3.2 Exploring the Dominant Logics of Strategy Research

In the following, we consider the identified 20 documents as those strategic realities by means of which we examine whether research in strategic management is based on dominant logics. If *SMJ*-authors heavily cite these documents and this journal is considered to be the most important outlet for strategy researchers, there is reason to believe that if we show that these documents reflect the basic belief structures of the dominant logics, we can assume that the underlying assumptions really are dominant. It needs to be recognized that the judgment whether a strategic reality submits to a dominant logic depends on *our interpretation*. Especially when considering that deconstruction argues that texts can have more than one meaning, this is a necessary limitation to our analysis. As we are looking for underlying research assumptions, we face the problem that premises scholars attach to their work are often not directly communicated. Statements like 'I regard the market to be objectively given' or 'I think the strategic concepts introduced in my book are valid independent of their context of application' rarely exist. Therefore, it is crucial to read between the lines and to refer to examples as well as methodological remarks to come up with a reasonable judgment.

3.2.1 Strategy Context – The 'Necessity of Adaptation'

Recall that the 'necessity of adaptation' represents the widely held assumption that the environment exists in an objective manner and that organizations need to adapt to this *one* environment to achieve appropriate 'strategic fit'. Companies make up their environment in the sense that *all* organizations are part of one environment. This environment represents the point of reference for strategy formation. When looking at the identified 20 strategic realities, we stumble over various statements that represent this logic. Williamson (1975: 20) argues that "I assume, for expositional convenience, that 'in the beginning there were markets'." For him the market is given in a sense that it provides an origin all actors can refer to. The market is 'there' regardless of whether organizations are 'there'. The implications of this perspective for strategy are far reaching and mostly highlight the need to achieve a match between organization and environment. Hofer and Schendel (1978: 4) argue that "[t]he basic characteristics of the match an organization achieves with its environment is called its strategy" and Ansoff (1987b: 24) regards strategy as being concerned "with establishing an 'impedance match' between the firm and its environment." The

logic behind these statements is the concept of 'environmental fit' that draws a distinction between environment and organization.

Lawrence and Lorsch's (1967) contribution outlines a contingency theory of organizations by investigating what kind of organization it takes to deal with certain environmental circumstances. By following an open systems approach, they argue that "an important function of any system is adaptation to what goes on in the world outside." (Lawrence and Lorsch 1967: 7) Although, both authors explicitly dismiss an organization theory that is based upon a single best way of organizing in all situations, the contingency approach they outline still assumes that there is a one best way, though one that is bound to the circumstances of a specific environment. The industry forces organizations to adopt a particular strategy and structure; the environment demands and the organization reacts accordingly.[36]

Nelson and Winter (1982: 14) in their contribution *An Evolutionary Theory of Economic Change* provide a widely recognized critique of neoclassical economic modeling. In line with evolutionary thinking, they conceptualize the market as a 'selection environment' that defines the routines which determine the conduct of organizations.

> "Given a flow of new innovations, the selection environment thus specified determines the way in which relative use of different technologies changes over time." (Nelson and Winter 1982: 263)

Determination in their model also runs the other way: firms determine the market to a certain extent. Thus, Nelson and Winter's (1982: 18) core concern is with "the dynamic process by which firm behavior patterns and market outcomes are jointly determined over time." This makes their model a kind of co-evolutionary framework. Although, the co-determined nature of the environment reaches beyond much traditional thinking in economics, it is still 'determination' that makes the core of their theory. In a world of determination, regardless whether we conceptualize it as a joint or a simple one-way type, there is only one environment that provides the Archimedean point for strategy formation. Firms may have a saying in constituting *the* environment, yet the industry conditions, that are partly the outcomes of firms' decisions, are relevant for all market players and consequently determine their conduct.

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[36] Strategy scholars have followed this contingency approach as the following statement indicates: "The field needs to begin to ask and test scientific questions: if a manager finds conditions X, Y, and Z, then he is most likely to be more effective if he follows strategy 'A' than 'B'." (Fahey and Christinsen 1986: 169) Hofer (1975) even outlines a contingency theory of business strategy.

The environment/organization distinction becomes most obvious when referring to Andrews's (1971) well-known SWOT analysis, where strengths and weaknesses refer to the inner dimension and opportunities and threats define the outer dimension. Both dimensions are clearly separated, representing the notion of a boundary surrounding an organization adapting to its environment 'out there'. In a similar way, Thompson (1967), who follows an open systems approach, views the environment as something organizations need to adapt to in order to achieve fit. He argues that the organization deals with variables of the environment over which it has no formal authority or control. This implies that "[t]he organization must conform to the 'rules of the game' or somehow negotiate a revised set of rules." (Thompson 1967: 148)[37]

The 'necessity of adaptation' is also reflected in Chandler's (1962) strategic reality since he states that

> "because all of them [his examined organizations] operated within the *same* external environment, these chapters in the collective history of the industrial enterprise [...] followed roughly the underlying changes in the over-all American economy." (Chandler 1962: 385, emphasis and annotation added)

Elsewhere in his book he covers the question why strategies come into being in the first place and argues that new strategic moves are necessary to respond to the opportunities and threats created by the environment (Chandler 1962: 15). It is thus reasonable to assume that Chandler views the environment as something organizations need to adapt to while making sense of their strategy. Based on the work of Chandler, Rumelt (1974) argues that because of certain developments in the environment (e.g., growth of markets and changes in technology) firms face a need to diversify. Diversification is an adaptive response to the needs of the market.

The 'necessity of adaptation' can also be discussed with regard to the market and resource-based paradigm. Advocates of the resource-based view implicitly refer to the adaptation-based logic by defining the value of resources with regard to a company's environment:

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[37] Thompson's (1967) notion of adaptation reaches beyond a simple determinism. He states that "[w]e must emphasize that organizations are not simply determined by their environments. Administration may innovate on any or all of the necessary dimensions, but only to the extent that the innovations are acceptable to those on whom the organization must and can depend." (Thompson 1967: 148) The environment may be altered, however, only to the extent that *the* environment accepts these 'rules of the game'. In other words, Thompson does not reach beyond the assumption that there is only one environment that is relevant for *all* organizations.

> "Firm attributes may have the other characteristics that could qualify them as sources of competitive advantage (e.g., rareness, inimitability, non-substitutability), but these attributes only become *resources* when they exploit opportunities or neutralize threats in a firm's environment." (Barney 1991: 106, emphasis in the original)

According to this logic, resources are only valuable if the environment regards them as such. Although Barney remains remarkably silent on the question which environmental characteristics make a resource valuable, other advocates of the resource-based view define 'customer value' as the condition under which resources can be a source of sustained competitive advantage. Stalk et al. (1992: 62) argue that a "capability is strategic only when it begins and ends with the customer." In this sense, the customer becomes objectified and the organization is once again in search of 'environmental fit'. Organizations are thought to possess unique bundles of resources which are thought to be determined by the environmental context.

A similar way of reasoning applies to the market-based view. Porter (1980, 1985) treats industry structure as an objective entity firms need to adapt to. He explicitly stresses the importance of 'environmental fit' which he sees as one important variable in determining the appropriateness of a competitive strategy (Porter 1980: xix). According to Porter, the industry environment *determines* the competitive rules of the game:

> "Industry structure has a strong influence in *determining* the competitive rules of the game as well as the strategies potentially available to the firm. Forces outside the industry are significant primarily in a relative sense; since outside forces usually affect *all* firms in the industry. […] The intensity of competition in an industry is neither a matter of coincidence nor bad luck. Rather, competition in an industry is rooted in its underlying economic structure and goes well beyond the behavior of current competitors." (Porter 1980: 3, emphasis added)

In Porter's strategic reality the industry is perceived as an objective structure that circumscribes the actions of organizations located within it. There is much about *the* environment but little about the way organizations create this environment. Notwithstanding our discomfort with the underlying assumptions of Porterean industrial organization economics, it needs to be recognized that Porter also modified the traditional assumptions of the Mason-Bain paradigm by allowing conduct (strategy) to influence industry structure.

> "Traditional IO theory took industry structure as exogenously given, and held that the firm's strategy and performance were fully determined by this structure. […] Policy practitioners, on the other hand, have long observed that firms can fundamentally *change* the structure of their industry through their actions. […] However, firms cannot always change industry structure, and thus under-

> standing industry structure in the traditional IO sense is crucial." (Porter 1981: 613, emphasis in the original)

At first glance, this seems to move Porter beyond the 'necessity of adaptation' because organizations are allowed to change the industry structure by means of their actions. Regrettably, Porter does not really take this assumption seriously nor allows it to unfold to its full potential. First, he assumes that there are certain structural parameters (for instance determined by technology) that cannot be altered. Hence, industry evolution can take different paths, however only within the 'space' provided by these parameters (Porter 1981: 616). Second, even though he allows for modifications of the industry structure, he still believes that the modified structure is relevant for all market players. The premise of determinism is not abandoned but weakened by allowing organizations to influence *the* environment. This, however, does not imply that organizations create *their* environment.

A similar way of reasoning applies to Scherer (1980) who is less concerned with strategic management but outlines an economically focused theory of industrial organization. Similar to Porter he assumes that "[c]onduct in turn depends upon the *structure* of the relevant market." (Scherer 1980: 4) Even though he acknowledges the feedback effects of firm conduct on market structure, it needs to be doubted that he moves beyond an adaptation-based view. He argues that

> "[b]y introducing a much richer complement of independent variables, we should be able to predict conduct from structure, and performance from conduct, with greater precision and confidence." (Scherer 1980: 6)

Scherer does not really challenge the underlying logic of traditional, Mason-Bain-type, industrial organization thinking. Although he uses a more sophisticated analysis and emphasizes firm conduct more than Bain (1968) does, there is no critical discussion of the basic premise that market structure determines firm conduct.

In their much-cited contribution *A Behavioral Theory of the Firm*, Cyert and March (1963: 1) remark that "[i]f the market completely determined the firm's economic behavior, these internal attributes would be little more than irrelevant artifacts." At first, this seems to move the authors beyond the 'necessity of adaptation' as they assume that firms have some control over the market. While Cyert and March outline a theory of the firm, they do not discuss this issue in any more detail but note that for them the environment consist most of all of other organizations. Then, the question remains if these organizations make up the environment in such a way that all social actors are parts of one reality. From our perspective it needs to be

doubted that Cyert and March really dismiss this assumption as they outline a variety of models to predict organizational behavior in the environment. In a world of non-environmental determinism there is no room for prediction of organizational moves because organizations make up their own environment and do not belong to some overarching 'market' that is controlled by invariant laws.

A contribution that deserves special attention is the one by Pfeffer and Salancik (1978). In their book *The External Control of Organizations – A Resource Dependence Perspective*, they have much to say about the relation between an organization and its environment. Their basic argument can be summarized as follows:

> "Because organizations import resources from their environments, they depend on their environments. Survival comes when the organization adjusts to, and copes with, its environment, not only when it makes efficient internal adjustments." (Pfeffer and Salancik 1978: 19)

At first this sounds much like an argument that is based upon adaptation logic. Pfeffer and Salancik (1978), however, are quite aware that many scholars of organization theory tend to view the environment as something given, an assumption they criticize. Accordingly, they suggest that organizations also have an influence on their environment.

> "Perhaps one of the most important influences on an organization's response to its environment is the organization itself. Organizational environments are not given realities; they are created through a process of attention and interpretation." (Pfeffer and Salancik 1978: 13)

Again, we face the question whether the authors really make a case against the notion that there is one reality all organizations share or whether they assume that, even though firms can influence their environment, this environment remains the focal point for all organizations. On the one hand, Pfeffer and Salancik (1978: 72) take much care to point out that "[t]he events of the world around us do not present themselves to us with neat labels and interpretation" but that organizations give meaning to their environment. On the other hand, they follow an open systems approach in line with adaptation-based logic and put much emphasis on the role of constraints. A position that reaches beyond the 'necessity of adaptation' does not claim that what happens in an organization "is also a consequence of the environment and the particular contingencies and constraints deriving from that [one] environment." (Pfeffer and Salancik 1978: 3, annotation added) Throughout their discussion it remains unclear whether the environment really is an organizational construction.

The work of Miles and Snow (1978) explicitly addresses the problem of adaptation-based logic. They try to reach beyond the rigid view of the organization-environment relationship by arguing that

> "*[o]rganizations act to create their environment:* Until recently, much organizational research has been based on the assumption that organizations respond in predictable ways to the conditions which surround them, adjusting their purpose and shape to meet market and other environmental characteristics." (Miles and Snow 1978: 5, emphasis in the original)

At the same time they also claim that organizations need to adapt to their environment to achieve 'fit'.

> "Organizational survival may be said to rest on the quality of the 'fit' which management achieves among such major variables as the organization's product-market domain, its technology for serving that domain, and the organizational structure and processes developed to coordinate and control technology." (Miles and Snow 1978: 19)

This runs counter to their original premise that organizations create their environment. Throughout their book it remains largely unclear whether they move beyond adaptation-based research or not. What can be said for sure is that they recognized the problem of conceptualizing the environment as a determining force at an early point in time.

The contribution of Penrose (1959/1995) discusses the relation between environment and organization in a similar way. Towards the end of her book, Penrose argues that

> "[t]he environment has been treated not as an objective 'fact' but rather as an 'image' in the entrepreneurs mind; the justification for this procedure is the assumption that it is not the environment 'as such', but rather the environment as the entrepreneur sees it, that is relevant for his actions." (Penrose 1995: 215)

According to this perspective, there is no need to adapt to an external environment because the organization creates its environment. Unfortunately, Penrose (1995: 217) does not really uncover the circular relationship between environment and organization as she argues that managers need to interpret their environment *correctly* in order not to fail in their efforts. Then the question remains: Who determines whether the environment has been interpreted in a correct way? Regrettably, Penrose discusses these issues in no more depth. We can thus only assume that even though she recognized the importance of moving beyond the assumption of a given environment, there still is a smack of determinism in her arguments; as she argues herself: "There can be no question that for any particular firm the environment 'determines' its opportunities." (Penrose 1995: 217)

Last but not least, Quinn's (1980) contribution needs to be discussed. Although Quinn is not much concerned with the organization/environment-relation, he presents determinants of the strategy process some of which are "dictated by the industry and its environment" (Quinn 1980: 43) and consequently cause similarity within one industry. Quinn assumes that these determinants have a significant influence on the strategy process of an organization. In reverse, this implies that firms need to adapt to these circumstances in order to manage their strategy process in a meaningful way. We can thus argue that Quinn (1980) subordinates to the 'necessity of adaptation'.

To conclude, even though some scholars have challenged the 'necessity of adaptation', none of the outlined strategic realities fully reaches beyond the linear logic between environment and organization. Most scholars that challenge the notion of 'fit' still present arguments that reveal a conceptualization of the environment that is based on the assumption that all organizations are part of one common reality. Yet, a serious treatment of the circular relation between both spheres would uncover the paradoxical foundation of the environment-organization bond. Henderson and Mitchell (1997: 8) put it in a nutshell by contending that strategy research "has taken primarily an adaptive view of organizational and environmental change, arguing that many firms can adapt their strategies and capabilities as competitive environments change."

3.2.2 Strategy Process – The 'Primacy of Thinking'

Recall that the 'primacy of thinking' argues that strategies are thought to be developed as a grand plan based on forecasts and implemented thereafter. The process can be split up into a number of sub-activities which, if followed in the right manner, lead to the construction of a successful strategy. Thinking is privileged over action because strategies are formulated through a tightly controlled process of human thought. It is believed that strategy is initiated by top management and based on a rational analysis of the environment and the organization. The strategy process is viewed as an intentionally designed meta-plan forecasting the future direction of a company. Put differently, formulation and implementation follow each other in a linear manner like a cause determines its effect. Bromiley and Papenhausen (2003: 415) notice that rational optimization has much in common with strategy. This implies that authors often make the assumption that agents identify certain alternatives, evaluate them to then choose 'the best' of all possible alternatives. This procedure assumes that preferences exist in a fully accessible way *before* decisions are made.

Central to the distinction between formulation and implementation is the premise that structure follows strategy. According to Chandler (1962: 14), who raised the need for strategic planning by arguing that "structure follows strategy and that the most complex type of structure is the result of the concentration of several basic strategies", changes in the environment create the need for new strategic moves (formulation) which in turn require an adaptation of the organizational structure (implementation). Only when the strategy is known, management can begin to specify the appropriate structure of an organization.

> "It seems wise here to emphasize the distinction between formulation of policies and their implementation. The formulation of policies and procedures can be defined as either strategic or tactical. *Strategic* decisions are concerned with the long-term health of the enterprise. *Tactical* decisions deal more with the day-to-day activities necessary for efficient and smooth operations. But decisions, either tactical or strategic, require *implementation* by an allocation or reallocation of resources – funds, equipment, or personnel." (Chandler 1962: 11, emphasis in the original)

The assumption is that each time a new strategy is formulated, the organizational structure is changed accordingly to fit the novel strategic needs. Rumelt (1974: 149) is in line with these arguments ("The data gave strong support to Chandler's proposition that 'structure follows strategy'") but adds that structure may also affect strategy. This is because certain structural moves, which occurred due to small (not strategically intended) variations in products, might lead to further diversification activities. That strategy follows structure does not imply that the thinking/action opposition is reversed, but that firms are more likely to diversify further than if they had not reorganized. Structural reorganization is a 'nucleus' for subsequent strategic moves.

Miles and Snow (1978: 7) agree with the early Mintzbergean perspective that strategy is a pattern in a stream of major and minor decisions about an organization's future domain. Their association with the 'primacy of thinking' becomes most obvious if we consider that Miles and Snow (1978) proclaim, with reference to Chandler (1962), that strategy can be inferred from the behavior of an organization. They argue that one can "conceptually associate strategy with *intent* and structure with *action*." (Miles and Snow 1978: 7, emphasis in the original) Decisions that are made at a certain point in time tend to harden to become the relevant aspects of tomorrow's organizational structure.

Another author who is commonly associated with the formulation/implementation opposition is Andrews (1971) and the proponents of the so-called 'design school', to use Mintzberg's (1990a) terminology.

Like Chandler, Andrews sees corporate strategy as an organization process from which two important aspects can be separated.

> "The first of these we may call *formulation,* the second *implementation*. Deciding what strategy should be may be approached as a *rational* undertaking. […] The process described thus far assumes that strategists are analytically objective in estimating the relative capacity of their company and the opportunity they see or anticipate in developing markets." (Andrews 1971: 24-25, last emphasis added)

Andrews advises managers to identify the underlying market opportunities and threats as well as the strengths and weaknesses of a company as objectively as possible. Based on this information, management produces several strategic alternatives of which one is to be selected. Mintzberg (1990a: 175-179) reminds us of the key premises that underlie Andrews's conception of the strategy process. First, Andrews understands strategy formation as a tightly controlled and conscious process of thought. Strategy is associated with intentionality because action follows once a strategic alternative has been chosen. Second, responsibility for that control must rest with *the* strategist (usually the chief executive officer).[38] Third, strategy formation must be kept simple. This premise directly refers to the second one because if strategy can be controlled by one mind, the resulting process is kept rather uncomplicated. And fourth, strategies should be explicitly articulated throughout the organization because an unstated strategy does not require actions and thus results in ineffectiveness (Andrews 1971: 110).

A similar rational approach is suggested by Ansoff (1987b). Unlike Andrews, he highlights the influence of staff planners instead of the manager and provides a more detailed and formalized approach to planning supported by a range of analytical techniques (Mintzberg 1991: 463). Notwithstanding these differences, Ansoff puts much emphasize on the linear nature of the strategy process. This is nowhere more evident than in his action cycle model of strategy development which divides the entire strategy process into a number of small steps. Ansoff (1987b: 178) refers to this method as a 'cascade' that starts from a rather broad choice of the firm's future business areas and progresses to generating specific projects (e.g., newly developed products or service innovations), which are implemented at the end of the process. This formalized approach has also been applied by Hofer and Schendel (1978: 5) who argue that "[i]f strategy is important, its formulation should be managed and not left to chance. In this regard, organizations need formalized, analytical processes for formulating ex-

[38] The limitations of this 'command and control' model are discussed by Levy et al. (2003: 98) and Stoney (1998: 4).

plicit strategies." Throughout their book they describe a variety of such formalized analytical concepts. Considering their numerous references to the work of Andrews and Ansoff, it is obvious that these concepts are based on a linear notion of causality.[39]

Moving to the Porterean market-based view, we rarely find explicit statements regarding the nature of the strategy process as Porter is more concerned with strategy content.[40] Nevertheless, at the very beginning of his book *Competitive Strategy*, Porter (1980: xiii) argues that strategy

> "may have been developed explicitly through a planning process *or* it may have evolved implicitly through the activities of the various functional departments of the firm." (emphasis added)

This seems to place him somewhere in the middle of the deliberate/emergent strategy debate as he conceives both forms as an either/or-decision and does not highlight their interrelated nature, as do Mintzberg and Waters (1985: 258). Yet, Porter (1980: xiii) qualifies this statement by arguing that the sum of the different departmental approaches rarely equals 'the best' strategy for an organization. Because of this, there are significant benefits to gain from an *explicit* (and thus planned) approach to strategy. Porter (1980: xiv) sees his strategic reality as a way to *predict* industry evolution. Prediction means that strategic moves are not left to chance but are deliberately planned. In Porter's (1996: 77) strategic reality, strategy requires "strong leaders that are willing to make choices." Such choices

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[39] The formulation/implementation distinction is most evident in Ansoff's book *Corporate Strategy* which is divided into two parts: Strategy Formulation and Strategy Implementation. Ansoff's (1987b) belief in the rational capacity of human beings to forecast – a view from which he rarely deviates (Ansoff 1991) – becomes obvious when considering the advice he gives for diagnosing environmental turbulence: "The procedure for diagnosing environmental turbulence is to circle the observed values which best describe the condition of each of the eleven attributes [which cause environmental turbulence]. For purposes of capability diagnosis, the circled values should represent the anticipated condition of the environment five to seven years into the future." (Ansoff 1987b: 207, annotation added) Over the years, Ansoff (1979: 31) also critically reflected upon this mode of thinking but never replaced his basic presupposition of a linear strategy process.

[40] The resource-based view remains notably silent on these issues as well. Even though Barney (1991: 113) recognizes that a formal planning system enables a firm to recognize and exploit its resources, he does not provide any precise statements on the nature of the planning process. He only highlights the role of informal strategy-making processes in achieving competitive advantage. Following Barney, such informal processes may be of value because they are rare and less likely to be imitated.

position the company and have a horizon of a decade or more (Porter 1996: 74).

Quinn (1980) provides one of the best-known critical assessments of formal planning by stressing the notion of 'logical incrementalism'. Formal planning is nothing more than a textbook notion because strategies seem to emerge in quite different ways. In fact, he argues that "the real strategy tends to evolve as internal decisions and external events flow together to create a new, widely shared consensus for action among key members of the top management team." (Quinn 1980: 15) His empirical study of strategy formation in nine organizations reveals that strategy making takes an incremental form since effective strategies emerge from a series of strategy formulation subsystems. Because the different subsystems cannot be fully coordinated and each subsystem possesses certain cognitive limits, the overall strategy does not evolve in a well-planned way. Quinn's (1980: 18) point is that "no single formal analytical process could handle all these strategic variables simultaneously on a planned basis."

At first, it seems that Quinn's notion of logical incrementalism moves us beyond the 'primacy of thinking' because it stresses, similar to Mintzberg's (1979) perspective, the non-causal flow of the elements of the strategy process. A closer look, however, demonstrates that although Quinn (1980) challenges the classical, formalized nature of the strategy process, he does not alter the deeply held assumptions of strategizing as a process based on a distinction between thinking and action. The 'logical' component of 'logical incrementalism' suggests that skillful managers maneuver the strategy process in a purposeful and effective manner (Quinn 1980: 17). As a result, Quinn (1980: 58) assumes that strategy making is still largely guided by rational behavior. Even though, formulation is a messy process in which different subsystems can hardly be synchronized and Quinn (1980: 145) is well aware that strategy making consists of the incremental interaction of formulation and implementation, his notion of logical incrementalism still assumes that thinking and action can be separated. Incrementalism assumes that goals (ends) and alternatives (means) are coupled through feedback loops (Fredrickson 1983: 568). Yet, this does not imply that thinking (the formulation of goals) *intermeshes* with action (the implementation of alternatives). The interrelatedness of formulation and implementation (e.g., by means of feedback loops) is different from a gradual production of a strategy within the course of action. This is not to suggest that Quinn's strategic reality assumes strategy to be based on a clearly identifiable process, but that logical incrementalism cannot entirely reach beyond the well-established thinking/action opposition because thinking and action are still decoupled.

Out of the 20 documents selected for this discussion of dominant logics, the contributions that stem from organization theory and economics do not directly touch upon the 'primacy of thinking' from the viewpoint of *strategy*, but rather address assumptions with regard to the behavior of men in general and/or its consequences for administrative organization. Since it is reasonable to assume that these general assumptions have inspired the argumentation of strategy scholars, we need to identify the presuppositions these contributions follow.

Pfeffer and Salancik (1978) remain notably silent about their assumptions regarding the nature of the decision-making process. Indeed, they conceptualize the environment as a source of uncertainty, constraint, and contingency (Pfeffer and Salancik 1978: 229) and criticize various authors for conceiving organizations as rational instruments for the attainment of goals (Pfeffer and Salancik 1978: 23). They prefer to view organizations as coalitions "altering their purposes and domains to accommodate new interests, sloughing off parts of themselves to avoid some interests; and when necessary, becoming involved in activities far afield from their stated central purposes." (Pfeffer and Salancik 1978: 24) Nevertheless, their overall model of how environmental effects shape organizations looks considerably like a linear process. First, they claim that there is a causal relationship between environmental factors and the distribution of power and control in organizations; the environment influences the distribution of power and control in organizations and consequently the decisional preferences regarding the choice of executives and their tenure. The selection of administrative staff in turn influences organizational actions (Pfeffer and Salancik 1978: 229). It thus needs to be doubted that the authors move beyond a linear notion of decision-making, although they put much emphasize on uncertainty and contingency.

Thompson (1967: 134) takes a mixed approach to model decision processes. He distinguishes between certain and uncertain cause-effect premises and devotes much attention to the constraints of certainty, yet at the same time acknowledges the existence of certainty in particular circumstances. For him organizations are neither rational-machines nor natural systems. Although Thompson challenges the belief in rational organizational processes, he does not dismiss the underlying linear process model. This is because he still believes that principles of administration that are derived from the rational model can be applied "to those portions of organizations which are so protected from exogenous variables that the closed system is practical." (Thompson 1967: 145) This raises the question: Which are the parts of organizations that are so protected that rationality works and how do we identify them?

A similar critique applies to the contribution of Cyert and March (1963). Aware of the limitations of full rationality, they claim that planning should be avoided "where plans depend on predictions of uncertain future events [and emphasized] where the plans can be made self-conforming through some control device." (Cyert and March 1963: 119, annotation added) It remains open how we can meaningfully distinguish between both situations. Cyert and March only advise that in the absence of plans, managers need to solve pressing problems instead of developing sophisticated forecasts. In either case, managers achieve a reasonable decision situation that can be solved and must be 'managed' within the scope of the proposed overall organizational decision-making process. This process represents a step-by-step procedure for dealing with decision problems. Even though the authors allow for feedback loops and organizational learning, the linear character of the model cannot be overlooked: first comes the thinking, followed by the action.[41]

Similar to Cyert and March, those strategic realities that are based on economic analysis stress the bounded rational character of decision-making (Nelson and Winter 1982; Scherer 1980; Williamson 1975, 1985). The concept of bounded rationality deserves our attention as it upsets many of the traditional assumptions regarding decision-making. Bounded rationality is a semi-strong form of rationality that, following the work of Simon (1957, 1979), considers the fact that humans face certain cognitive as well as language limits which make it impossible to identify future contingencies in an *ex ante* manner. According to Simon (1957: 198), bounded rationality reflects that

> "[t]he capacity of the human mind for formulating and solving complex problems is very small compared with the size of the problems whose solution is required for objectively rational behavior in the real world – or even for a reasonable approximation to such objectivity."

Bounded rationality considers (a) that complete knowledge of the consequences that follow each choice is impossible to achieve, (b) that since consequences necessarily lie in the future, imagination must supply the lack of judgment; yet imagination is based on values, and values cannot be

[41] Ironically, Cyert and March's decision model, although still based on a thinking/action type of perspective, leaves us with a framework that radically downplays the importance of organizational goals in general. This is why Ortmann (1976: 59) concludes that Cyert and March leave the impression that organizations are goalless and problems are simply solved as they occur. Cyert and March (1963: 119) argue: "We assume that organizations make decisions by solving a series of problems; each problem is solved as it arises; the organization then waits for another problem to appear."

anticipated, and (c) that full knowledge of all possible alternatives cannot be achieved (Simon 1971: 81). The pairing of uncertainty with bounded rationality makes it unfeasible for decision-makers to account for all possible consequences – hence: "the future is permitted to unfold" (Williamson 1975: 9). The notion of bounded rationality challenges the very essence of the 'primacy of thinking' because bounded rational individuals recognize that they cannot foresee all the things that matter to them. It is thus unlikely that people find the mathematically best solution to their problems. Rather, individuals act intentionally rational; they give the best they can under the limitations they face. The *maximizing* goal of choice is replaced with the goal of *satisficing*; a perfect utility-maximizing choice becomes out of reach.[42]

Simon's perspective of rationality highlights that the problem of choice is one of describing consequences, evaluating them, and connecting them with behavior. Although he moves from an objective notion of rationality (in which thinking *determines* action because 'correct' behavior maximizes given values in a situation) to subjective rationality (in which maximization is relative to the actual knowledge of the subject), the basic premise of 'thinking before action', which is of interest to our analysis, remains unchanged.[43] Simon presents a strong argument by considering decision premises to be incomplete and bound to psychological limits, yet his conclusions are not much in favor of a perspective that highlights the gradual production of decision premises in the course of action. For instance,

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[42] The authors of the economically based strategic realities share this critique. Nelson and Winter (1982) criticize the economic perspective that often conceptualizes economic man as a perfect mathematician; they argue that "[t]his affront to realism is not innocuous. It opens the door to full reliance on the notion of fully preplanned behavior, even in contexts where the level of complexity involved is such as to overwhelm the aggregate capacity of Earth's computers." (Nelson and Winter 1982: 66) Similarly, Scherer (1980) argues: "Decision makers cannot know precisely how strong and how elastic demand will be in the next period, let alone ten years hence, or how far labor unions will carry their struggle for higher wages in forthcoming negotiations, or how rival sellers will react to a price increase, or what the prime interest rate will be next June." (Scherer 1980: 29) In addition, we do not discuss in which way 'individual' and 'organizational' rationality are linked to each other. Simon (1971: 80, 96-109) puts great emphasis on this topic, yet also remains rather vague in describing their relation. Basically, he assumes that organizations 'permit' individuals to approach problems reasonably and even closer to objective rationality (Simon 1971: 80).

[43] "In a strict sense, a decision can influence the future [i.e. action] in only two ways: (1) present behavior, determined by this decision, may limit future possibilities, and (2) future decisions may be guided to a greater or lesser extent by the present decision." (Simon 1971: 97, annotation added)

Simon's (1971: 96) model of how individual behavior is integrated into social systems involves three principal steps: (1) the individual makes broad decisions within the limits of the existing organizational policy (substantive planning), (2) the individual establishes more specific mechanisms that cause day-to-day decisions to conform with the substantive plan (procedural planning), and (3) the individual executes the plan within the framework set by the preceding two steps.

To conclude, although the notion of bounded rationality has been applied by authors of strategic realities (especially, but not limited to, Nelson and Winter 1982; Scherer 1980; Williamson 1975, 1985) and extended strategy process research, the basic conception of a formulated strategy that is implemented thereafter is not fundamentally altered but only limited in its rational-capacity. *Fully* reaching beyond the 'primacy of thinking' implies considering that thinking and action intermesh. Strategy formulation, then, can no longer be detached from implementation: both processes are deeply intertwined and mutually inform each other. Yet, this perspective requires that we tackle the paradox that every decision faces.

3.2.3 Strategy Content – The 'Fullness of Rules/Resources'

Recall that the 'fullness of strategic rules and resources' treats strategic rules and resources as if they were full of meaning and thus *a priori* given. This leads to a generalization of preferences; either the rules of the market or the resources an organization possesses are thought to be applicable without a deep consideration of the context they occur in. Scholars tend to belief that there is something like a final origin of competitive advantage that needs to be discovered. In other words, rules and resources are conceptualized as if they are full of meaning (i.e. iterable and as a result generalizable across contexts) and thus able to define their own conditions of application. As indicated in section 1.1, strategic rules try to generalize *across* organizations, while resource-based theories are aware that firms possess heterogeneous resources yet presume that resources *within* a firm are given regardless of context. To facilitate orientation, we first discuss the 'fullness of strategic rules' and then move to the 'fullness of strategic resources'.

The 'Fullness of Strategic Rules'

It goes without saying that Porter's (1980, 1985) contributions provide by far the most well known rules of strategy. Among Porter's (1980: 3) "general analytical techniques" we can find the notion of industry analysis that

attempts to offer the rules of competition.[44] Porter (1985: 7) is convinced that he has listed "all the elements of industry structure that may drive competition in an industry." The underlying logic is not to apply the rules and thus fill them with meaning, as this would require eventually breaking with their nature or even reinventing them, but to 'tick boxes' working through the long list of determinants that supposedly define the rules of the game. It is in this spirit that Harfield (1997: 8-9) reports:

> "On first reading, my view is that the 'American East-coast Business Ideals', as embodied in *Competitive Strategy*, are not applicable to New Zealand. Competition to gain a dominant market share is about large markets with assumed growth, that is not New Zealand, is it? I assume that the few large public companies might aspire to this model, but New Zealand is made up of small and medium size, often family owned businesses where cooperation is as important as competition. There are factors other than 'profit above industry average' as a reason for being in business, aren't there? The only way to find out is to ask the managers, and listen to their stories."

Porter does not give reference to the process of applying his rules of competition to a context, a context that might force managers to redefine the rules, to break with them – not for the sake of breaking, but to appreciate their context of application.[45]

Porter's second widely shared view is reflected by the rules of achieving competitive advantage by means of positioning. Based on an analysis of industry structure, he argues that there are two types of competitive advantage (lower costs and differentiation) that, combined with the competitive scope (broad or narrow target), yield three generic strategies: cost leadership, differentiation, and focus. To achieve a competitive advantage, organizations cannot be "all things to all people" (Porter 1985: 12), they need to make choices. A firm that does not make such choices or even decides to follow more than one possible strategy becomes 'stuck in the middle' – it possesses no competitive advantage. According to Porter (1985: 16), "[t]his strategic position is usually a recipe for below-average perform-

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[44] Porter (1985: 4) even speaks about 'rules' while arguing that "strategy must grow out of a sophisticated understanding of the rules of competition that determine an industry's attractiveness."

[45] We should note that Porter (1991) has written an article called *Towards a Dynamic View of Strategy* in which he separates his own thinking about strategy in a cross-sectional problem (the causes of superior performance) and a longitudinal problem (the dynamic process by which competitive positions are created). However, in discussing the importance of the longitudinal problem, Porter (1991: 109) only pushes back the chain of causality. He writes about the *origins of the origins* of competitive advantage and acknowledges the importance of the local environment in shaping strategy.

ance." Porter (1980: 40-41) provides long lists of skills that are required to implement each of the three generic strategies. Again, we find a logic similar to that of the concept of industry analysis: the rules to achieve competitive advantage are *pre-given* and managers are advised to make a choice between one of the strategies. Even though Porter (1985: 11) recognizes that *specific* actions are required to implement each strategy, he still places much emphasis on the general and prefabricated nature of his *generic* strategies.

The 'tick the boxes' approach followed by Porter becomes even more obvious if we consider his famous value chain analysis. A value chain displays the total value a firm creates by conducting its value activities. Porter (1985: 39) distinguishes between primary and support activities and argues that the way in which these activities are performed "will determine whether a firm is high or low cost relative to competitors." He is well aware that firms may possess different value chains because their histories differ, but also claims that "every firm is a collection of activities that are performed to design, produce, market, deliver, and support its products" (Porter 1985: 36) and that "[i]n any firm all the categories of primary activities will be present to some degree and play some role in competitive advantage." (Porter 1985: 40) Once again, this demonstrates that Porter provides generalized tools that usually leave some possibility of altering the nature of the tool by choosing among a list of predescribed determinants *prior* to application. The value chain needs to be open for modification to tap its full potential. Here, modification means not adjustment within some predescribed limits but the inevitable enactment within the idiosyncratic context of an organization.[46]

The toolbox of the strategist is extended when looking at the work of Hofer and Schendel (1978) who provide a variety of 'analytical concepts' for strategy formulation. Their analysis in based upon the so-called

[46] What, for instance, if we consider the ever increasing role of information technology? Information systems considered by Porter (1985: 43) to be a support activity, often play a major role in firms' sales activities and their aftersale assistance, and thus eventually qualify as a primary activity in Porter's terminology. Another example regards 'inbound logistics' which, according to Porter (1985: 40), is a primary activity that can be found in every firm (at least to some extent). Inbound logistics is concerned with receiving, storing, and disseminating inputs to the product. But what about corporations that do not need or have inbound logistics, such as a web-designer. There is no material handling, storing, vehicle scheduling or similar operations in such a firm. After all, Porter's analysis is guided by the notion of an idealized firm which does not always exist, especially if we consider that the nature of firms constantly changes with societal trends (e.g., the use of the internet).

'Hofer/Schendel strategy formulation models' for business and corporate level strategy. These models provide a fine-grained view of how to proceed when formulating strategies on both levels of analysis. In the course of their book, Hofer and Schendel introduce a variety of analytical concepts that can be used by mangers to put their proposed strategy formulation process into practice. On the business level, for instance, they suggest six generic strategies that firms can follow. To select 'the right' strategy, firms are advised to consider the stage of evolution of the product/market segment, its current and desired competitive position, as well as its resources. Similar to Porter, the authors argue that any of those six generic strategies are needed to come up with a business strategy. They do not recognize that these suggestions, however important in guiding managers' efforts in strategy making, need to be applied to a specific context, a context in which it might be necessary to modify a generic strategy to such an extent that there is no reason to assume that anything like a true standardized strategy can exist beforehand.

Ansoff (1987b: 111) follows a similar approach as he outlines a portfolio that specifies the combination of available strategic business areas in which a firm wishes to achieve its objectives and subsequently defines four competitive (generic) strategies that firms can use to succeed in each of these areas. Overall, Ansoff's approach to strategy is similar to the ones described so far: develop criteria that allow us to distinguish between different strategic scenarios in order to offer predefined strategies that can then be implemented. Ansoff even moves one step further by applying the different components of the portfolio to different types of firms. For example, managers that look for a strategy for integrated synergistic firms are given the following advice:

> "Since its product-market decisions have long lead times, it needs guidance in R&D, and it must be able to anticipate change. Much of its investment is irreversible, since it goes into R&D, which cannot be recovered, and physical assets, which are difficult to sell." (Ansoff 1987b: 113)

Ansoff believes that there are certain strategic alternatives managers should follow under specific circumstances. Again, we do not doubt that it is useful to provide strategists with some generic options of what a strategy could possibly look like. However, it is dangerous to do so without acknowledging the process of application.

Andrews (1971) provides yet another example of what it means to deliver necessary emptiness. His work has popularized the idea of distinguishing between the external situation a firm faces (its opportunities and threats) and its internal characteristics (strengths and weaknesses). Today, we rarely find any textbook on strategy that is not referring to this distinc-

tion. Over the years, Andrews's confrontation of external and internal factors became better known as SWOT analysis and was described as a rigorous analytical tool (Weihrich 1982). Needless to say, SWOT analysis overlays corporate diversity with a generic solution. Neither Andrews (1971) nor any of his predecessors, relate the relevance of the SWOT approach to managers' ability to use and apply this tool to their specific context. All that is offered by Andrews (1971: 71) are long lists of factors that could possibly be considered during an analysis. What are the results of this orientation?

Hill and Westbrook (1997) investigate the use of the SWOT concept empirically. Their study of 20 companies that conducted a SWOT analysis reveals that all firms produced long lists of vague terms with the help of consultants. Only in three instances were these lists used for strategy development at all. That is why Hill and Westbrook (1997: 50-51) conclude that:

> "Our principal conclusion has to be that, from the evidence given above, SWOT as deployed in these companies was ineffective as a means of analysis or as part of a corporate strategy review. […] The relevance and usefulness of any approach is in part related to the ability of those involved. Unless their understanding of how an application should be made, together with the ability to undertake the work in a rigorous and meaningful way, then the outcomes will be less than adequate."

Their study shows that the success of SWOT depends on the competence of managers to apply, and thus also modify and extend, the original framework. Simply ticking boxes and producing long lists of vague terms does little good. After all, Hill and Westbrooks' study demonstrates that it is not enough just to call for a consideration of the context of application, but most of all to train those people that are supposed to use strategic concepts. As a result, Andrews's notion of SWOT analysis neglects the process of filling and thus contributes to the 'fullness of strategic rules'.

Another widely used concept is Miles and Snow's (1978) adaptive cycle which describes a model of organizational adaptation. Based on Child's (1972) strategic choice approach, Miles and Snow (1978: 21) argue that adaptation depends on managers' perception of the environment. They believe that this process can be broken down into three major problems – entrepreneurial, engineering, and administrative – that form a cycle which the organization constantly has to deal with. Based on this cycle, they outline four strategic types of organizations – Defender, Analyzer, Prospector, and Reactor – that represent alternative ways of moving through the cycle.

> "That is, if management chooses to pursue one of these strategies, and designs the organization accordingly, then the organization may be an effective com-

> petitor in its particular industry over a considerable period of time. On the other hand, if management does not choose to pursue one of these 'pure' strategies, then the organization will be slow to respond to." (Miles and Snow 1978: 14)

The underlying logic is much the same as in the works that have been introduced up to this point: managers are advised only to follow 'the pure' strategies proposed by the authors: if not, they are told to end up with efficiency losses. The four archetypes suggested by Miles and Snow (1978: 29) possess certain characteristics. Depending on the nature of the firm a manager works in, s(he) needs to choose the right form to be able to predict behavior (Miles and Snow 1978: 30). The four 'pure' types of strategic organizations suggested by the authors do not account for the fact that managers, even if they believe that they fit into one of the categories, need to make sense of the general archetype.

The 'Fullness of Strategic Resources'

Both contributions from the resource-based view that are among our list of strategic realities (viz. Wernerfelt 1984 and Barney 1991) conceptualize resources as *a priori* given and thus full of meaning prior to application. Barney (1991: 101) distinguishes between physical capital resources, human capital resources, and organizational capital resources and Wernerfelt (1984) claims that only those attributes of these types of resources that enable a firm to implement its strategy are called firm resources. The pioneers of the resource-based view in strategy depict resources as stable building blocks of organizations that can be clearly identified and measured, or, in Wernerfelt's (1984: 172) words: "anything which could be thought of as a strength or weakness of a given firm." Resources are 'real' and exist regardless of context or the observer. Yet, such a conceptualization does not reflect the messy realities that managers face when *applying* resources and thus risks lacking meaning for scholars and practitioners alike. Wernerfelt (1995: 172) himself, in a ten-year retrospective view on his own trail-blazing article, argues that "'resources' remain an amorphous heap to must of us."

There are two other points that support our assertion that resources are conceptualized as given. First, Wernerfelt (1984) argues that resources can be used in several products and suggests creating a resource-product matrix that depicts the importance of a resource in a product. Similarly, Barney (1986) uses the concept of 'strategic factor markets' to point out that firms can purchase resources there. Inevitably, these notions imply that resources are easily transferable and for that reason need to be 'given' in some way. Second, because most research in the resource-based view is informed by traditional economic analysis, authors like Wernerfelt and

Barney seem to assume that managers use their resources optimally (Bromiley and Papenhausen 2003). That is, these authors discuss the link between resources and performance in a direct manner, neglecting the managerial choices that are necessary to put resources into action. Again, if people play no or only a minor role, resources are defined without any relation to their application. Even firms with similar tangible resources will have different performance outcomes depending on how management chooses to employ resources.

It should not go unnoticed that Nelson and Winter's (1982) approach to evolutionary economics provides the ground on which much recent resource-based theorizing rests. Their view of the organization as a set of interdependent operational and administrative routines gave rise to the notion of dynamic capabilities (Teece et al. 1997: 516). Although, this conception attaches a 'dynamic' label to the discussion, by emphasizing that competences need to change by means of these dynamic capabilities, the basic problem that dynamism and learning cannot be fixed prior to application remains. Pfeffer and Salancik (1978) argue in favor of a resource-dependence approach determining an organization's action from the 'outside'. Organizations depend on individual or cooperative actors that control critical resources. In their analysis, the authors focus on the question of how resources can be acquired by an organization to avoid vulnerability and to ensure survival (i.e. a state in which a supply of resources is assured). Whereas this points out that resources are important, their ontological status remains unquestioned. Resources are treated as 'given' entities that are either supplied or not supplied by the environment.

Penrose (1995) reaches beyond the notion of 'given resources' by arguing that it is not the resources *per se* but the context-bound use of these resources – the services in Penrose's terminology – that matter. She provides the only contribution within our list of strategic realities that truly reaches beyond a dominant logic, in this case beyond the 'fullness of strategic resources'.[47] According to Penrose (1995: 25), resources consist of a bundle of *potential* services that need to be activated within application; it is not resources *per se* that are important but only the services resources render. Her distinction between resources and services reaches beyond the existing dominant logic because she recognizes that resources need to be transformed into services to be of value to organizations. Regrettably, her insights have been neglected by the major contributions of the resource-based view on strategic management (Kor and Mahoney 2004).

[47] This is even more surprising if we consider that the first edition of Penrose's contribution dates back to 1959, a long time before there was something like a resource-based view on strategy.

3.2.4 The Dominant Logics of Strategy – An Overview

Having discussed the three dominant logics of strategy research and their embeddedness in the 20 most prominent strategic realities, we conclude that *mainstream* strategy research is dominated by certain logics. Our discussion of the 'necessity of adaptation', the 'primacy of thinking', and the 'fullness of strategic rules and resources' shows that the orthodoxy in strategy research still relates to well-established patterns of thought. Figure 12 summarizes the relation between strategic realities and dominant logics.

No.	Publication (Strategic Reality)	Context Necessity of Adaptation	Process Primacy of Thinking	Content Fullness of Strategic Rules and Resources
1	Porter (1980)	O	X	X
2	Rumelt (1974)	X	X	n.a.
3	Porter (1985)	O	X	X
4	Chandler (1962)	X	X	n.a.
5	Williamson (1975)	X	O	n.a.
6	Nelson/Winter (1982)	X	X	X
7	Pfeffer/Salancik (1978)	O	O	n.a.
8	Miles/Snow (1978)	O	X	X
9	Cyert/March (1963)	O	O	n.a.
10	Thompson (1967)	X	O	n.a.
11	Hofer/Schendel (1978)	X	X	X
12	Wernerfelt (1984)	n.a.	n.a.	X
13	Barney (1991)	X	n.a.	X
14	Lawrence/Lorsch (1967)	X	n.a.	n.a.
15	Andrews (1971)	X	X	X
16	Penrose (1959/1995)	O	n.a.	*
17	Ansoff (1965/1987a)	X	X	X
18	Williamson (1985)	X	O	n.a.
19	Scherer (1980)	X	O	n.a.
20	Quinn (1980)	X	O	n.a.

X = represents dominant logic; O = challenges dominant logic but does not reach beyond it; * = reaches beyond dominant logic

Fig. 12. The Embeddedness of Strategic Realities in Dominant Logics

It should not go unnoticed that a number of scholars challenge the dominant logics. However, with the exception of Penrose (1995), they do not entirely reach beyond the established orthodoxy. Whereas these challenges come from inside the mainstream, there are also a variety of challenges that come from outside the mainstream literature (see section 3.4). Most strategic realities that challenge the identified dominant logics, from within or outside the mainstream literature, provide modified perspectives

on the oppositions (e.g., formulation/implementation); however, they do not confront the deeply held assumptions on which scholarly activity rests. From our perspective, any serious attempt to reach beyond the dominant logics has to consider their inherent paradoxical nature as a point of departure for further reasoning. To demonstrate that the determinism that is inherent in the dominant logics (e.g., an objective environment that determines strategic conduct) is based on impossibility (i.e. paradox), we need to expose paradoxical reasoning and show how such reasoning informs future research.

In conclusion, we argue that knowledge in strategy research possesses an *ideological function*. That is to say, the interests of a dominant part of the research community are represented as the interests of the whole. We characterize this ideology as a functionalistic one that approaches research concerns from a positivistic and deterministic stance (Burrell and Morgan 1979: 26). This reminds us of the philosophical work of Husserl's (1970) *The Crisis of European Sciences and Transcendental Philosophy* in which he argues that the rationality of the modern sciences is based upon an idealization of the world that can be called 'the mathematization of nature'. Such an idealization is a way of looking at the world *as if* it could be clearly represented and expressed by a researcher (Leiss 1975). The implicit assumption that is attached to such research is that outdated theories are replaced by new ones that are better in terms of accuracy and validity. In their recent study of strategy research Boyd et al. (2005: 852) claim that this model is still considered desirable by many strategy scholars.

3.2.5 The Embeddedness of Paradigms in Dominant Logics

When contrasting our argumentation in section 2.2 with the claims presented in this chapter, one may be puzzled by the fact that the *pluralism* of paradigms in strategy, which presumes a variety of opinions about what strategy is or should be, conflicts with the *overarching* character of dominant logics. If there is no generally accepted paradigm to pull together the many issues of strategy research, why do dominant logics remain in the field as outlined in the preceding section? Does the notion of dominant logic run counter to our assumption that different paradigms have occurred in strategic management theory thus far? What is the relation between dominant logics and paradigms anyway?

We believe that the pluralism among strategy paradigms exists only on a *superficial level*. Even though resource-based thinking currently dominates the field, no serious researcher would simply forgo definitions of market-based reasoning. This pluralism is of a superficial nature because every

paradigm favors almost the same *dominant* metatheoretical assumptions. These assumptions – largely stemming from the functionalist roots of the field – run throughout the paradigms as *dominant logics*. In other words, dominant logics do not represent a paradigm, at least if we understand a paradigm as defined in section 2.2.1, but are *attached* to the paradigms that occurred in the history of strategic management. As a result, strategy research not only relates to certain dominant logics, but these logics are also well represented by the paradigms of the field.[48]

Our premise is that even though the paradigms of strategy research may differ at first glance, because they focus on such diverse issues as long-range planning of internal functions or the analysis of industry structure, they are stuck in a straightjacket. Even though model problems and model solutions differ among paradigms, the assumptions of strategy scholars have largely converged into a single set of assumptions *on a metatheoretical level* (Boyd et al. 2005: 853). These assumptions are represented by the dominant logics that we identified in the preceding section; they reflect a fundamental belief about how the world works and help to frame research problems and the chosen methodology. Regardless of whether we agree with Daft and Buenger (1990: 82) that strategy research 'rushed' into this convergence at too early a point in time, the devotion to dominant logics inhibits intellectual growth by focusing attention on minor research problems and neglecting 'the big picture' within which these problems are located.

To illustrate this point, consider the following example. The 'necessity of adaptation' cuts across a variety of paradigms in strategy research. The need for environmental fit was recognized by the early advocates of the 'planning' paradigm (e.g., Andrews 1971; Ansoff 1987a; Chandler 1962) and sustained by the 'forecasting' orientation in the 70s (e.g., Hofer and Schendel 1978). Even the advocates of the 'market-based' paradigm (e.g., Porter 1980, 1985) and the 'resource-based' paradigm (e.g., Barney 1991) relate, either implicitly or explicitly, to the belief that the environment is in some sense an 'origin' for strategic reasoning and that consequently firms have to adapt to whatever is demanded by this 'origin'. Thus, the 'neces-

[48] This highlights the relation between normal science and dominant logics. Whereas normal science occurs *within* a paradigm, dominant logics *cut across* paradigms (see also section 1.1 and the glossary). Kuhn (1970: 245) heavily criticizes researchers for taking too lax an attitude towards normal science: "If [...] some social scientists take from me the view that they can improve the status of their field by first legislating agreement on fundamentals and then turning to puzzle solving, they are badly misconstruing my point." See also the essays in Lakatos and Musgrave (1970).

sity of adaptation' reflects a deeply rooted assumption that has been followed by a variety of strategic realities in different paradigms.

To answer the questions posed at the beginning of this section, we note that dominant logics correspond to parts of paradigms, i.e. their metatheoretical assumptions. Thus, the notion of dominant logic neither runs counter nor conflicts with the idea of paradigm. It is our firm belief that unless the dominant logics are challenged, they will continue to govern future paradigms regardless of whether they favor resource, market-based, or any other kind of reasoning. The question strategy scholars face is whether they wish to remain comfortably seated in the fast train that is heading to nowhere as described by Daft and Buenger (1990) or whether they wish to bring this train to a halt, not to reverse its direction, but to take a deep breath and figure out where they wish to go and how they wish to get there in the first place.

3.3 How Dominant Logics Come into Existence

Having claimed that strategic management possesses three dominant logics that are well reflected by the metatheoretical assumptions of its underlying paradigms, we now face the question of why the field is able to *sustain* these logics without getting trapped in a crisis situation that accompanies rare revolutionary upheavals as described by Kuhn (1996). If strategy research manages to 'blackbox' many of its presuppositions into dominant logics, we have to discuss the underlying micropolitical dynamics that help to nourish conventional wisdom. In other words, we need to ask why such blackboxing is possible in the first place. By discussing these concerns, we supplement the more content-based arguments of the preceding sections with a discussion about why dominant logics exist anyway and how they are maintained. This discussion is necessary to understand the 'political' dimension of scientific practice on which some of our recommendations for future knowledge development in the area of strategic management rest (section 8.1).

To show how socio-cultural factors influence the development of belief structures, we discuss concepts stemming from the *sociology of scientific knowledge* and the *sociology of science* and apply them to strategic management. While the sociology of science is concerned with science as an institution, the sociology of scientific knowledge addresses the process of intellectual legitimization that we define as "the process by which a theory becomes recognized as a part of a field – as something that cannot be ignored by those who define themselves, and are defined, as legitimate par-

ticipants in the construction of a cognitive field." (Lamont 1987: 586). Legitimized theories are often referred to as '*facts*', that is a piece of knowledge which has been collectively stabilized from the midst of scientific controversies and is strongly confirmed by later scientific contributions (Latour 2002: 42). Overall, there is widespread agreement that knowledge can be shown to be socially constructed within scientific controversies. Starting from this social constructivist perspective, we discuss the production (section 3.3.1) and perpetuation (section 3.3.2) of facts in strategy research to finally illustrate how facts that are sustained for a long period of time are transformed into dominant logics (section 3.3.3). We thus sympathize with the claim that there is no such thing as a natural scientific fact, a fact that is simply 'there' by the force of nature, because scientific knowledge in strategic management is the product of a lengthy process of construction. What we show in the following sections is that 'nature does not force the issue' (Pinch and Bijker 1984: 420), but that those arguments which are often referred to as facts are socially constructed by a network of diverse actors (e.g., scholars, editors, funding agencies) and do not appear from nowhere.

3.3.1 Producing Facts in Strategy Research

To open the black box of science, we need to consider the social mechanisms that curb or foster the incorporation of existing contributions into ongoing discourses. To understand *what* facts are, we have to understand *how* they are made and *who* made them in the first place. We must study science in the making and not ready-made science (Latour 2002). In studying the fabrication of scientific facts in strategic management, we follow researchers at the times and places where they do their work. As for most social scientists, a large part of the job is done behind the desk writing journal articles or books. It is thus reasonable to discuss the role of literature as an essential factor in the production of facts. Our concern is to explore how scholars succeed in producing a document that has the largest possible impact on the scientific community. As Law and Williams (1982: 537) argue, the impact of a paper or book depends on the *perceived* value that other researchers assign to one's own work. To be legitimized by others and to gain the status of a fact, a document needs to be reliable and relevant in the eyes of others. It is thus of fundamental importance how scholars present their 'product' to achieve maximum recognition.

To begin with, we have to recognize that the production of facts is a collective process. The point is that the fate of what a scholar says is in the later users' hands because the status of a statement depends on later state-

ments. By itself a statement is neither true nor false, neither a fact nor a fiction, it is made so by others, *later on* (Latour 2002: 29). It is made more of a certainty, and thus a fact, if others take it up and discuss it favorably. This again means that to produce facts in strategy research – and also in any other scientific field – one needs to gain credibility. Without credibility others will not refer to the research output one offers to the market of knowledge. We then need to ask: What enhances the scientific credibility of an article? What does an author do in order not to be ignored by the scientific community? If an article gets ignored, it cannot be turned into a fact. Even worse, it cannot be rejected; the voice of the scientist remains silent as if the article never existed.

Latour (2002), who has been at the forefront of studying the dynamics of science in the making, lists three factors that help to transform arguments into facts. First, there is the *argument from authority*. To impress a dissenter one needs prestige, most often the prestige of a major journal and/or the excellent reputation of a university or educational fund. Having these allies makes it hard for a dissenter to criticize a piece of literature. If one doubts the claim brought forward by the author, one also questions the honesty, good judgment, and hard work of a large number of well-established people (e.g., journal editors, professors that have supported the work of the author). Who wishes to have so many opponents? Concerning strategic management, we can state that an author who publishes in the *SMJ* definitely experiences positive spillover effects of the excellent reputation of the journal. MacMillan (1989: 391), after having identified the *SMJ* as the most outstanding forum for publication in strategy research by means of a survey among tenured professors, even claims that

> "each field of inquiry has a forum in which the work of scholars in that field should be presented, and if a candidate's work is accepted in that forum, then such work should be deemed scholarly."

To belong to the 'club', scholars need to publish in *the* forum, which in the case of strategic management is quite a limited one.

Even more interesting is a look at the affiliations of the authors of the 20 most cited documents in the *SMJ*. At the time their work was published, five authors (Porter, Rumelt, Andrews, Lawrence, Lorsch) were associated with Harvard University, while most other authors gained support from other top universities like MIT (Chandler), Yale University (Williamson [1985]), University of Pennsylvania (Williamson [1975]), Stanford University (Hofer), Northwestern University (Scherer), and University of California at Berkeley (Pfeffer). As a minimum, the financial strength and prestige of these institutions supported the success of these authors (e.g., through granting research funds). This argument is consistent with the em-

pirical findings cited in Park and Gordon (1996: 111) who report that scholars from prestigious universities were among the top contributors for high-rated journals (see also Boyd et al. 2005: 842; Hunt and Blair 1987: 200; Ofori-Dankwa and Julian 2005: 1315). A dissenter wishing to attack a contribution that gained authority from any of the abovementioned sources will face considerable opposition, even at the very beginning of this endeavor. An argument based on authority is what Pinch and Bijker (1984: 425) call rhetorical closure, that is, the introduction of a 'knockdown' argument by means of scientific authority to offer 'definite' proof of how things are, thereby closing a debate.

Following Latour (2002: 33), the second factor that influences the credibility of a scientific contribution is its *reference to former well-known texts*. The presence and absence of references is often regarded as a direct indicator of the seriousness of a paper. Without any references at all, the paper is 'naked'; without references to the major debates of a field the paper has few chances of being regarded as a legitimate contribution (Adatto and Cole 1981). The reason for including well-known references is rather simple: to attack a document that is loaded with literature means that the dissenter not only has to weaken the arguments of the author, but also the (legitimized) arguments that the author refers to. Again, we see that a paper that is on its way to becoming a legitimate contribution to a scientific discourse can contribute from positive spillover effects provided by the existing prestige of former articles/books. It is easier to convince people by means of documents that they already know and trust than to persuade them of something very remote or even contrary to their current belief structures.

What does that mean for strategy research? Phelan et al. (2002: 1163) report that the number of references per *SMJ* article has considerably increased over the last two decades (from a mean of 25 per article in 1980 to a mean of 75 per article in 1999). Hence, there is reason to believe that in the strategy community the amount of literature clearly is an indicator of the seriousness of an article. In terms of reference to well-known texts, we can state that existing articles in the *SMJ* clearly give reference to the 'big names' of the field, as demonstrated by the citation index that was introduced in section 3.1. Surprisingly, most of the 20 strategic realities that make up the list of the most cited documents in the *SMJ* contain neither long lists of references nor much discussion of prestigious former texts (Porter's *Competitive Strategy* includes just four pages of bibliography). The latter may be due to the pioneering character that most of the strategic realities had for the (back then) still developing field of strategy. Chandler's (1962) contribution, for instance, refers often to federal documents

and statistics. This changes if we look at the few more recent documents that belong to the list of the 20 most popular strategic realities. Barney's (1991) article, for example, gives reference to a lot of well-known pieces of work like Andrews (1971), Ansoff (1987b), Hofer and Schendel (1978), Nelson and Winter (1982), Penrose (1995), Porter (1980, 1985), Scherer (1980), Wernerfelt (1984). As strategic management reaches maturity, both the number of references and the citation of well-known documents play a vital role in enhancing the credibility of strategic realities.

Latour's (2002) third factor that helps documents on their way to becoming facts is *being referred to by later texts*. The assertions that are made in the article need the support of later articles to make it more of a fact. In other words: arguments need to be continuously reproduced to become and stay a fact. This is the recursiveness of scientific practice; the probability of being treated favorably by subsequent articles increases if the respective text refers to already well-known pieces of work that in turn enhance their visibility and make it even more impossible to be evaded by future scholars. That is why Zimmerman (1989: 458), while offering advice on how to improve the likelihood of publication in major journals, advises authors to target the largest audience possible as 'marginal' opinions are not likely to be referred to by other scholars.

Once an author is referred to by later texts, her/his arguments become simplified. A few statements are isolated from the original text to be used over and over again. Repetition not only enhances the visibility of the author but also simplifies the initial argument. For instance, Porter's (1980) prominent idea of the structural analysis of industries, originally presented on 31 pages with in-depth discussions of the various determinants of the intensity of competition, is usually referred to in the following way:

> "Porter's (1980) Five Forces Model, by more clearly specifying the various aspects of an industry structure, provides a useful analytic tool to assess an industry's attractiveness and facilitates competitor analysis. The ability for a firm to gain competitive advantage […] rests mainly on how well it positions and differentiates itself in an industry. The collective effects of the five forces determine the ability of firms in an industry to make profits." (Hoskisson et al. 1999: 426)

> "Porter's […] five forces model is based on two arguments: (1) industry structure determines the nature of competition in an industry, and (2) the nature of competition is a major determinant of firm profitability." (Hill and Deeds 1996: 429)

Porter's argument becomes simplified and is thus easily accessible to a wide range of scholars. The Five Forces Model turns into a fact, something

that is not questioned anymore and can be turned into a one-line statement ('Porter has shown that …'). Of course, most journal articles are written under consideration of space constraints that do not allow for an in-depth discussion or even repetition of original contributions. Yet, once an author is being referred to by later texts and her/his arguments become simplified and thus accessible to a wider audience, a new fact is born.

From our anatomy of scientific rhetoric, we conclude that there are many supportive mechanisms an author can rely on when trying to gain credibility from the reader. Such mechanisms influence the process through which peers come to define a theory and its producer as important and give rise to what are called facts – knowledge that is little disputed in a discourse field. We have shown how the strategy field has produced facts by making use of these mechanisms. It is important to recognize that there is nothing wrong with facts, as they exist in every (mature) discourse field. For strategy scholars, the Five Forces Model is the Four-P approach to the marketing researcher: collectively stabilized knowledge claims that are confirmed by later scientific contributions.[49]

3.3.2 Sustaining Facts in Strategy Research

The term 'fact' already symbolizes stickiness. Nonetheless, one may wonder why we cannot simply break out of this vicious circle? Why not simply write an article that does not cite any facts at all, an article that just contains one's own thoughts? First, such an article would hardly pass through any double-blind review process in any of the major journals that are considered to be a 'good outlet for research'. Second, since the allocation of research funds and tenure-policy is mostly tied to scholars' list of publications, there is strong pressure to publish in high-ranked journals. To become 'somebody' in a discourse one needs highly recognized publications. Yet, to publish in a well-established journal one needs to reference the facts of the field. Communication needs to be accessible by others (Luhmann 1994: 198). Breaking out of this circle is possible as the list of critical publications, which can be found in almost every discourse, re-

[49] The creation of facts is also discussed by the literature on management fashions (Abrahamson 1996; Kieser 1996; Miller and Hartwick 2002). Yet, whereas the literature on management fashions deals with publications that are supposed to *address practitioners* (e.g., 'Total Quality Management'), Latour's (2002) remarks focus on the stabilization of facts *within the scientific community*. Management fashions have a mass appeal even outside the scientific community (Abrahamson 1996), while scientific facts usually are only discussed in the scholarly community.

veals. But, it takes more than a few articles to challenge existing facts. Then, the question is: How do existing facts withstand the assaults of dissenters?

The Perpetuation of Facts and the Matthew Effect

A good way to enter the discussion is to acknowledge Merton's (1968, 1988) work on the so-called *Matthew effect* which asserts that science is a 'sticky business' because famous people tend to become ever more famous. Greater increments of recognition for scientific contributions are assigned to scientists with considerable repute, whereas scholars who have not made their mark yet are often neglected or rendered less visible. As a consequence the credit for scientific work is often misallocated because if *similar* research findings are communicated by a well-known researcher and by one who is less widely known, it is the first who usually receives the prime recognition. Merton (1968: 59) concludes from this that scholarly contributions have a greater influence and visibility when a scientist of high rank introduces them. The Matthew effect demonstrates that science is not a private, but a public business and that the prior repute of researchers advances the speed of diffusion of their contributions. As a result, particularistic (judged on the basis of certain personal or social attributes) criteria operate as the basis for judging the dissemination of a scientist's work (Hunt and Blair 1987: 194).

According to the Matthew effect, scientific practice is based on a self-conforming process that curbs the advancement of knowledge in a field of research. Well-known scholars sustain 'their' facts because their statements are better recognized by other scholars and become the premise upon which other researchers build their work. Arguments that are introduced by prestigious scholars are more likely to become a fact and keep their status *over a longer period of time*. That is why the 'second book' of an author tends to profit from the success of the first one and, because it often builds on the premises of the first one, sustains already existing facts. If scholar X writes (for various reasons) a very successful book that introduces the knowledge claim Y which becomes a fact, the second book of scholar X, even though different in focus, is often based upon the basic premises that were introduced in the first book. Hence, by writing the second book scholar X not only introduces a new fact, knowledge claim Z, to the field because of her/his prior success, but at the same time enhances the credibility of knowledge claim Y.

In strategy research we can examine the Matthew effect when looking at the extended list of the most cited documents displaying the 50 most influential documents (Ramos-Rodriguez and Ruiz-Navarro 2004: 989). For in-

stance, after the rapid success of Porter's (1980) trail-blazing book *Competitive Strategy*, which ranks first in the list, it is not too surprising that the successor *Competitive Advantage* (Porter 1985) ranks third. Overall, Porter appears four times in the list. Similarly, Rumelt's (1974) very influential book *Strategy, Structure, and Economic Performance*, which ranks second in the list, paved the way for the success of his subsequent writings (Rumelt 1982, 1984, 1991) all of which rank among the top 50 documents. Williamson even appears twice in the top 20 documents. The prime recognition of *Markets and Hierarchies* (Williamson 1975) enhanced Williamson's visibility and therefore legitimized the success of *The Economic Institutions of Capitalism* (Williamson 1985).

All of this is *not* to claim that the works of these authors do not deserve to be in the list because the Matthew effect supported their success. We are well aware that Porter's, Rumelt's, and Williamson's contributions to the strategy field represent much needed and innovative discussions that have inspired and enriched the work of other researchers. Nevertheless, the Matthew effect demonstrates that the repeated success of scholarly contributions can be ascribed (at least partly) to her/his eminent status and existing visibility. Bergh et al. (2006: 92) support this claim. In their empirical study of *SMJ* article impact, they find that when citing a document authors pay most attention to the author and whether s(he) has already published other influential articles and/or books. They also find in their citation analysis that "if a scholar's initial work has had little impact, then the impact of their later work seems likely to suffer from the same fate." (Bergh et al. 2006: 92) To conclude, the Matthew effect demonstrates that scientific success is path dependent; the prior repute of scholars helps to *sustain* already existing facts in strategic management and thus makes a critical assessment of these knowledge claims virtually impossible.

The Perpetuation of Facts and the Institutional Environment

The perpetuation of facts can also be discussed from a different, more institutional, angle. For this, we need to assess the nature of scientific discourses. Fuchs and Ward (1994a, 1994b) distinguish between 'conversational' and 'factual' fields of scientific practice. As opposed to *conversational fields,* in which scholars observe one another to see the blind spots of argumentation, constantly reread the 'classics' everyone refers to, and favor criticism over explanation, *factual fields* are more closely coupled, have stronger resources, and professional networks to produce 'objective' knowledge. In factual fields the theoretical blind spots are protected and rendered more or less invisible because the basic facts the field is built upon are not seen as constructions, but as the way reality is or at

least as approaching truth. Factual fields rely on strong institutional pressures for conformity and place rather tight restrictions on the amount of tolerated skepticism. Both types of fields produce facts. Yet, while conversational fields constantly challenge the knowledge claims that have become rigid, factual fields sustain these claims and debate their legitimacy only sporadically. As a consequence, factual fields show much homogeneity in their intellectual structure.

From our perspective, strategic management possesses the characteristics of a factual field of inquiry. We can observe the homogeneity that characterizes a factual field when looking at the professional network association, the *Strategic Management Society* (SMS), which is tightly coupled and allows for little heterogeneity in terms of its intellectual strategy. The SMS provides a kind of 'intellectual umbrella' for strategy scholars ever since its foundation in 1981. Meyer (1991: 830) even claims that "the Strategic Management Society is our only professional organization whose meetings are regularly attended by academics, consultants, and CEOs." The SMS is internationalized with members from over 50 countries: however, there are no local or regional sub-groups. As opposed to other fields (e.g., organization theory), there are few other *internationally recognized* professional associations that address strategic management.[50] Because of the missing regional differentiation and the resulting homogeneous character, strategic management possesses no regional research agendas as other disciplines do.[51] The missing regional differentiation of the SMS is plastered by the strong representation of articles by North American scholars in the *SMJ* (the SMS's official research outlet). When looking at the number of articles by source of authorship, the amount of American-only authored articles increased dramatically between 1980 and 1999. Out of a total of 994 articles for this time period, 724 were solely authored by American scholars (Phelan et al. 2002: 1164). In addition, out of the 125 editors for the journal, 100 hold appointments at North American universities. This raises the question of why the dominance of North American authors has not caused the development of a distinct European professional network with a related research agenda?

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[50] As possible alternative, internationally recognized networks we may note the *Business Policy and Strategy Division* of the *Academy of Management* (*http://www.bpsdivision-at-aomconference.org*) and the UK-based *Strategic Planning Society* (*http://www.sps.org.uk).*

[51] See, for instance, Pilkington and Liston-Heyes (1999) who show that production management possesses a European and American research agenda. Their argument is based upon a citation analysis demonstrating that European and American authors in production management cite from different sub-fields in the literature.

Knyphausen-Aufseß (1995: 263) offers an interesting discussion in this context. He investigates whether there is a difference between North American and European strategy research and concludes that even though European scholars possess a distinct theoretical research tradition at a *general level*, they still import a great deal of 'mainstream' American research. A regionalized European research agenda is, at best, a project for the future. We are thus confronted with a self-energizing logic: Europeans import much of the American research agenda because it is widely accepted and in doing so miss the opportunity to build up their own research tradition in strategic management. The absence of their own research agenda in turn fosters the adoption of the existing American mainstream. Ultimately, this brings up the question of why the American mainstream is legitimized among European scholars who supposedly possess their own theoretical tradition at a general level, as Knyphausen-Aufseß (1995) argues? One plausible answer is that the increasing globalization of science and scholarship, Schott (1993) even speaks about 'world science', concurrently fosters its Americanization (see also Kieser 2004). With regard to Australia, Clegg et al. (2000: 109) argue that research there "is merely another field experiment in the global laboratory of universalizing US management theory." And Hamm (2005: 16-17) remarks that

> "[i]n Western science, the peak of the pyramid is usually assumed to be science in the US. Western scientists are supposed to follow rather closely what happens in their field in the US, have close connections to their US colleagues and derive many of their hypotheses from the US literature (which would give them at least a tiny little chance to get their own writings published in the US). […] Those who do not follow the US literature closely enough are seen as parochial. […] What is pushed to prominence in US social science is defining the mainstream."

This is a tough accusation that does not hold for *every* discourse in the social sciences. Nevertheless, for reasons discussed above there is evidence that strategy research not only has its roots in America but over time has also Americanized scholars from other parts of the world. Strategy research is still marked by its birth in mid-20th century corporate America (Whittington 2004: 63).

To further explain the factual nature of the strategy discourse, we also need to take a closer look at the *SMJ*. To speak only of American mainstream research may be oversimplifying. What do we mean by this when we refer to the *SMJ*? To gain a better understanding of what is considered scientifically desirable for publication in the *SMJ*, we should take a look at the journal's editorial policy. Schendel, who has been the editor-in-chief

ever since the foundation of the journal in 1980, outlines that policy as follows:

> "More empirical research is needed in the field. We do not want for theories, but we do want for theories that have been adequately tested against empirical data […] future research should, wherever possible, be normative in character […] future research should be more rigorous." (Schendel and Hofer 1979: 394)

Not surprisingly this editorial policy has been progressively implemented over the past 25 years leading to an increase in empirical articles by a 7:1 ratio compared to non-empirical work.[52] Phelan et al. (2002: 1167) conclude their assessment of the first 20 years of the *SMJ* with the recognition "that it has become harder to make a competitive contribution with this [conceptual] type of paper." (annotation added, see also Bergh et al. 2006: 82) While officially introducing the editorial policy of the *SMJ*, Schendel et al. (1980) argue that a paper acceptable to the journal should contain material that has been tested, or at least is testable, in order to come up with necessary generalizations. Scholars who wish their work to become published in the *SMJ* need to comply with this type of research in order to get around the editors who have the function of 'gatekeepers' (Astley 1985: 508). In consequence, Heugens and Mol (2005: 122) report that existing methodological orthodoxy has a strong bias towards a specialization on quantitative work in US strategy departments and Bergh et al. (2006:

[52] Not all empirical work inevitably fosters positivistic assumptions. There is need to distinguish between qualitative and quantitative data analysis. Whereas qualitative work often relies on single case studies that are investigated by means of different methodologies (e.g., ethnomethodology), quantitative work is based on larger sample sizes and mostly quantitative modeling. Using the number of samples as an indicator for quantitative and qualitative work, we can claim that *SMJ*-content has been largely influenced by quantitative studies of larger sample size. Phelan et al. (2002: 1166) find that *SMJ*-authors using primary data utilize 175 cases on average while authors who rely on secondary data use an average of 1250 cases per study. These findings are in line with Lampel and Shapira's (1995) claim that strategic management research is dominated by large database research (for some critical arguments on the use of statistical modeling see the discussion by Mingers 2003). Shook et al. (2003: 1233) report that general linear models remain the most dominant data analytic technique used by *SMJ*-authors. The issue of empirical versus non-empirical research pops up every once in a while in discussions about and in the *SMJ*. Bartlett and Ghoshal (1991: 7) argue that strategy research tends to have a too narrow perspective dominated by economical methodology. They claim that the field "has become narrower […] in its search for stronger and more rigorous theory." In a direct reply, Schendel (1991: 2) *criticizes their critique* for painting "a narrow and temporarily limited view of the development of the strategy field."

91) assert that articles with a high level of 'methodological rigor' receive more recognition in the strategy discourse.

It goes without saying that with such an editorial policy in place there is not much room to challenge deeply held assumptions. Strategy researchers, at least in the *SMJ*, have not taken much care to reflect on the intrinsic subjectivity of social relations. It seems that Starbuck's (1982: 8) early appeal to be *slow and reluctant* in claiming to have approximated true objectivity and clear cause-and-effect relations went unnoticed by the strategy field. In addition, it needs to be noted that *SMJ*-articles contain an increasing level of intrajournal citation. Phelan et al. (2002: 1163) find that by 1999 the average *SMJ*-article is citing 10 previous *SMJ*-articles and that *SMJ*-articles comprised about 15% of all references in a published article. This makes it even harder to challenge the deadlocked assumptions inherent to so much, mostly quantitative, empirical work.

Our discussion demonstrates that strategic management possesses the characteristics of a factual field of inquiry; the tightly organized professional network (SMS) together with its associated journal (*SMJ*) support the homogenous nature of the field. The SMS and *SMJ* are dominant (compared to other networks and journals) and dominated (by an empirically-focused research policy). This is *not* to say that the SMS is a superfluous institution or that the *SMJ* is a journal not worth reading, but that their tightly controlled character is a factor that should not go unnoticed when asking why the field has been able to sustain its facts for a long time.[53] Intellectual homogeneity does not imply that everybody necessarily agrees with the established assumptions or that all scholars share the same opinion about what strategy is or should be, but that strategic management is built upon a set of intellectual presuppositions that are little debated in the community. Singh et al. (2003) even call for more replication of empirical studies to protect against the naïve acceptance of research results. Despite our doubts that a duplication of empirical studies can do any good to improve the reliability of research results – because each study is situated in a specific context that influences its outcomes and a duplication of contexts is basically impossible – it is more dangerous to have an unquestioned answer than an unanswered question.

[53] Pettigrew et al. (2002: 6) put it in a nutshell: "Both SMJ and SMS have had their critics and there have been periodic questions about the lack of critical reflection and narrowness of the epistemological, methodological, and theoretical base of writing in the field of strategic management. It is now common to talk of the post-Porter era in strategy, perhaps as we shall see the more general changes in epistemological and theoretical discourse in the social sciences at the beginning of the 21st century […] will collectively push the field of strategy and management in some fruitful new directions?"

In this section we have argued that the perpetuation of facts in strategy research depends on the factual nature of the field fostered by the institutional settings that define what counts as scientifically desirable and the dynamics of the Matthew effect. As any other discourse, strategic management possesses institutions (professional associations and journals) that legitimize scholarly work and define what counts as important. Yet, as opposed to other discourses, strategic management represents a factual rather than a conversational field.[54] This is because the tightly organized nature of its professional network and the narrow editorial policy of its major journal foster a high degree of homogeneity among scholars. In factual fields, the continuous legitimization of facts depends on their 'quality' that is judged in accordance with established knowledge claims that are maintained by a tightly coupled network of scholars. All of this points to an irony: existing facts are made stronger through 'weak' thinking, that is, the adherence to existing rules while playing the game (Booth 1998: 9). We are not suggesting that path dependency in research is necessarily a bad thing; indeed, all scientific research is in a way path dependent (Kieser and Nicolai 2005) – but that in strategic management research this dependency has created a situation where it is hard to unlock the dominant path to eventually create alternative ones (Garud and Karnoe 2003).

3.3.3 From Facts to Dominant Logics

Having discussed the production (section 3.3.1) and perpetuation (section 3.3.2) of facts in strategy research, it is time to ask how facts relate to the notion of dominant logics. We argue that under certain conditions sustained facts can be transformed into dominant logics. For this it is necessary to realize that dominant logics are concerned with the *assumptions* that are attached to facts. Therefore, we have to distinguish a controversy *about facts* from one about the *assumptions that underlie facts*. Facts are

[54] Compare for instance the international 'business ethics' discourse that has no predominant association, relies on a variety of high-ranked journals (e.g., *Business Ethics Quarterly* or the *Journal of Business Ethics*) and is much concerned with discussing and questioning its underlying assumptions (Spence 1998). Hence, business ethics can be described as a conversational field of research. Although there are certain facts (viz. legitimized pieces of knowledge), for instance the idea of stakeholder dialogues, the assumptions that are attached to these facts are heavily debated by the community. Mitchell et al. (1997), for example, suggest that it is not self-evident that managers perceive the attributes that enable a classification of stakeholders in the same way and thus started a debate about epistemological issues in stakeholder theory.

not dominant logics, but the guiding assumptions of an array of sustained facts can be turned into a dominant logic if the assumptions remain unquestioned. Certainly, this is most likely to happen in a factual field of research where existing institutional pressures prevent reflection. Facts and dominant logics are different conceptions and to question a fact does not always mean to question a dominant logic. For instance, we can criticize the well-established fact that there are five forces that determine competitiveness in an industry. Freeman (1983) suggests that one can easily find more forces (e.g., the power of stakeholders). Yet, to question this fact does not inevitably mean to question the underlying assumptions that 'shape' the fact (e.g., that the environment is 'given'). Without doubt, there are many critical pieces of work that challenge certain facts: however, there is rarely a discussion of their underlying assumptions (i.e. the attached dominant logics).

Referring back to our distinction between conversational and factual fields of inquiry, it can be claimed that factual fields sustain existing facts, or at best challenge them sporadically, whereas conversational fields not only question the facts themselves but also their underlying assumptions. As a result, factual discourse fields transform facts into dominant logics by not questioning their underlying assumptions, while conversational fields continuously debate the underlying research logic and thus inhibit the rise of dominant logics (Fuchs and Ward 1994a; Figure 13).

	Facts	Dominant Logics
Conversational Discourse Field	Constantly challenges facts	Constantly challenges underlying assumptions and thus inhibits rise of dominant logics
Factual Discourse Field	Sustains facts - challenges come only sporadically	Does not challenge underlying assumptions and thus builds up dominant logics

Fig. 13. The Relation Between Discourses, Facts, and Dominant Logics

Only if facts are sustained for *quite some time* do they provide input for a dominant logic. It takes a lot of time and numerous facts with *similar assumptions* to create a dominant logic. For instance, even though Ansoff's (1987b) and Andrews's (1971) works explicitly rely on a linear notion of

strategy, the related dominant logic (the 'primacy of thinking') did not immediately appear with the publication of their work. It needed other publications that reproduced the fact (Hofer and Schendel 1978; Lorange 1980; Shrivastava and Grant 1985) for the dominant logic to slowly, almost unnoticeably, creep into the discourse. The *fact* – that a strategy process is linear – was even challenged from various angles. Some claimed that the political dimension is neglected (Pettigrew 1977), while others highlighted the importance of conceptualizing the strategy process in more incremental ways (Quinn 1980). Nevertheless, the *dominant logic* that thinking comes before action, which is not bound to the strategy process but a very basic assumption about the causality of life, has not been challenged within the mainstream.

To conclude, dominant logics emerge from scientific facts that are sustained for a long period of time. Most importantly, in producing dominant logics, facts are not only sustained but their deeply held assumptions are not put into question. This is what Bachelard (1987: 47-50) criticizes when he argues that dominant logics hinder scientific progress because researchers become so used to them that discussing them is too inconvenient or not perceived as useful. In this way, well-established intellectual habits are more and more appreciated not because of their intrinsic value but because of their repeated, unreflective use. As a consequence, assumptions that are attached to dominant logics are treated *as if* they were clear and straightforward and no alternatives existed, whereas in fact this clarity is a socially constructed misconception. The socially constructed process of knowledge creation and perpetuation is based on a complicated system of actors (e.g., supporters, opponents, editors), institutions (universities, professional networks, and profit as well as non-profit organizations), and rhetorical devices (citation of former texts, authority an article gains from the reputation of a journal). Such a complex interplay of different factors makes it hard to generalize and even harder to apply these issues to a specific scientific discourse like strategy (Lamont 1987: 601). Citations make up an almost endless network of papers and authors that is hard to unravel. Thus, not every established fact nor the success of every much-cited article can be explained by the factors introduced above; they simply provide a rough guide for navigating the 'jungle of science'. After all, *luck* may be a factor that is hard to account for.