Ugly Duckling No More: Pasts and Futures of Organizational Learning Research

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Introduction

For nearly thirty years, organizational learning theory has been an ugly duckling in the pond of organization theory: interesting, but living on the fringes. In the preceding Crossroads essay, Chris Argyris argued that learning research has been hampered by the defensiveness of different theorists emphasizing incremental or radical learning processes. He called as well for a new regime of applied interventionist research. In this essay we first offer a more optimistic interpretation of the historical contrast between incremental and radical models of learning. We support Professor Argyris' reminders about the dangers of defensiveness, but we argue defensiveness is difficult to assess. And, in the case of learning research, we suggest that the early advocacy of both incremental and radical models of learning may have helped advance understanding.

We then address theoretical and research frontiers for learning research, a second theme of Professor Argyris' essay. We outline three key theoretical questions for further work. We call for more systematic empirical learning research, suggesting that the paucity of such research may have resulted less from defensiveness than from the demanding requirements of doing crisp, systematic learning research. The need for scholarly empirical work is enhanced, we believe, by the growing popularity of organizational learning models among practitioners. Concurring with Professor Argyris' broad concern with enhancing research fruitfulness, we suggest ways to supplement traditional organizational research methods. In particular, we argue that it makes sense to cast wider nets for models of learning and adaptation, to sustain qualitative investigation, to use simulation techniques, and to maintain stronger—and perhaps even experimental—linkages between applied and theoretical research.

In conclusion, we note drawbacks in learning as a central framework, but support its shift to center stage in organization theory. Among other things, the learning framework offers a sensible middle ground between arrogant theories of total human control and sad theories of human helplessness.

The Dispute Between Incremental and Radical Learning and the Role of Defensiveness

Incremental Versus Radical Organizational Learning

A blind reviewer of an early version of this essay referred to the authors of this piece as the single-loop learning group. We found this designation quite surprising, because we don't think of ourselves or others as single-loop or double-loop researchers. Indeed, without giving it great thought, we assume there is now widespread—if implicit—consensus that: (1) the distinction between incremental and radical organizational learning is useful, and (2) both types of learning can produce adaptive advantage for an organization, but both can also present distinct dangers.

Two Historical Emphases. In the past decades, students of organizational learning have developed at least two distinct streams of work. Cyert and March (1963), in *The Behavioral Theory of the Firm* (BTOF) described firms as adaptive learning systems in which much behavior unfolds through standard operating procedures. If performance does not meet aspiration levels, however, problem-driven search occurs. Routines that appear to produce useful outcomes are retained, while others are discarded. Learning is generally incremental.

Since the early formulation, behavioral learning scholars have claimed that incremental learning is not only common (Cyert and March 1963; March and Olsen 1976; Levinthal and March 1981, 1993; Mezias and Glynn 1993) but can have useful outcomes (March 1981; Miner 1990, 1991). In addition, many researchers outside the BTOF tradition also stressed incremental learning. Research on the learning curve in manufac-

turing, for example, assumed the presence of continuing incremental improvements in production efficiency (Yelle 1979).

As these ideas developed, Argyris and Schön (1978, pp. 2–3) defined two types of learning called *single-loop learning*, in which error detection "... permits the organization to carry on its present policies or achieve its present objectives," and *double-loop learning*, which occurs when "... error is detected and corrected in ways that involve the modification of an organization's underlying norms, policies and objectives." Much of Argyris' early writings stated or implied that only double-loop learning has adaptive potential: he argued that single-loop learning is the enemy of organizations in solving many difficult problems (Argyris and Schön 1978).

While using different terms, other researchers also noted that the failure to undertake radical learning, could lead to disaster. Starbuck (1984) for example, showed that gradual improvement of mechanical adding machines was a death trap for a firm that needed instead to learn how to compete in a world of electronic calculators.

Current Convergence. We think these two distinct lines of thinking have now converged to an implicit conclusion on two points: (1) Both incremental and radical learning are meaningful concepts, and (2) both learning types can enhance survival and prosperity under some conditions, but can also hurt organizations in other conditions. Two clusters of scholarly work along with related anecdotal evidence prompt our conclusion. First, active learning researchers themselves have supported these points. In his accompanying essay in these pages, Argyris, for example, describes his participation in research on the application of functional management tools, e.g., strategy, managerial accounting, manufacturing techniques. These experiences led him to conclude that in some cases single-loop interventions "... created liberating alternatives. Although they did not create double-loop learning, it appeared that they prevent the need for double-loop learning in the first place" (Argyris 1995). A good financial reporting system, for example, could empower people and lead to useful outcomes.

In the meantime, BTOF scholars have emphasized the double-edged sword of incremental learning and the potential value of radical learning. Levitt and March (1988) for example, called attention to "competency traps." In a competency trap, an organization obtains short-term gains from continuing to develop current competencies, but thereby loses out on the chance to move to a new, substantially more useful competency. March (1991) explicated in detail the related trade-off between allocating resources to the exploitation of existing practices or to the exploration of new alternatives.

Second, researchers working in closely related streams of work have converged to the related distinction between incremental and deeper change, and have provided substantial qualitative evidence for value and dangers in each. Research on technology has emphasized that both incremental and radical technological changes can enhance organizational prosperity (Abernathy and Clark 1988; Anderson and Tushman 1990; Tushman and Anderson 1986). A similar distinction is a thematic hallmark of research on organizational innovation. Ettlie et al. (1984), Dewar and Dutton (1986), Tushman and Anderson (1986), Nord and Tucker (1987), Nadler and Tushman (1989) all distinguished between incremental and radical innovation, although they use a variety of terms. Tushman and Nelson (1990) presented historical evidence that a new, external technology can be either competency-enhancing or competence-destroying. In the first case, incremental or single-loop learning can be useful, while in the second, it can be disastrous.

Finally, reports from practitioners on their experiences with total quality, re-engineering, and other process-oriented management techniques highlight the potential value and danger of both sorts of learning. For example, many specific techniques for "continuous improvement" clearly embody incremental learning and have had powerful impact in at least some cases (Bisgaard 1989; Winter 1994). At the same time other TQM practices specifically seek "breakthrough" or radical learning, as do some aspects of current "re-engineering" practices (Deming 1986; Hammer and Champy 1993; Winter 1994).

This de facto convergence, we think, suggests that the current intellectual frontier is not a battle between unconditional claims for either incremental or radical learning. Instead, we need to produce more precise theory and more extensive empirical data about the conditions under which each leads to prosperity or problems, or both. This work can now build on the rich history of ideas developed in the past decades.

The Issue of Defensiveness

In his accompanying essay, Professor Argyris (1996) argues that the early emphases on incremental versus radical learning arose from the personal defensiveness of scholars. He fears that defensiveness will continue to hamper future work. This is, of course, both a

serious charge and a classic concern in the practice of science. Critical theorists and postmodernists have called attention to the striking power of starting frameworks to limit our ability to absorb contrasting perspectives (Agger 1991). In a fascinating historical study of an individual instance, Gould (1989) provides a very concrete tale explicating how an early paleontologist's deepest values apparently led him literally to see incorrectly the features of Precambrian fossils. Gould argues that this allowed him to maintain his assumption that evolution produces progress towards greater complexity and diversity in spite of the fact that this progression appears to be false.

The potential dangers of defensiveness obviously do not apply only to paleontologists. They are probably especially pernicious in the social sciences where definitive evidence may be even harder to come by than in fossils. Invoking defensiveness as a key factor in the history of incremental versus radical learning, seems to us somewhat puzzling, however. First, attributing defensiveness to others requires careful inspection of available evidence about others' actions. In this case, we do not have evidence about the mental states of researchers who initially emphasized incremental processes. This early emphasis could have arisen from the nature of the organizations being studied, from choices about modelling priorities, from preferences for model simplicity, from accidents of timing, or from theoretical tastes. In addition, behaviorist theorists went on to explicate the dangers of incremental learning (March 1991), which would seem to suggest ongoing development rather than defensiveness.

In the "radical" learning tradition Professor Argyris has access to his own experience, of course, and suggested his own behavior may have included defensiveness. His willingness to consider this possibility is exemplary, an important reminder that we found useful in considering our own work. Even here, however, we see another possibility. We suspect that Professor Argyris may be too hard on himself retrospectively. He may be interpreting a strongly held presumption—which he corrected based on later experience—as harmful defensiveness. In addition, his persistence in exploring the value of radical learning may actually have served a useful role in enhancing attention to both types of learning.

The difficulty of assessing defensiveness in others and the subtleties of its interpretation suggest to us that defensiveness may be a concept most valuable in the private inspection of our own work. In general, the best way to overcome prior blinders of *other* researchers is to conduct current research that provides compelling evidence for an alternative viewpoint. Paleontologists gave up the view that evolution produces progress towards more diversity because of the accumulation of detailed, careful drawings and facts about specific fossils themselves (Gould 1989).

The case of incremental versus radical learning also demonstrates the difficulty of firmly assessing consequences of researchers sticking to particular viewpoints. In some cases too early a convergence in a field can lead to harmful collective narrowness. This may have happened in the area of early leadership studies, where an early dominant model led to somewhat narrow work for decades. In contrast, we speculate that the early partial isolation of the "incremental" versus "radical" learning emphases may have had useful results through permitting each perspective to be elaborated with vigor. Even more importantly, the fact that scholars in somewhat independent traditions have now converged to the view that both types of learning can pay off strengthens the credibility of this conclusion.

Theoretical Frontiers

An important factor in our perspective on incremental and radical learning is our belief that organizational learning now stands on the threshold of moving center stage in organization theory, both in an applied and theoretical context. Practitioners and consultants increasingly argue that organizational learning may be "... the only sustainable competitive advantage..." (Stata 1989, p. 64; Root 1994). Academic journals publish a steadily increasing number of articles on learning. Formal organizations seem increasingly protean (as in the boundaryless organization, rise of contingent workers, re-engineering and the apparent effects of hypercompetition). In addition, knowledge assets such as scientific research, patents and technological competencies clearly affect organizational survival and prosperity. The increased pace of technological evolution underscores this fact. For example, the total quality movement offers a complex array of practical techniques for trying to apply the previously abstract concepts of collective learning (Deming 1986; Bisgaard 1989). Re-engineering advocates also typically work with intended learning processes (Hammer and Champy 1993).

"Learning" may play out as a fad or represent a more fundamental shift in our dominant vision of organizations. We favor a fundamental shift to a learning framework. We think it will require building on the past decades' work to explore three key questions;

Table 1 Key Questions in Organizational Learning

Levels of Learning (Sample Issues)	Key Learning Processes	Sample Factors Affecting the Impact of Learning
Individuals Acquisition of new skills, norms and values; effects of experience and ambiguity; individual interpretation.	*Trial-and-error Learning* Repetition of successful routines; behavior and competencies; standard operating procedures.	*Presence of Superstitious Learning* Incorrect conclusion that action caused valuable outcome.
Groups Performance feedback; shared understanding; coordinated behavior.	*Inferential Learning* Informed observation; active experimentation; interpretation and information acquisition.	*Myopic Organizations* Overvaluing information from current period, local situation, or past success.
Organizations Aspiration level; intraplant and interplant learning; organizational information processing.	*Vicarious Learning* Observation and copying of successful routines; deduction from outcomes.	*Level of Noise* Rapid learning can be dangerous in the presence of substantial noise in the feedback process.
Populations of Organizations Shared experience; timing; technological standards; effects of varied copying rules.	*Generative Learning* Active and creative discovery processes.	*Number of Independent Subunits* Many learning frontiers can reduce usefulness of learning.
		Epistasis Effect of one routine depends on the presence of other routines.
		Timing Learning too early or too late.
		Interaction between Levels of Learning Same event may facilitate learning at one level but inhibit it at another.

which are in Table 1:

1) **Who or what is doing the learning?**
2) **What are the key learning processes?**
3) **When is learning valuable?**

Who / what Is Doing the Learning?

Much popular work on organizational learning seems to assume that learning goes on in the heads of individuals. Considerable evidence suggests, however, that we need to address specifically individual, group, organizational and population level learning.

Individuals. Substantial field research documents *individual* learning in organizations, whether of new skills (Heneman et al. 1989), norms and values (Argyris and Schön 1978; Van Maanen and Schein 1979; Barley 1986) or technical information. In addition, it has been shown that experience and ambiguity can affect the way individuals adapt to experience (Glynn et al. 1991). Strategy and product development researchers have recently provided additional evidence that individual frameworks (Dutton and Duncan 1987) and "thought worlds" (Dougherty 1992, p. 191) affect the acquisition and interpretation of new information.

Groups. There is also evidence that *groups* can learn. Lant (1992) provided evidence that group decision making responds to performance feedback in a manner similar to individuals, and that the groups in her study adjusted aspired levels of performance in a manner consistent with a trial-and-error learning process. However, the aspiration levels of her groups had a strong upward drift and the groups tended to adjust them more quickly than would be predicted by a strongly incremental process. Other research suggests group learning can be conceptualized as distinct from

changes in the cognitions of their members, with shared understandings and tacitly coordinated behaviors (Brown and Duguid 1991). A somewhat unusual study arguing this point, for example, details the actual behavior of a group responsible for moving their massive ship into a harbor with a broken navigation system (Hutchins 1991). The author provides tantalizing evidence that the group developed effective computational routines that were outside the awareness of any of its individual members.

Organizations. The behavioral theory of the firm, of course, emphasized *organizational* learning, with qualitative evidence that problem-driven search arises when current collective performance is judged to be a failure, and tends to favor consideration of solutions similar to current behavior. More recent work has elaborated some of the ways in which interpretations of success and failure determined by aspiration levels affect routines governing behavior, decision-making, risk-taking and attributions for performance (Mezias 1988; Milliken and Lant 1991; March and Shapira 1992). Current work has also begun to clarify the detailed processes of intraplant and interplant learning. For example, Epple et al. (1996) found that a second shift added at a manufacturing plant achieved a productivity in two weeks that had taken the first shift many months to achieve, suggesting that knowledge became embedded partly in organizational structure and technology. Moorman (1995) also provided early survey data supporting the claim there are distinctly *organizational* information processing patterns. Recent evidence also suggests that learning from experience may occur in some phases of product development but not in others (Van de Ven and Polley 1992).

Populations of Organizations. Finally, researchers have now called attention to *population level learning*, when entire collections of organizations acquire new types and mixes of organizational routines through shared experience (Mezias and Lant 1994; Miner and Robinson 1994; Miner and Haunschild 1995; Darr et al. 1995). Among other things, these authors suggest that populations can learn both "too early" and "too late." The early setting of technological standards may lead to a suboptimal technology, for example, while a late adoption of standards in one country might permit a population of firms in another country to determine the dominant design and capture the market (Miner and Haunschild 1995). They argue as well that organizational and population level learning may not tend to the same outcomes, and that each level of learning can enhance and retard the other.

For the next phase, we think it will be crucial to clearly distinguish learning at all four levels and study each in its own right. This can prevent false conflicts. Indeed, to some degree, the apparent differences between the single-loop/double-loop and behavioral learning schools arose from a tendency to emphasize one level rather than another. Professor Argyris' work traditionally emphasized individual mental processes, while the behavioral theory of the firm has highlighted collective organizational features. Research on all four learning levels has practical as well as academic implications. Appropriate managerial and governmental attempts to enrich organizational prosperity would rely on different mechanisms depending upon which learning level was judged most crucial. Training of individuals makes sense if individual learning is the main issue. Government incentives for new organizational practices could represent appropriate interventions if organizational processes embodied the crucial learning level.

What are the key learning processes?

Behavioral theories of the firm began with the assumption that standard operating procedures drive much organizational action, and postulated that actions that appear to produce results tend to become incorporated as procedures. Such a process can be seen as a form of trial-and-error learning at the organizational level, and emphasizes *behavior and competencies* over individual mental processes. In a similarly behavioral spirit, strategy theorists working from resource-based view of the firm and students of technology management have begun to tackle the difficult issue of the learning of competencies (Teece 1987). Practical evidence for the importance of competencies in contrast to pure information can be seen in the history of VCRs. Competency-based learning (e.g., developing the ability to effectively and quickly develop, produce and market VCRs) had crucial competitive impact. U.S. researchers with scientific knowledge could not easily acquire tacit competencies of key Japanese firms (Lurie 1987). Interestingly, the emphasis on learning competencies in contrast to information is supported by research on cognitive learning that points to two distinct types of memory. Declarative memory appears to consist primarily of ideas and facts, while procedural memory stores action sequences, scripts or other active routines (Cohen 1991).

Much learning research emphasizes *inferential learning* rather than the acquisition of competencies. Infer-

ential learning can be distinguished from pure trial and error learning, which does not require deductions about why particular behaviors have specific outcomes. Inferential learning can arise from informed observation of natural variation but also from active experimentation. March et al. (1991), for example, explicated ways in which organizations try to enhance inferential learning from experience when they have little experience, as when airlines seek to prevent rare accidents. Product development studies have claimed that organizations sometimes observe the results of their own improvisations and draw inferences about fruitful produce and process revisions (Moorman and Miner 1995, Miner et al. 1996). Surprisingly, there is almost no research explicitly addressing organizational *experimentation* in pursuit of inferential learning. Although its potential importance has long been noted (Argyris and Schön 1978), understanding inferential organizational learning lies at the core of decades of research in psychology as well as the philosophy of science. Conceptualization of its occurrence and distinct features within and by organizations, however, presents a distinct but continuing challenge.

Both behavioral and inferential learning are well represented in existing organizational learning theories. Two other important learning types have received more recent attention. In *vicarious learning*, organizations observe other organizations and copy successful routines or deduce more abstract knowledge from observing outcomes. Some work has called attention to its potential importance for learning by individual organizations (Huber 1991; Lant and Mezias 1992; Campbell 1994). Formalization of vicarious learning in the practice of "benchmarking" offers an interesting context for research on such learning, but we do not as yet have a developed body of empirical research on such practices. Understanding vicarious learning by individual organizations is especially important because it can produce systematic population level outcomes. The frequency and accuracy of vicarious learning has powerful implications for patterns of behavior in populations of organizations (Levitt and March 1988). It can lead sometimes to systematic "improvement," but in others to run-away processes (Boyd and Richerson 1985; Miner and Haunschild 1995). Students of technology management have made interesting progress in modelling advantages and disadvantages of second-movers, who can observe the results of first movers in product introduction (Lieberman and Montgomery 1988), but much remains to be done in this area.

Generative learning, or discovery, represents a fourth crucial learning process ripe for increased empirical investigation. Many theories have tacitly assumed a fixed world of exogenous conditions to which organizations must adapt. Generative learning includes an active, creative component that appears to go beyond discovering preformed external regularities (Pelz and Andrews 1966). To some degree, early emphases on radical or double-loop learning invoked the potential for such creation (Argyris and Schön 1978). More recently, Nonaka (1990), has stressed knowledge creation in product development, while Senge (1990a, 1990b) has also stressed the importance of such creative processes. Generative learning presents formidable conceptual and modelling problems, but remains of crucial interest. It is especially important for students of scientific invention, product development and "creative" organizations.

When is Learning Valuable?

Although "learning" carries a positive connotation in many cultures, research on organizational learning clearly shows it may or may not produce good outcomes. Organizations can learn to do bad things, from society's perspective, such as organizing illegal activities or colluding to harm society (Baker and Faulkner 1993). Organizations can also learn things that are incorrect. In superstitious learning, a firm may incorrectly conclude its own actions caused a valuable outcome and repeat that action, producing harmful outcomes (Argyris and Schön 1978; Levitt and March 1988). Organizations may also be myopic, overvaluing information from the current period, local situation, or past success (Levinthal and March 1993; Mezias and Glynn 1993).

Prior research points to several crucial factors determining learning impact, *learning rates* and the *level of noise* in the feedback process, for example. An important finding of simulation models is that in the presence of substantial noise, very rapid learning can actually be dangerous, as early signals lead to strong and inappropriate outcomes (Lounamaa and March 1987; March 1991). The *number of independent learning subunits* in a learning system also can powerfully determine adaptive outcomes. Formal modelling indicates that under certain circumstances too many learning frontiers can actually reduce the usefulness of learning (Lounamaa and March 1987).

Epistasis, or the degree to which the impact of individual features of a system depends on the presence of other features (Kauffman 1989) represents a less-explored but equally crucial factor in driving learning outcomes. To the degree that organizations are highly epistatic systems, they face serious problems in trying

to learn by adjusting one routine at a time. Specific learning strategies will have different consequences under high and low levels of epistasis. Additionally the *timing* of learning in relationship to exogenous events is equally important. In principle, organizations (and populations of organizations) can learn both "too early" and "too late" in terms of their environments, adapting within limited windows of opportunity for adaptive learning. Finally, the adaptive outcome of learning processes may depend on the interaction of learning processes at different levels (Levinthal 1991).

The variety of learning levels and processes makes it unlikely that we will find any simple, noncontingent list of key determinants of valuable learning results. It is also clear that practices learned in current environmental configurations may prove to be disastrous in later configurations. It will nonetheless be essential to explore specific factors affecting learning outcomes. These factors are not only of theoretical interest. Many have potential practical implications. For example, in an epistatic system, experimenting with change in a single practice might lead to the false conclusion that it was a mistake, because the new practice needed to be implemented in the presence of other routines.

Research Modes

In Professor Argyris' essay on learning research, he argues not only for more effective theoretical development, but also for a shift to more interventionist research on organizational learning, consistent with the focus of his prior work. It seems to us very appropriate to consider the question of learning research modes at this time, but we envision a somewhat different trajectory of investigation. We see a dramatic need for more systematic empirical learning research with special emphasis on longitudinal studies. We concur with Professor Argyris, however, that it may be fruitful to expand the research modes in studying learning. In particular, we see value in casting a wider net for models of adaptation, sustaining qualitative empirical work, increasing our collective competency in simulation and formal modelling, and forging more productive—and

Table 2 Research Modes

Need for More Systematic Empirical Research	Options for New Tools in Learning Research
Historical Predominance of – general schematic models – field-based qualitative insights – simulation studies	*Casting a Wider Net for Learning Models* – interdisciplinary approaches (e.g., chaos; complexity; natural language theories) – focus on distinctive properties of organizations
Difficulty of Quantitative Research on Organizational Learning – complex models – dynamic nonlinear processes with interactions – need for longitudinal data	*Qualitative Research* – specific constructs difficult to measure quantitatively – valuable insight into micro-processes – danger of missing power of interpretation and passion in learning
Practitioner Interest Exceeds Research Base	*Simulation and Other Modelling Tools* – generates new hypotheses – shows nonobvious logical consequences of precisely formulated systems
	Use of Applied Research – dangers of interventionist research – De facto natural experiments in learning – TQM – re-engineering – "Precompetitive" collaborative projects with practitioners – Closer attention to work done by colleagues

perhaps even novel—links between applied and theoretical learning research. Table 2 summarizes several key issues regarding research modes.

The Pressing Need for Systematic Empirical Research

The ratio of systematic, empirical learning research to learning theories is far too low. Historically, learning articles have consisted primarily of (1) general schematic models of organizational learning (2) field-based qualitative insights, and (3) simulation studies. Even in the outstanding 1991 special *Organization Science* issue on organizational learning only three out of ten articles presented any empirical data, with only one of these using formal quantitative analysis.

Surprisingly, existing research converges much more than is obvious when one first encounters this literature. Both qualitative and simulation data confirm the likelihood of competency traps or the dangers of "single-loop" learning, for example. For areas like this, the time is right to get on with tackling empirical work on vital determinants of such outcomes. We need a solid empirical basis for making robust predictions about organizational factors that increase the propensity to fall into competency traps. We need empirical data on the depth, duration, intensity and outcomes of such traps. Overall, systematic field studies are needed to link the many possible learning outcome patterns more tightly to the concrete world of operating organizations.

One can develop many accounts of why researchers have not produced more systematic empirical research until recently. We believe the single most powerful historical reason for the lack of outstanding quantitative empirical research on organizational learning (at any level of analysis, about any mechanism) is that it is excruciatingly hard to do well. Learning models are easy to invoke but difficult to study systematically. Even simple learning models involve dynamic nonlinear processes with interactions between multiple units over time. Cross-sectional data and modelling assumptions based on equilibria are typically inadequate to test even crude learning models. Data at the organizational and population levels must often be collected over time and linked tightly to proposed learning mechanisms. If the learning involves more than simple trial and error, measures of theoretical constructs such as organizational memory must be developed. Models that invoke cognition further complicate any observational research, since key constructs of such models are difficult to measure.

Fortunately, the past few years have seen a substantial increase in focussed and cumulative field research (e.g., Argote and Epple 1990; Epple et al. 1991; Van de Ven and Polley 1992). Researchers increasingly work with longitudinal data and use analytic techniques such as event sequence analysis and event history analysis. These studies provide additional hard evidence for both individual learning and aggregate learning processes beyond individual change. The fact that good research has been hard to do does not imply we should lower the aspiration for top-notch formal empirical research. We believe that the current popularity of learning as a practical vision for managers increases rather than decreases the need for rigorous systematic research.

The Need for New Tools in Learning Research

At the same time, several features of the current landscape do suggest we may want to consider supplementing even serious traditional research methods in exploring organizational learning.

Casting a Wider Net for Learning Models. Formal modelling of learning processes often converges with other powerful models involving game theory, cultural evolution (Body and Richerson 1985), neural networks, language acquisition and what is broadly referred to as complexity theory (Kauffman 1989). Organizational learning research can and should both draw from and contribute to these discourses. For such models to be specifically organizational, however, will require us to delineate the distinctive properties of *organizations* and their processes and develop the implications for adaptation processes. It is hard to see how the learning framework can develop its full potential unless a distinct minority of learning researchers develops the skills to pursue such models.

Qualitative Research. It also bears repeating that rigorous empirical research can and perhaps even must include first-rate qualitative as well as quantitative work. Qualitative work offers special insights; this should be especially obvious in the study of constructs such as thought-worlds, organizational memory, and inferential learning. For example, qualitative work has provided some of the best insight into the micro-processes by which new organizational routines become part of taken-for-granted organizational practices (Barley 1986; Leonard-Barton 1990). Leonard-Barton (1990), underscored the degree to which technology transfer requires the active reinvention of an innovation when it crosses organizational boundaries, demonstrating that both energy and time were required for such learning. Qualitative research can help us avoid

some of the temptations to deny the power of interpretation and passion in learning (Argyris and Schön 1978).

Simulation and Other Modelling Tools. Productive models of organizational learning clearly will also require increasingly sophisticated technical modelling tools. Simulations have already produced many important insights such as the potential danger of rapid learning (March 1991) or the possibility of a stable set of organizations who adapt through copying others (Lant and Mezias 1990). Even if viewed primarily as a crucial generator for new hypotheses, simulation offers an important tool for continuing work. There are dangers. Many scholars harbor deep suspicions of simulation as a research tool. Results can depend on minor starting conditions or model characteristics. Some scholars believe simulation research creates a temptation for young scholars to sit at hidden workstations, far from real organizations, and simply generate minor modifications to existing simulations. Even at their best, simulation results do not test links between theory and the natural world. Instead, they examine the nonobvious logical consequences of precisely formulated systems, given defined starting conditions and values. To counter these dangers we will—as a community of researchers—need to develop both stronger skills in building fruitful simulations along with more explicit norms about their creation, presentation and evaluation.

Making Better Use of Applied Research. For decades, organizational researchers in professional schools have faced the twin calls of theoretical rigor and immediate practitioner value, shifting the balance point under different social and intellectual regimes. We do not concur with calls for a massive shift to interventionist or action research on learning along the lines of Professor Argyris' accompanying comments. However, we think his remarks point to the important need for better linkages between applied and theoretical work. In considering the value of moving to primarily interventionist research, it is useful to keep in mind well-familiar dangers of consulting or helping relationships as the sole or primary context for social science research (Heller 1986). At a practical level, the ethical interventionist researcher must devote substantial intellectual energy to helping solve immediate organizational problems. More fundamentally, deliberate efforts to have a major impact on an organization crucially change the processes under study and inevitably make the researcher a player in the politics of the organization.

Routine ethical tensions of doing field research become even more problematic if the researcher seeks to actively help an organization learn about crucial competencies. Researchers deeply involved in helping a firm do better in product development, for instance, might need to protect both specific technological knowledge, (e.g., chemical formulae), as well as proprietary organizational learning processes producing effective product development. The more organizational learning is a key survival characteristic for competitive firms, the more interventionist researchers must in good faith attend to specific, local solutions, and also keep them in confidence. Finally, these researchers, perhaps more than others, can appreciate the potential for unintended outcomes of actions in complex systems. For these reasons, we don't envision interventionist research as the sole or even the principal vehicle to advance either theories or applications of organizational learning.

On the other hand, we support calls for developing more fruitful links between practitioners, consultants and scholarly researchers, and experimentation with possible novel forms of collaboration. To begin, *current de facto natural experiments in learning* within and across nations represent an important opportunity for scholarly researchers. Organizations' efforts to implement TQM and reengineering practices offer a vast set of local experiments in which many, many organizations seek to learn explicitly from their own experiences. Explicit benchmarking programs in and across nations directly embody vicarious learning. Economic conversion processes in developing and newly capitalistic countries can be seen as crucial experiments in overcoming the obstacles to behavioral—as opposed to informational—learning about the workings of capitalism. While prior organizational and strategy research has amply confirmed the many difficulties in assessing outcomes of specific organizational actions, widespread natural experiments nonetheless offer important laboratories for systematic research.

Academic learning theorists may also want to consider experimenting with *carefully selected projects in the domain of "precompetitive" but collaborative work* with practitioner groups. Many fields in the physical sciences in the United States are now experiencing a sea change with respect to the link between theory and practice. Engineering schools that once thrived on federal funds and private funds for basic research, for example, now explore complex new relationships through research consortia, increased sponsored research, and much faster movement from precompetitive science to competitive inventions (Teece 1987;

Mitchell 1991). Many business schools have also attempted to incite more applied research by their faculty, and key national groups are experimenting with structures that sustain industry-scholar interaction. For example, the Sloan foundation has sponsored focussed studies by industry, while the National Science Foundation's initiative on new quality practices involved industry leaders as full partners in proposal development and evaluation. Even in the traditional basic disciplines, recent years have seen a renewed questioning of the validity and need for basic research in political science, sociology and related disciplines.

We do not have a finished view of the exact form of such work, but it may make sense to experiment with alternative forms for such collaboration in focussed areas. Such programs would typically involve multiple sites and some form of ongoing relationships with practitioners, but would avoid emphasis on individual organizational interventions by researchers. Research consortia in the physical sciences offer one model, but there are also a variety of models within business schools and public policy settings. Fruitful collaboration of this sort has been shown to be possible but fragile, requiring sophistication in managing expectations on all sides and careful attention to ethical concerns.

At a minimum, we concur with calls for learning researchers—both theoretical and applied—to *pay closer attention to our colleague's work.* Even within theoretical work, fragmentation and lack of cumulative results have marked prior work (Huber 1991). Furthermore, there is also only a thin bridge between consultants and managers who advocate "organizational learning" and many learning theorists. This bridge deserves strengthening.

Between Arrogance and Cynicism: The Promise of Organizational Learning

We started this essay comparing learning theory to the ugly duckling, implying learning research until recently has perhaps been loved chiefly by its parents, but it may soon become a swan admired by many. The growing popularity of learning models among both practitioners and researchers presents dangers we can anticipate in advance and may need to take into account in pursuing a learning agenda.

For example, theories emphasizing the value of incremental (in contrast to radical) learning can tacitly support the status quo. Professor Argyris has emphasized danger in his recent essay and prior work, and we concur with his concern. That this concern is not an abstract generality can be seen when one examines the impact of other tacit assumptions in organization theory. In the lifetime of currently active researchers, organizational observers worked from assumptions that perpetuated gender roles. For example, it was assumed in some research that women were unsuited for management roles because of biological predispositions to expressive roles at home. However, theories advocating radical learning processes can also be potentially destructive. As academics who personally find learning especially attractive, and work from a secure base of tenured positions, we may need to watch out for overenthusiasm in urging radical learning processes for others. "Forced marches" of all employees into learning systems created with some simplistic learning ideologies can create information overload, personal anxiety, and organizations that fail to provide important social continuity.

Overall, the learning metaphor can also sanitize emotional and political processes. The antiseptic, "cool," learning metaphor can mask political processes with mixed ethical or emotional outcomes. To illustrate the latter, imagine that upper management deliberately uses competitive research development teams and "shoot-outs" between the teams as a way to generate new ideas and select among them, conceptualizing this as trial and error learning at the organizational level (Miner and Robinson 1994). If these contests produce stigmatization or unemployment for some of the participants, this process can be seen as manipulation that serves organizational but not necessarily the participants' well-being. Uncritical popularity of learning theories among practitioners may even present dangers to conducting good basic research. For example, some early efforts to suggest leaders could create and use organization cultures to simple, purposeful ends probably harmed progress of more illuminating work on organizational culture.

Neither the long incubation period of the learning framework nor these dangers, however, are reason to doubt the potential value of learning models for organization theory and management practice. We support and are heartened by the movement of learning models to center stage in organization theory. As an approach to practice, or normative theory, learning offers an image of management that is more realistic than traditional planning and control models, yet avoids cynical assumptions of total managerial impotence.

As a framework for fundamental research, learning has even more promise. It offers precise models that can be used to explore emergent and interactive phenomena. It requires attention to the dynamic qualities

of organizations as systems. It neither invests organizations with phenomenal powers in the clear-sighted pursuit of simple goals nor lapses into visions of organizations mired in random fates. It offers, then, the potential—although not the assurance—of models rich in validity, implications and intellectual excitement.

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