream thinking is within Traditional Business Administration. One of the reasons for this is, that teaching simple models which involve no risk just diligent application is easier than telling students (MBAs especially) that they cannot be taught perfect management and strategy and that indeed luck and creativity are the decisive ingredients. The question then is what is more important, easy lectures and good advertising arguments or proper education and honest science? No competition, one would have hoped. The other reason is that fewer and fewer researchers have the necessary

[14] The Wöhe is a 1300-page introduction into Business Administration and was written by the nestor of German Business Administration Günter Wöhe and is so precious to German Business Administration that it is still published under Wöhe's name, despite him having stopped working on it some 15 years ago. It has been revised by several authors since his retirement. It has sold more than a million copies.

philosophical training to take part in a debate. That development is maybe even more worrying.

Bibliography

Beer, S.: 1972, Brain of the Firm: The Managerial Cybernetics of Organization. (Penguin Press: London)

Bennis, W. G. and J. O'Toole: 2005, "How The Business School Lost Their Way." Harvard Business Review, 83 (5), 96-104

Blaug, M.: 1992, The Methodology of Economics: How Economists Explain (Cambridge University Press: Cambridge)

Burrell, G. and Morgan, G.: 1979, Sociological Paradigms and organizational Analysis. (Heinemann: London)

Conklin, J.: 2007, Wicked Problems and Social Complexity. White paper, Cognexus Institute, www.cognexus.org

Deetz, S.: 1996, Describing Differences in Approaches to Organization Science: Rethinking Burrell and Morgan and their Legacy. Organization Science, Vol. 7, No. 2, pp. 191-207

Drucker, P. F.: 1964, Managing For Results. (Harper & Row: New York)

Espejo, R. and Harnden, R.(Eds): The Viable System Model, Interpretations and Applications of Stafford Beer's VSM. (Chichester: Wiley)

Fayol, H. 1949 , General and Industrial Management. (Pitman: London)

Ghoshal, S. and Bartlett, C.A.: The Individualized Corporation: A Fundamentally New Approach to Management. (HarperBusiness: New York)

Ghoshal, S.: 2005, Bad management theories are destroying good management practices, Academy of Management Learning and Education, 4 (1), 75-91.

Gomez, P.: 1981, Modelle und Methoden des systemorientierten Managements. (Paul Haupt: Bern)

Hamel, G. and Prahalad, C.K.: 1990, The Core Competence of the Corporation. Harvard

Business Review, May-June, 79-91

Hamel, G. and Heene, A. (eds): 1994. Competence-based competition. (Wiley: New York)

Handy, C.B.: 1989, The Age of Unreason. (Harvard Business School Press: Harvard)

Hoppe, H.-H.: 1997, On Certainty and Uncertainty, Or: How Rational Can Our Expectations Be? Review of Austrian Economics, 10, (1), 49-78

Hühn, M.: 2000, Rezension von H.E. Büschgens, Grundlagen des Bankmanagements. In: Die Bank – Zeitschrift für Bankpolitik und Bankpraxis, September 2000.

Hühn, M.: 2007, The Ethical and Theoretical Context of Corporate Governance. Journal of Corporate Ownership and Control, Vol 5, Issue 1, pp 282-291

Kaplan, R.S. and Norton, D.P.: 1992, The Balanced Scorecard – Measures that Drive Performance. Harvard Business Review, Vol. 70, Issue 1, pp71-79

Kuhn, T.S.: 1962, The Structure of Scientific Revolutions, (University of Chicago: Chicago)

Lakatos, I.: 1978, The Methodology of Scientific Research Programmes: Philosophical Papers Volume 1 (posthumously edited by John Worrall and Gregory Currie) (Cambridge University Press: Cambridge)

Maturana, H. R., J. T., Lettvin, W. S. McCulloch and W. H. Pitts: 1959, What the frog's eyes tells the frog's brain. Proc. Inst. Radio Engr. 47 (11)

Maturana, H. R. and F. J. Varela: 1980, Autopoiesis and Cognition: The Realization of the Living, (Reidel: Dordrecht

Mayo, G.E.: 1949, The Social Problems of Civilization. (Routledge and Kegan Paul: London)

Mintzberg, H.: 1975, The manager's job: folklore and facts, Harvard Business Review 53 (4), 49-61

Mintzberg, H. : 1979, The Structuring of Organizations. (Prentice Hall: Englewood Cliffs, NJ).

Mintzberg, H. : 1983, Power in and Around Organizations. (Prentice Hall: Englewood Cliffs, NJ).

Mises, L. von, 1985: Theory and History. (Ludwig von Mises Institute: Auburn)

Morgan, G.: 1986, Images of Organization. (Sage: London)

Penrose, E.T.: 1959, The Theory of the Growth of the Firm. (Oxford: Oxford University Press)

Porter, M.E.: 1980, Competitive Strategy: Techniques for Analyzing Industries and Companies. (Free Press: New York)

Porter, M.E.: 1996, What is strategy? Harvard Business Review, 74 (6), 61-78

Popper, K.: 1934, Logik der Forschung (Springer: Vienna).

Popper, K.:1965, Conjectures and Refutations. (Harper and Row: New York)

Rittel, H.: 1969, Reflections on the Scientific and Political Significance of Decision Theory. Working Paper 115, The Institute of Urban and Regional Development, University of California, Berkeley

Schwaninger, M and Espejo, R.: Organisational Fitness. Corporate Effectiveness through Management Cybernetics. (Frankfurt/New York: Campus)

Wöhe, G.: 2002, Einführung in die Allgemeine Betriebswirtschaftslehre. (Verlag Franz Vahlen: Munich), 21st edition