

RESEARCH NOTES AND COMMUNICATIONS

THE ECONOMIC MODELING OF STRATEGY PROCESS: 'CLEAN MODELS' AND 'DIRTY HANDS'

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We argue that our model of learning in alliances (Khanna, Gulati and Nohria, 1998) is an economic model of strategy process. We discuss implications of this view for the strategy process vs. content debate, for the appropriate testing of models of strategy process, and for the role of economics in helping understand strategy process. We propose that the 'clean models' from economics and 'dirty hands' of traditional process inquiries offer research designs that are complementary rather than incompatible (Hirsch, Michaels and Friedman, 1987). Copyright © 2000 John Wiley & Sons, Ltd.

In this paper, we explain why we think of our earlier model of the dynamics of learning alliances as an economic model of strategy process (Khanna, Gulati, and Nohria, 1998; hereafter, KGN). By using the KGN model as an exemplar, we hope to highlight the complementary role that economic reasoning can play in traditional strategy process research. We also revisit the larger issue of the traditional separation of strategy content and strategy process research. Finally, the latter half of the paper draws attention to several general issues that model-building efforts such as ours must confront.

Revisiting the logic of the KGN model of learning in alliances

In KGN we modeled the dynamics of the learning behavior of firms in alliances, as opposed to the more frequently studied decisions of firms to enter into alliances. We focused specifically on modeling how sequences of events unfold as each partner's learning progresses. In particular, we sought to understand how and why alliances frequently combine elements of both cooperation and competition between partners, and to identify conditions under which competitive 'racing' would be the predominant form of behavior by each partner. Such racing behavior involves each firm trying to learn as much as it can from its alliance partner in an attempt to preempt a situation in which the other is the first to sufficiently complete its learning and terminate its involvement in the alliance. When such racing

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behavior occurs alliances might terminate before all firms have learned the maximal amount from their partners and thus appear to external observers to be, in a sense, unstable.¹

To understand the determinants of racing behavior KGN used the terminology of private and common benefits. As argued in KGN, private benefits are those that a firm can earn unilaterally by picking up skills from its partner and applying them to its own operations in areas unrelated to alliance activities. Common benefits are those that accrue to each partner in an alliance from the collective application of the learning that occurs as a consequence of being part of the alliance. Common benefits are thus obtained by areas of the firm that are part of the alliance. Generalizing beyond two-firm alliances, one can think of private benefits as being those that exclude some participants (in which case they are 'private' to the recipient firms) and common benefits as those that do not have any such exclusionary property. Most realized benefit streams to firms in an alliance can be thought of as being composed of combinations of private and common benefits.

The distinction between the two benefit streams has important analytical consequences. Consider two-firm alliances composed purely of *private benefits* wherein each partner expects to get *something* from the other, but what each partner gets does not accrue to the other. KGN argue that the outcomes of such alliances are likely to be biased towards cut-throat competition, with much racing behavior. On the other hand, alliances characterized by the complete absence of such exclusionary private benefits, wherein all participants share some common benefit stream, are likely to be characterized by firms cooperating in an effort to realize the *common benefits* and, thus, by an absence of racing behavior. The model specifies an explicitly dynamic learning process in which firms constantly reevaluate the appropriate extent of investment in learning and update their investment decisions as a function of their past investment decisions and of the resolution of uncertainty in their own, and their partners', learning processes.²

Inkpen (2000) has made several useful observations regarding KGN. We welcome the opportunity to comment on his misinterpretation of our central notion of common benefits. Inkpen (2000) says that the 'collective learning' that underpins common benefits 'primarily involves learning about the partner in a manner that enables more efficient cooperation.' This is *not* what KGN say or imply. Our focus is not so much on how firms learn to learn from one another; rather we define common benefits simply to be those that emerge from within the targeted scope of the alliance. This is more than learning to learn from each other.³ The distinction that KGN make is between 'learning with' a partner to earn common benefits and 'learning from' a partner to earn private benefits.⁴

In addition to outlining the implications of these different kinds of benefit streams, KGN sought to specify an empirically verifiable construct that would enable an understanding of some of the conditions under which the mix of private and common benefits might vary and result in greater or lesser racing behavior. The 'relative scope' construct was intended to allow future scholars to subject the model's claims to empirical verification. Relative scope of a firm in a particular alliance is defined as the ratio of the scope of the alliance to the overall scope of the firm. By definition, the relative scope measure lies between 0 and 1. The smaller the scope of the alliance (i.e., the fewer the markets targeted by the alliance, the closer its value to 0. Its value is exactly 1 if the firm has no interests in markets not targeted by the alliance.

KGN demonstrate that the relative scope measure is proportional to the ratio of private to common benefits. Intuitively, the smaller the relative scope measure, the less opportunity for the firm to earn private benefits by applying what it has learned to areas outside the scope of the alliance relative to the opportunity to earn common benefits from within the scope of the alliance. Thus, the temptation to race—triggered by the prospect of earning private benefits and

¹ This, of course, raises the issue of why firms enter alliances. Khanna (1996, 1998) demonstrates that even though one could envision such an outcome for certain alliances at the very outset, in an *ex ante* expected value sense it might be perfectly rational for all firms to consent to participate in the alliance.

² Game theory, which is the disciplinary paradigm out of which the KGN model emerges, is ideally suited to modeling

interdependence among small numbers of players (Ghemawat, 1997), as in a prototypical alliance. Other disciplines have, of course, also studied mutual interdependence situations extensively (see, for example, Weick, 1979, Ch. 4).

³ See Anand and Khanna (2000) for an empirical analysis of how and when firms learn to use alliances to learn from their partners.

⁴ We thank Peter Ring for helping us clarify this point.

by the threat of being locked out of getting any benefits if the partner is the first to earn private benefits and terminate the alliance—is increasingly balanced by the incentive to cooperate in the earning of common benefits as relative scope approaches 1. The model thus allows us to relate, in a parsimonious and empirically falsifiable way, an observable and measurable characteristic of an alliance, the relative scope measure, to an observable and measurable outcome of an alliance, the extent to which alliance managers report the presence of competitive behavior.

Economic models of strategy process

Inkpen's (2000) primary critique is that the KGN model of learning in alliances pays inadequate attention to process considerations. Our model, however, is precisely one of strategy process, albeit logically deduced from economic first principles of rational, profit-maximizing behavior rather than inductively derived from field observations.

We are certainly not the first to offer a theory of strategy process. To the best of our knowledge, however, our model is the first attempt to try to understand the strategy process through formal economic considerations. We consider KGN to be a model of strategy process for several reasons. Our view of process is similar in spirit to that articulated by Pettigrew (1992), who emphasizes the 'sequence of events that describe how things change over time.'⁵ Further, our model explicitly addresses one of the three categories of questions that form the domain of strategy process research as articulated by Chakravarthy and Doz (1992: 6), namely: 'How does [a firm] modify its ... decision processes both in response to environmental changes and through its own proactive actions?' KGN focus on the decision process used by each alliance partner to determine the extent of the resources it allocates to learning and dem-

onstrate the interrelated nature of partner firms' decision processes. Of course, it is emphatically not our intent to imply that the stylized processes which we model are the only, or even the most important, descriptors of the process of learning in the alliance. But we do intend the approach to be illustrative of how one should think of the economics of different processes.

Finally, the KGN model satisfies an important methodological criterion for strategy process research, that of having to peer within the organization's 'black box' to determine how these decision processes play out as learning evolves.⁶ It is important to note that our focus on interfirm alliances is distinct from that of the majority of process studies, which are specifically intrafirm.⁷

The KGN model seems nevertheless to also fall into the 'strategy content' stream of work by some criteria articulated by Chakravarthy and Doz (1992). For example, it draws primarily on (a branch of) microeconomic theory as a disciplinary base. The dual content-process nature of the model leads us to question the traditional sharp distinction between strategy content and strategy process research. Put simply, we find it difficult to answer the 'what to do' question (the traditional domain of strategy content research) without an appropriate understanding of the costs and benefits of different approaches to 'how to do it' (the traditional domain of strategy process research). Thus, in the KGN model managers in a partner firm cannot possibly determine whether to allocate resources to learning in a manner consistent with racing to learn without having some idea of how the sequence of learning events appears to be unfolding in the alliance in question. The decision to allocate resources to learning is in this case a 'content' issue, albeit made only with a recognition of how it influences, and is influenced by, the learning processes. As Schendel (1992: 3) puts it, 'the "how" of gaining the position is inextricably linked to the "what" of position.'

In a somewhat different context, DiMaggio (1995) draws a related distinction between 'covering law theories,' the primary purpose of which is to explain the variance in some phenomenon

⁵ Although there are other kinds of theories of strategy process, we believe that the one on which Pettigrew (1992) focuses is most commonly identified with 'process.' Van de Ven (1992) offers an excellent taxonomy that distinguishes four kinds of theories of strategy process: life cycle, teleological, dialectic, and evolutionary. Within his categorization, ours is closest to a teleological theory of strategy process, based on the assumption that each firm in the alliance is a purposeful and adaptive actor that chooses from alternative courses of action to move towards the espoused goal of learning from its partner.

⁶ A corollary is that nonpublic data sources are likely to be especially useful in testing such models.

⁷ Doz (1996) and Ariño and de la Torre (1998) are recent exceptions.

without focusing on the processes that cause the variance, and process theories wherein the focus is on the narrative of the sequence of events. As DiMaggio asserts, however, the distinction is far from clear-cut and good theory often combines various aspects of explaining variance as well as highlighting process. Indeed, as Sutton and Staw (1995) and DiMaggio (1995: 392) point out, 'process theories can creep towards variance theories,' as, for example, if we consider that a variance theory can be formulated to explain dependent variables that are, in effect, summary statistics for the processes in question. There is much similarity between the relationship between DiMaggio's variance and process theories, on the one hand, and the relationship between strategy content and strategy process theories, on the other.

To see this consider, by way of illustration, how one might gather 'content' data to test a 'process' theory of learning in alliances. Imagine gathering the following data on the learning process for a snapshot of each of a cross-section of alliances: resource commitments made by the various partners in the alliance; measures of the private and common benefits which accrue to each alliance partner; measures of relative scope; elapsed time from the initiation of the alliance; each firm's perception of its partners' commitment to the alliance; and each firm's perception of the amount of cooperation in the alliance. These economic and behavioral data are summary descriptors, albeit imperfect, of the learning process; we would have been less inclined to collect these data absent our economic model of strategy process. The variance across such data can be used to test the predictions of the KGN model about the likely behavior of partners and the outcome of the alliance. Thus, cross-sectional variation in summary descriptors of the learning process can be tested for consistency with the predictions of our economic model of strategy process.

In addition to addressing the debate between strategy content and strategy process, there is a related conventional wisdom that we wish to question. This concerns the acceptance of (the absence of) a role for economics in strategy process research. As Hirsch, Friedman, and Koza (1988: 91) point out, "'Economics' market-oriented approach is commendable when analyzing the content of broad goals to be set for the corporation—i.e., in the policy 'formulation' stage. There, it is appropriate to take an idealized

view of organizations and markets, if only to conduct thought exercises and focus managers' attention on market forces. But to do so without an awareness of the processes through which policy is developed and implemented is naïve." We agree that a thorough content analysis has to take process realities into account. But the notion of content and process as mutually exclusive and assertion that economic modeling will not permit an understanding of process issues may not be entirely true. For example, an extension to the KGN model (Khanna, 1996, 1998) uses economic modeling to show that partner firms factor into the decision to enter into an alliance the possibility of the alliance's premature termination. Thus, each firm recognizes that the learning process may well be such that it may earn neither private nor common benefits if its partner chooses to terminate its involvement in the alliance after becoming the first to earn private benefits. Such a pessimistic outcome may be adequately outweighed by an optimistic scenario in which the firm in question is either the first to earn private benefits or is able, along with the partner firm, to earn common benefits. In an *ex ante* sense, therefore, the expected outcome may be positive, even after factoring in the real possibility of a scenario in which a firm may earn nothing for its learning labors. This provides an example of a situation wherein the 'content' decision of whether or not to participate in an alliance is very much contingent on the recognition of the 'process' realities of how the alliance might unfold. It is also an example of how economic modeling of the unfolding of process helps to shed light on the content decision.

Since we believe the utility of modeling by strategy scholars to complement field observations, we believe it is useful to discuss several issues that such an exercise raises. We hope that the subsequent comments will provide a small step in encouraging discussion regarding the economic modeling of strategy process. We also intend to be responsive to Inkpen's (2000) observations regarding KGN in the course of these general comments.

Models as benchmarks

Our model posits a benchmark for learning behavior against which actual instances of learn-

ing behavior can be evaluated. Without a measurable benchmark (which we offer), a theory cannot be subjected to the test of empirical falsifiability (Popper, 1968). Indeed, Inkpen's (2000) opening paragraph compliments our discussion of suboptimal behavior in alliances; such a discussion of suboptimality is, of course, impossible without a benchmark optimum such as the one in our model.

Our benchmark is based on criteria of economic efficiency within a noncooperative game-theoretic modeling framework; that is, it specifies the best way for a firm to behave given the behavior of other firms and identifies an equilibrium whereby no firm can do better by unilaterally altering its behavior pattern. Certainly, there are numerous other possible theoretical paradigms within which benchmarks can be established.⁸ A prominent example is provided by Weber's model of bureaucracy. A more recent example of an alternative specification of a benchmark model is Ring and Van de Ven's (1994) theoretical investigation of the learning process in alliances that emphasizes the importance of equity as well as efficiency considerations. Bartlett and Ghoshal's (1993) study of multinationals emphasizes a managerial theory of the firm, which they claim rests on premises that are quite different from those emphasized by economics. Their examination of best and worst practices implicitly specifies a benchmark model for well-managed multinationals. Larger samples of the practice of multinational management can be tested for consistency with such a benchmark. Grounded field-based inductive theory has also been used to posit descriptive benchmarks of phenomena. Again, one can use summary indicators of larger samples of each phenomenon to test whether the descriptive benchmark is an accurate descriptor of reality.

The important point is that the model posited in KGN lends itself to empirical validation. This is not a trivial point. The existence of the school of thought that says that process models are particularly hard to test only implies that the ingenuity of the theorist is even more crucial. To

our mind, the best models are those that posit a simple benchmark for which it is possible to identify proxies (see Ghemawat, 1997) for creative attempts to establish benchmarks based primarily on non-cooperative game theory. Ultimately, whether or not the models from one paradigm are superior to those from others is, we believe, an empirical question.

Model structure

It is useful, at the outset, to recall Thorngate's (1976) notion of commensurate complexity, which reminds us that a theory cannot simultaneously be general, accurate, and simple. As Weick (1979: 35) puts it: 'The more general a simple theory is, for example, the less accurate it will be in predicting specifics.' Inkpen's assertion that our model does not account for every real-world 'finding' is neither one that dismays us, nor one with which we have any quarrel. Indeed, our attempt has been to optimize along the dimensions of generality and simplicity rather than accuracy. This in no way implies that we do not value models which place greater stress on accuracy, as, inevitably, do many of the conventional descriptive strategy process models.

To achieve our goals, we have had to simplify and abstract from unnecessary detail. As King, Keohane, and Verba (1994: 42–43) point out: 'Simplification has been an integral part of every known scholarly work—quantitative and qualitative, anthropological and economic, in the social sciences and in the natural and physical sciences—and will probably always be ... Systematic simplification is a crucial step to useful knowledge.'

Thus, the KGN model focuses attention on *some* aspects that the model-builders considered important. In order to operationalize the concept of relative scope in KGN, we sought a convenient representation of the scope of the firm. For illustrative purposes, we chose to represent firm scope using product scope and geographic scope axes, enabling each partner firm's activities to be represented by some area on the two-dimensional grid defined by these axes in the way clarified in KGN. It is also possible to represent alliance activity as (some portion of) the intersection of the areas of the partnering firms. This simple diagrammatic representation clarifies that some

⁸ Indeed, there continues to be active and ongoing debate about the extent to which rational choice models, such as those derived from game theory, capture the essence of organizational phenomena (Gibbons, 1999; Granovetter, 1999). Thus, Freeman (1999: 164) asserts that 'Foolishness and rational choice exist side by side in every organization.'

part of each firm's activities are within, and other parts outside, the alliance.

The desire for parsimony explains our decision to represent 'degree of overlap' using these axes and not some others that might be substantially more complex. Inkpen's assertion to the contrary notwithstanding, the use of these particular axes does not imply that we believe these to be the only dimensions worth considering. Using a more 'complete' characterization of overlap would not change the simple fact that some activities of the alliance partners are within, and others outside, of the alliance's targeted scope. The essence of private benefits is that they are earned outside the immediate domain of the alliance in question, whereas common benefits are earned within the alliance.

It may be useful for us to illustrate, following Inkpen (2000), how our model applies to the joint venture between General Motors (GM) and Toyota, termed the New United Motor Manufacturing Inc. (NUMMI). A careful application of the theory would direct attention to the several product-geographic markets in which GM and Toyota compete (e.g., luxury vehicles in the southwestern United States, luxury vehicles in the northeastern United States, and compact vehicles in the midwestern United States are all different product-geographic market combinations). NUMMI targeted production of vehicles for some of these markets (perhaps for compact or mid-sized vehicles in some part of the United States or for compact cars across all geographic regions within the United States). Benefits that GM and Toyota capture within these target markets are 'common.' Benefits that each can capture in non-targeted product-geographic markets are, in contrast, private to each of the firms. These may accrue to GM, for instance, because it learns something about Japanese automobile manufacturing techniques that it can then use elsewhere. Inkpen (2000) draws attention to NUMMI benefits that spread to other parts of GM and Toyota, but simultaneously asserts that GM's private benefits will be low. This is an incorrect use of our terminology and leads to a misapplication of our model. Using Inkpen's example, if GM Argentina's managers benefited from GM's learning in NUMMI, our terminology would suggest that this is one kind of private benefit that accrues to GM. It is precisely to capture such phenomena that we use the language of private benefits and common benefits.

As another explicit modeling decision consider the criticism that the KGN model is unrealistic because it does not model asymmetric alliance objectives (Inkpen, 2000). The point of the KGN model is precisely to explore the evolution of asymmetries as the dynamics of an alliance unfold, even when the alliance is between *ex ante* symmetric firms. Of course, it is trivial to generate *ex post* asymmetries if we assume up front that firms are different. If we demonstrated the emergence of *ex post* asymmetries in a model in which *ex ante* asymmetries existed, the reader would not be able to tell whether it was the dynamics of the learning process or the assumption of *ex ante* asymmetries that caused the asymmetries to arise. Thus, our assumption of *ex ante* symmetry does not mean that we did not consider that partners have symmetric objectives; rather it is a modeling assumption designed to clarify a particular point.

The robustness of our model to some issues raised by Inkpen has already been discussed in KGN and in Khanna (1998). Accordingly, we restrict ourselves to commenting only briefly on where most of these issues are already discussed:

- *Knowledge relatedness*: Although understanding barriers to information flows and know-how transfer was not our focus in KGN, we explicitly acknowledged the importance of understanding the nature of the knowledge being transferred and the partners' ability to absorb the knowledge (KGN, 1998: 196, 202).
- *Nature of the learning process*: KGN clearly discuss (KGN, 1998: 203–204) why the order of learning is not central to any of the key results. Inkpen further suggests that KGN err in representing learning as a 'tidy' process. However, we follow a tradition of economic theorizing in modeling learning as a stochastic process. This is *not* the same as saying that KGN are so naïve as to assume that the learning process is not fraught with varying degrees of complexity and ambiguity.
- *Dynamics of learning*: KGN's operating definition of 'dynamics' is the evolution of incentives to invest in learning. KGN do claim that Hamel's (1991) constructs are not helpful in understanding the evolution of these incentives. However, Inkpen's assertion that KGN do not find Hamel's concepts useful for understanding learning more generally is incorrect. Indeed,

KGN acknowledge the utility of Hamel's constructs in the main text of their article.

- *Alliance forms*: We certainly do not view KGN to be primarily about one form of alliance or another. To our mind, the language of private and common benefits can help us understand the effects of different governance structures (Anand and Galetovic, 1998; Gulati and Singh, 1998). For example, particular governance structures might facilitate the earning of private benefits, which will then affect the way learning proceeds in our model.

We remain open to the possibility that our conception of the alliance learning process will be proven, by systematic tests, to be flawed and that other dimensions will be proven to be worth incorporating. Other scholars might choose a different abstraction of the learning process than ours. After all, 'one person's multidimensionality is another's goulash; one author's focus another's crude reductionism' (DiMaggio, 1995). However, the determination of what features ought to be in a model and what ought not to be should follow from scientifically conducted tests. Inkpen's marshaling of field interviews that do not appear to be designed to test the KGN model as evidence against it is probably not the way we would proceed. We turn, therefore, to the important issue of model testing.

Testing the model

Inkpen (2000) concludes that because the NUMMI joint venture had high levels of private benefits, the KGN model predicts its instability. This distillation of a prediction from our model is inaccurate. What the KGN model suggests is that the *ratio* of private to common benefits is a factor that determines the stability or lack thereof of a venture. Further, the model can be modified to deal with a situation of repeated interactions (Khanna, 1998: 351–352), as was likely to have been the case between GM and Toyota.⁹ Absent an attempt to understand the relative magnitudes of private and common benefits for each of GM and Toyota, and absent an understanding of numerous exogenous events in the auto industry

that KGN do not explicitly model, it is perhaps premature to conclude that the KGN model does not illuminate this example.

This example raises an even more interesting issue. Should a model be disconfirmed by a single counter-example, a stringent test proposed by Popper (1968)? There continues to be debate regarding this point. Recently, some social scientists have argued that particular tests should neither confirm nor disconfirm a social science theory once and for all. King *et al.* point out that

the question is less whether, in some general sense, a theory is false or not—virtually every interesting social science theory has at least one observable implication that appears wrong—than *how much of the world the theory can help us explain* ... The process of trying to falsify theories in the social sciences is really one of searching for their bounds of applicability. (King *et al.*, 1994: 101; italics in original)¹⁰

It is indeed possible that a more nuanced analysis of the NUMMI case would suggest that the KGN model is an inappropriate descriptor of the evolution of that alliance. If this is the case, we feel that such an observation should be treated as lowering our priors that the KGN model is a sensible descriptor of reality and should be added to evidence accrued from other instances.

How, then, should the KGN model be tested? We can think of two different, not mutually exclusive, approaches. A series of case studies that collects longitudinal data on the evolution of alliances and focuses on constructs predicted to be useful by the KGN model could certainly be used to confirm or reject the model's validity. Such studies could further our understanding of factors beyond the ones we have identified as important and also extend our understanding of the boundary conditions under which our model might be appropriate. Alternatively, cross-sectional tests of particular implications of the KGN model could be constructed from large-sample data collected from public sources or from survey data. Indeed, we exercised great care to

⁹ For additional research on the implications of repeated interactions in alliances see Gulati (1995a).

¹⁰ We should add that one colleague who kindly commented on our paper preferred the view that a single counter-example can refute a model rather than King *et al.*'s (1994) position.

identify concrete, testable implications in KGN.¹¹ Some recent studies have picked up on this idea and find evidence that suggests that constructs similar in spirit to the idea of relative scope do explain important aspects of alliance behavior (Baum and Silverman, 1998; Baum, Calabrese, and Silverman, 2000). Our hope is that such systematic empirical tests will identify opportunities to refine and, indeed, correct the model and provide the sharper lens needed to return to the field to carry out further observations of process in alliances. Development of empirical operationalizations of the KGN model, or of any such model, are vitally important for this mode of inquiry to progress, lest it fall prey to the critique, leveled at rational choice theory in political science, that it formulates theoretical edifices without empirical foundations (Green and Shapiro, 1994).

The process of constructing an economic model of process

The process by which we arrived at the KGN model is perhaps instructive. Initial field interviews revealed to us several factors that affect whether or not a firm struck a cooperative or competitive posture *vis-à-vis* its alliance partner. A tangible outcome of initial research was our analysis of unilateral commitments (as opposed to jointly undertaken activities in an alliance), which we reported in Gulati *et al.* (1994). These observations, and others from fieldwork reported in Nohria and Garcia-Pont (1991) and Gulati (1995a), also formed the basis for the deductive reasoning, which culminated in KGN,¹² that we used to specify a benchmark against which our behavioral observations could be evaluated. Our intent was to relate observable features of the alliance (hence, the emphasis on relative scope in KGN), with theoretical justification for why the observable feature was important, to observable outcomes of the alliance (i.e., the extent to which

competitive behavior ensues in the alliance).¹³ We do not dispute the utility of the conventional approach of inducing descriptive models from field data. However, we found it worthwhile to subject the induced descriptive reality to the deductive logic of economic models and to subsequently use such models to guide further field observations. We are partial to the idea of the utility of such a dynamic interplay between field observations and economic models. The idea of theory as 'work-in-progress' (Weick, 1995) rather than finished product appeals to us as a descriptor of the early stage of model building embodied in KGN.

It should be clear that we do not subscribe to the view that either inductive or deductive research is inherently superior. Rather, we view the two as complementary. Our views are more in accord with Ghemawat's (1997) emphasis on the complementarity between 'low fat' deductive models and description than with Glaser and Strauss's (1967) assertion of the superiority of inductive work over what they refer to as 'logico-deductive theories.'¹⁴ It is simply not clear to us why inductive work should have a monopoly on understanding process issues. Although excellent inductive work has defined the strategy process field since the work of Bower (1970), Doz (1976), Burgelman (1983), and Haspeslagh (1983), we believe that the marginal returns to deductive work in this area are also likely to be very high.

Superimposed on this iterative process is the disciplinary interplay between economics and sociology that we have found to be fruitful in our

¹³ It is certainly possible that our judgement of the right factors to model is flawed and that other process considerations are indeed quite relevant to an economic model such as the one we construct. This is why we need well-operationalized tests of models rather than an empty juxtaposition of opinions, whether our own or those of others, for our understanding to expand.

¹⁴ Academia's assertion of the need for one or another form of research appears to vary over time. Glaser and Strauss (1967) were clearly reacting to their perception that sociology was dominated by attempts to test what they refer to as sociology's 'great man' theories and by inadequate attempts to further empirically motivated theoretical development. More recently, John Van Maanen (1989), as cited by Sutton and Staw (1995: 378), has called for a hiatus on the development of theory and a focus on pure description. Contrary to the impression articulated by Hirsch *et al.* (1987: 14–16), several prominent economists have called for descriptive work (see, for example, the research call issued by the editors of *The Journal of Industrial Economics*, <http://haas.berkeley.edu/~jindec/vision.htm>).

¹¹ For example, we asserted in the KGN model that it is the *relative*, not the absolute, levels of private and common benefits that affect the likelihood of competitive behavior in alliances. This is a claim for which data can be collected and that can therefore be tested and falsified.

¹² Some of the mathematical underpinnings of KGN can be found in Khanna (1996).

work.¹⁵ Hirsch, Michaels, and Friedman (1987) suggest that there seems to be an unbridgeable gap between economics ('clean models') and sociology ('dirty hands'). Our experience, in contrast, suggests that there is room for research that emphasizes the complementarity between clean models and dirty hands (Hirsch *et al.*, 1987). Gibbons (1999: 155), who similarly argues for the complementarity of approaches, calls for 'blending description, informal theory and formal economic modeling' in studying organizational phenomena. Indeed, the KGN model, though grounded in economics, is emblematic of the results of burgeoning and fruitful collaboration between economists and sociologists.¹⁶ At the portfolio level, with the research carried out by the entire academic community constituting the relevant portfolio, it appears to us that disciplinary eclecticism combined with iteration of deductive and inductive work is likely to be the most productive strategy.

There are multiple and related schisms within the strategy field. Polarization exists not only between researchers who define themselves as studying strategy process vs. strategy content, but also between those who favor inductive research over deductive research and those who methodologically ground their analysis in sociology vs. economics. Calls to bridge these fault lines (Huff and Reger, 1987; Zajac, 1992), have not received the attention they deserve. Instead, there has been some amount of stereotyping and labeling of each camp by the other that has probably deepened divisions. We hope that this article surfaces an ongoing latent debate between seemingly disparate groups of strategy scholars and serves, in particular, to initiate the building of useful bridges between strategy content and strategy process.

¹⁵ Of course there is no inherent reason why the iteration could not be carried out exclusively by economists or exclusively by sociologists.

¹⁶ Other examples of recent work that has drawn on economics and sociology are provided by an analysis of the effects of status on entry and predation in turn-of-the-century British shipping cartels (Podolny and Scott-Morton, 1999), studies of the economic and social predictors of the governance structure of alliances (Gulati, 1995b; Gulati and Singh, 1998), and by empirical analyses of the effects of social and economic ties in business groups in emerging markets (Khanna and Palepu, 2000; Khanna and Rivkin, 1999). Such cross-disciplinary work should be used to posit alternative theories that can then be tested. Indeed, the entire field of economic sociology can be seen as an outcome of increasing collaboration between the two disciplines.

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