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Articles

Using Computer Simulations to Understand the Management of Technology: Applications for Theory Development

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Descriptors

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Abstract

We propose that the methodology of computer simulation is applicable and useful to researchers examining the management of technology. We describe the features of a computer simulation, discuss inherent strengths and limitations of the methodology, and present an illustrative simulation. The purpose of this simulation is to explore the effects of an organization's routine-based management of technology under assumptions of a learning model. Results indicate that strategies to manage technology in complex, routine-based systems may have effects that are difficult to predict and control. Applications of simulation methodologies to issues of technology management are discussed.

Introduction

By their nature, technology and technological change represent sources of uncertainty and equivocality for organizations (e. g., Nelson and Winter 1977; Henderson and Clark 1990; Scott 1990; Tushman and Nelson 1990). The latest wave of new technologies has been characterized increasingly by stochastic, continuous, and abstract properties (Weick 1990: 7). A wide range of students of technology have recognized the implications of these facts for research. Mohr (1982) has argued that the challenge for students of these new technologies is the development of an appropriate process theory. Van de Ven and Angle (1989: 4) called for a theory of technology that '... explains how and why innovations develop on the basis of probabilistic arrangement of discrete events over time.' Similarly, Scott (1990: 137) characterizes the need for a maturation of technology research, as it makes the transition from cross-selectional to longitudinal. Theory building is important for studying various aspects of technology, including technology transfer (Contractor and Sagafi-Nejad 1981; Kedia and Bhagat 1988), the interrelationship between technology and the individual (Goodman, Griffith, and Fenner 1990; Weick 1990), the sociology of technology (Tushman and Nelson 1990), and multi-level perspectives on technological change (Tushman and Anderson 1986; Henderson and Clark 1990), as well as the intersections between the different levels of analysis (Sproull and Goodman 1990: 262). Furthermore, as issues surrounding the transfer of telecommunications, nuclear, and space technologies to other countries become more pressing, so does the need for better theory.

Recently, Tushman and Nelson (1990: 2) summarized the current state of theories of technological change and the management of technology concluding that much remains unclear: '[T]here is no ... literature to inform our understanding of either technological progress or the effects of technological change on organization and individual outcomes.' New classes of technology, differing qualitatively from previous ones, are emerging and introducing new dimensions of complexity (Goodman and Sproull 1990) and new issues that '... theorists have yet to grapple with' (Weick 1990: 39). In addition, these newer technologies are emerging at a faster pace with a contracting technological life cycle (e. g., Losee 1991). The result is that organizations are being confronted at an ever increasing rate by more complex and ambiguous technologies (Kanter 1989). Further, the new technologies are posing dilemmas for students of organizations who are attempting to formulate appropriate technology policies to guide organizational, interorganizational, and international economic activity. The problem is exacerbated by the fact that this research takes place in a context where '. . . systematically collected information . . . is not accessed easily and is only beginning to be compiled' (Auster 1990: 66). A need exists, then, for methodologies that assist researchers in examining issues in the management of technology. We propose that the computer simulation, with its capabilities for performing dynamic, complex, theoretical analyses, may be a useful and appropriate methodology for contributing to theoretical development in the field of technology management.

Simulation methods are capable of capturing two central, but sometimes methodologically nettlesome, features of technology—uncertainty and complexity—and exploring their effects on organizational outcomes. Furthermore, simulation methods are capable of modeling dynamic, interactive, and complex processes with stochastic disturbances or random error (Whicker and Sigelman 1991). Models that incorporate stochastic variation are particularly important in the light of recent work that questions assumptions of technological determinism (Sproull and Goodman 1990: 258). These views of technology as indeterminant or at least far less determinant than previously thought (Scott 1990: 123) suggest that the dynamic capabilities of computer simulations may be particularly useful. Because they permit an exploration of the effects of different kinds of technology strategies or policies, computer simulations should facilitate an understanding of process; thus, they can complement existing literature, which focuses largely on the outcomes of the technology management process (Sproull and Goodman 1990: 262). Finally, many of the questions that current research has identified as important for the future study of the management of technology involve better theoretical development. We believe that this need for theory building in the field of technology management coincides with one of the strengths of simulation methodology. In particular, we argue that computer simulations are a relatively inexpensive and expedient way to explore the complex issues and range of alternative approaches implicated in the management of technology. This is particularly true when the research departs from a view of technology as an ongoing, probabilistic, and developmental process.

We attempt to make two primary contributions in this paper: First, we argue that computer simulation methodology can advance theory building in the study of technology management. Second, we demonstrate the potential applicability of simulation methodology by modeling the management of technology as routine-based. In particular, we focus on the decisions of organizational units either to refine existing technology or adopt a new technology. The paper is organized as follows. First, we give an overview of computer simulations and discuss the advantages and disadvantages of the methodology for theory building. Second, we discuss some of the principal assumptions about both the management of technology and organizations that will form the basis of an illustrative simulation. We outline a model of organizations as experiential learning systems and apply this model to simulate the effects of a strategy based on organizational rules and routines for managing technology in large, established organizations. Third, we describe how these assumptions are translated into a computer model. Fourth, we derive some sample questions that the model can be used to assess, run the model, and analyze the results. Finally, we discuss applications of computer simulation methods to future technology research and propose an agenda for future research that should facilitate theory building in the area of technology management. A separate Appendix relates the particulars of the simulation program in more detail.

What is a Computer Simulation?

A computer simulation takes a complex set of assumptions, simulates a set of organizational processes, and represents the implications of these processes for organizational outcomes. The purpose of a computer simulation is to simplify and clarify ideas about technology and to see how premises based on these ideas may lead to outcomes, both intended and unintended. Because it allows the researcher to conduct systematic theoretical experiments, a computer simulation '... provides a means of deriving, in at least a preliminary manner, answers that would not be available if we relied on more commonplace methods of data acquisition and analysis' (Whicker and Sigelman 1991: 2). By manipulating simulation parameters, the researcher attempts to control for a variety of factors that may impact the technology process under study. In addition, simulations tend to be more expansive than traditional experimental designs because they allow the researcher to manipulate a larger set of independent variables, as well as their interactions; thus, simulations tend to be both rigorous and flexible (Whicker and Sigelman 1991). Computer simulations are a particularly appropriate methodology under the following conditions: when the system under study is complex, when the researcher wants to conduct sensitivity analyses to gain an understanding of the system and its subsystem behavioral characteristics, and when performance assessment lends itself to quantitative measurement (Colella, O'Sullivan, and Carlino 1974). All these conditions are certainly relevant and important in research on technology.

Five elements comprise a computer simulation: (1) researcher-specified assumptions that underlie the model being tested; (2) parameters, i. e., the fixed values or control variables; (3) inputs, or independent variables; (4) algorithms, or process decision rules that convert input values into outputs; and (5) outputs, or dependent variables (Whicker and Sigelman 1991: 7). A basic decision in any simulation is whether to make a model stochastic, to allow for probabilities and random error that may affect both the simulated processes and their outcomes, or deterministic, in which no allowance is made for stochastic variation. The type of model chosen—stochastic or deterministic—affects the number of times the simulation must be run (i. e., more runs are needed in a stochastic model), the types of statistical tests used to analyze the output, and the confidence one has in the results. Finally, the researcher has a choice of programming languages with which to conduct the simulation. Both general-purpose languages such as BASIC, FORTRAN, or PASCAL, or a specialized simulation language, such as SIMSCRIPT or HOCUS (Whicker and Sigelman 1991: 134) may be used.

While there are several advantages of simulation methodologies, there are, of course, limitations. First, and perhaps most important, simulation results are highly dependent upon researchers' theoretical assumptions and the initializing values of the independent variables. However, researchers can reduce potential bias by changing the assumptions of the model, or by varying the initial values of parameters, or both. Obtaining similar results with these variations would increase confidence in the findings; this is often described as a sensitivity analysis because the analyst examines changes in the outcomes of the similation as a result of varying assumptions and parameters. Second, a computer simulation is not a substitute for empirical research, but a complement to it. For example, a simulation can be used to develop more rigorous theories from a set of exploratory empirical findings. Predictions from these more rigorous theories can then be tested empirically. Similarly, simulation can be used to generalize from a set of empirical findings; this can be done by creating a system based on empirical results and introducing random error or varying the parameter values suggested by the empirical results. In this way, the researcher can examine the empirical phenomenon under different conditions and contexts that may be of theoretical interest. Finally, simulation models and their output can be difficult to communicate to those unfamiliar with the methodology. Thus, it is important for the researcher to convey a real sense of the phenomena under study, as well as the decision rules and algorithms used to run the model.

Simulation methodology has potentially wide-ranging applicability; according to Whicker and Sigelman (1991: 17–18), computer models can simulate '. . . any system that can be represented by symbolic terms and logical processes.' With respect to management and organizational theory, simulations have ranged from an emphasis on the normative implications characterized by intendedly rational economic behavior (Nelson and Winter 1982), to descriptions of boundedly rational but adaptive economic systems (Cyert and March 1963; Crecine 1969), to the descriptive implications of garbage-can systems (e. g., Cohen, March, and Olsen 1972; March and Olsen 1976). In addition, computer simulations have played an important role in developing theory on organizational learning (e. g., Cyert and March 1963; Levinthal and March 1981; Herriott, Levinthal, and March 1985; March 1988, 1991; Lant and Mezias 1990, 1992; Levinthal 1990). In this paper, we extend the use of simulation methodology to study issues in technological change.

In building a simulation model of the management of technology, we adopt a view of organizations as experiential learning systems. This view, which characterizes organizations as systems that can alter their routines in response to past experience, is consistent with previous research (March and Simon 1958; Cyert and March 1963; March and Olsen 1976; Argyris and Schon 1978; Levitt and March 1988). Despite the simplicity of the general premise of this perspective, much of the literature has focused on the fact that the simple building blocks of altering routines in response to interpretations of experience can lead to a longitudinal process of considerable complexity. This suggests that deriving the implications of the organizational learning process for technology management might be quite complicated. It is difficult to predict how the processes will unfold over time, in different contexts, to yield various organizational and technological outcomes. The unfolding of these processes can be observed, however, in a computer simulation. Our illustrative simulation model will be used to demonstrate how certain assumptions about organizational routines and different environmental conditions produce changes in decisions either to refine existing or adopt a new technology.

The Management of Technology

Using Simulation Methodology to Build Theory

The contribution of computer simulation methodology to theory building in the area of the management of technology lies in its ability to offer an experimental design without the usual costs (Whicker and Sigelman 1991: 10). In simulations, different types and levels of independent variables are under the control of the researcher and can easily be manipulated and studied for their effects on dependent measures of interest. Computer simulations allow the researcher to be flexible, to incorporate uncertainty into their theoretical model, to consider all possible combinations of independent variables, to examine interaction effects, and to simulate the dynamic properties of systems (Whicker and Sigelman 1991: 28–29). All of these features are, of course, relevant to any serious efforts to model technological change as a non-deterministic process (e. g., Mohr 1982; Sproull and Goodman 1990).

A complete assessment of the potential of simulation as a theory building tool in understanding technology processes requires explicit recognition of some of the limitations built into any simulation model. Whicker and Sigelman (1991: 30–31) outline three disadvantages of computer simulations as a theory-building tool. A first is the problem of omission of detail: As with any analytic model, reality is abstracted and potentially important details may be inadvertently omitted or modeled incorrectly. For example, simulations typically treat organizations as unidimensional actors; individual employees are represented, either implicitly or explicitly, as limited in their complexity. Further, some constructs are simply outside the grasp of a computer simulation. The major limit appears to be that simulation is appropriate only for modeling those aspects of technology that can be specified in terms of a finite number of quantitative parameters. A second problem, while not an inherent feature of the simulation methodology, is difficulty in communicating effectively with those who are unfamiliar with computer or simulation methodology. Common sources of problems revolve around the use of computer languages and interpretation of the results and output of the simulation programs. A third difficulty results in attempting to verify the models that are the basis of the simulation. We are careful to describe the premises of our model; to the extent that these assumptions are based on empirical evidence or are intuitively appealing, concerns about generalizability are mitigated (Morecroft 1985). In addition, we use variations on the model, constructed by changing initial parameter values and structural equations, to present partial tests of the simulation model (Morecroft 1985). Thus, we do try to address some of the shortcomings of simulation methodology specifically, ensuring that the boundary points of the theoretical foundations and outcomes of the simulation methodology are outlined. Within these limitations, however, it is our intent to demonstrate that computer simulations can be a powerful tool for theory building.

The Assumptions

Describing the first of the five elements of any simulation, assumptions on which the simulation is built, Whicker and Sigelman (1991: 7) state the following: 'Ideally, theoretical principles provide the underlying assumptions and algorithms.' The assumptions on which our simulation is built fall into two broad categories. The first category determines our assumptions about appropriate theory and conceptualizations in characterizing technology. The second category determines our assumptions about appropriate theory and conceptualizations in characterizing the units of the simulation, organizational actors. We discuss these separately below.

Assumptions about the management of technology. Our use of technology follows the broad definition proposed by Levinthal and March (1981: 187): 'By technology we mean any semi-stable specification of the way in which an organization deals with its environment, functions, and prospers.' By taking such an administrative perspective on technology, we do not mean to deny that there is a broad literature outside the management area that addresses technological development and change. Rather it is based on the assumption that an organizational perspective is most appropriate for focusing on technology broadly as an organizational process, rather than narrowly as a pure technical process. There are two justifications for this judgment. First, for any type of technology to be implemented in an organization, management must be able to recognize and support opportunities for change; most technological innovations involve both technical and administrative components (Leavitt 1965; Van de Ven 1986). As Nasbeth and Ray (1974: 310) point out, successful application of technology is dependent upon '. . . big changes in structure and administrative practices.' This argument has been reinforced further in recent work emphasizing the interdependence of technological innovation and organizational structure (e.g., Van de Ven, Delbecq, and Koenig 1976; Barley 1986; Marcus 1988; Attewell 1992). Second, as several scholars (e.g., Cole 1968; Arrow 1971; Chandler 1977; Williamson 1983) have argued, we have largely overlooked administrative contributions to the management of technology because they are not as easily identified, protected, or patented as the technology they support.

In examining technology management, we focus on a central tenet of the literature: the distinction between incremental technological change, which refines and improves an existing design, and radical technological change, which departs in a significant way from existing technological practices (Freeman 1982; Ettlie, Bridges, and O'Keefe 1984; Dewar and Dutton 1986; Tushman and Anderson 1986). These two types of technological change—radical and incremental—involve different organizational capabilities and can result in different organizational consequences. Incremental technological change tends to be competence-enhancing, compatible to the adopting organization, and reinforcing of existing organizational capabilities; in contrast, radical technological change tends to be competence-destroying, incompatible, and demanding of new technical, commercial, and problem-solving skills (Tushman and Nelson 1990; Henderson and Clark

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1990). Thus, these two types of technological change pose fundamentally different challenges to the management of technology and will be the focus of the simulation developed here. Of course, there are other important dimensions of technology. Examples include technical complexity (Tyre and Hauptman 1992), the extent to which a new technology is compatible with established organizational practices, assumptions, and systems of production (Tushman and Anderson 1986), whether the innovation is technical or administrative (Van de Ven 1986), and whether it comes from outside or is internally generated. However, we did not think a broader focus on dimensions of technology was appropriate given our focus on simulation methodology. We believe that the simulation methodology, with appropriate modifications, could accommodate these other attributes of technological change as well and will focus here on using it to study the management of technology.

We make two assumptions about opportunities for refinement and innovation: First, we assume that the distribution of innovation opportunities improves with the time since the last innovation adopted by the organization. This implies that, in the periods immediately following the adoption of an innovation, the pool of available innovations is relatively sparse. Conversely, as the time since the last innovation increases, the pool of available innovations improves, at an increasing rate (Levinthal and March 1981; Sahal 1981; Tushman and Anderson 1986). Second, we assume that the mean value of potential refinements to current technology decreases with each refinement that has already been made; this is equivalent to assuming that there are decreasing returns to refinement. With each incremental improvement to the current technology, users move closer to the limitations inherent in the technology, and further improvements become increasingly difficult (Levinthal and March 1981; Foster 1986). Taken together, these assumptions would produce the 'S-curve' function of technological progress described by Foster (1986: 32) that is characterized by cycles; this is consistent with both the theoretical arguments of Sahal (1981) and the empirical results of Anderson and Tushman (1990). The implication is that technological ferment and innovation will be followed by incremental change and refinement to dominant designs.

In studying the innovation versus refinement dilemma, we focus on the behavior of large, bureaucratic firms. Not only have these firms been the traditional focus of research on innovation and the management of technology (e. g., Monger 1988; Rubenstein 1989), but they also control most of the productive resources in the economy. In addition, despite the recognized necessity of executing large-scale technological changes in these established firms, and their difficulties in choosing and implementing innovations rather than routine, incremental changes have been noted by many researchers (e. g., March and Simon 1958; Kanter 1985, 1989; Tushman and Nelson 1990). These frequently reported difficulties suggest that the management of technology, and particularly the problem of balancing refinement and innovation, may pose more of a dilemma in large, bureaucratic firms than in smaller, more flexible firms. To focus on this problem, our model is intended to depict the behaviors of independent business units operating within a large, bureaucratic organization. For this reason, we restrict the simulation to an examination of the behavior of an independent business unit over time; we use the term

[...] in the simulation presentation to reinforce this idea. The technological innova[...]ns we model are of a single type adopted by one cohesive unit in an organiza[...]. Of course, large organizations may enact technological changes on multiple [...]nits simultaneously and even the business units that compose these large organ[...]zations may be pursuing multiple innovations concurrently, both technical and [...]ministrative. However, as long as the process by which each of these technolog[...]al changes occurs conforms to the characteristics of the process model we [...]ropose, our model should be generally applicable, despite this limitation. Of [...]ourse, the applicability could be affected by the impacts that different innovations [...]eing pursued by a single unit have on one another or the impacts that the technol[...]ogies of different organizational units have on each other. However, we had no explicit theory to guide us in modeling such impacts. Given the focus of the study on methodology rather than a particular substantive issue, we leave modeling of such intraunit and intrafirm dynamics to future research.

Assumptions about organizations. The key theoretical principles underlying our assumptions about the behavior of organizations are derived from a view of organizations as experiential learning systems (March and Olsen 1976; Levinthal and March 1981; Levitt and March 1988; Lant and Mezias 1990, 1992). Readers desiring more background should see *Organization Science* 2, 1991, which is devoted to this topic. This is consistent with previous work that relates technological change and organizational learning (e. g., Angle and Van de Ven 1989; Stata 1989; Brown 1991; Cohen and Levinthal 1990; Tushman and Nelson 1990; Attewell 1992; Van de Ven and Polley 1992). The contribution of a learning framework in studying technology lies in its formalization of the insight that the management of technology can be modeled as an experiential learning process; this model seems particularly appropriate for studying technology in established organizations because it takes into account the effects of organizational history, and in particular, how the firm's past may affect its future capabilites for renewal and change (Lant and Mezias 1990, 1992).

The main points and assumptions of this organizational learning perspective as we use it are summarized quite nicely in Levitt and March's conceptual paper (1988: 319). They described organizations as experiential learning systems that are '... routine-based, history-dependent, and target-oriented.' A review of the literature that forms the basis for each of these three characteristics offers a relatively complete exposition of how we use the organizational learning perspective in this study.

First, it is important to emphasize the view of organizations as routine-based systems that respond to experience. Summarizing the organizational learning perspective, Lant and Mezias (1990, 1992) argued that these models typically have three important categories of routines: search, performance, and change.

1. **Search:** The modeling of search routines focuses on the process by which organizations attempt to discover adaptive opportunities in an ambiguous world via a costly and routinized process of search (Simon 1957; March and Simon 1958; March and Olsen 1976; March 1981; Sahal 1981; Nelson and Winter 1982). Cyert and March (1963) make the distinction between search that is focused on improving

and refining current practices, i. e. problemistic search, and search that is focused on changing the practices used by the organization, i. e. innovative search. They argue that it is innovative search that leads to fundamental organizational change. Levinthal and March (1981) translate this into the distinction between refinement search and innovative search.

2. **Performance:** Performance routines typically underscore the argument that organizations compare actual outcomes against a moving target: an aspired level of performance that changes over time in response to experience. Several functional forms guiding the adaptation of aspiration levels have been proposed (e. g., Levinthal and March 1981; Herriott, Levinthal, and March 1985); we rely on a general form of aspiration level adaptation that has been supported in empirical work (Glynn, Lant, and Mezias 1991; Lant 1992).

3. **Change:** A focus on change routines in the organizational learning perspective has underscored the notion that refinement and innovation may be viewed as two outcomes of the same process. In particular, the organizational choice to refine current capabilities or attempt to adopt and implement new and different capabilites can be understood as a stochastic response to experience. Organizations are more likely to persist in activities associated with success and change activities associated with failure (March and Simon 1958; Cyert and March 1963).

Second, it is important to emphasize that the learning process is history dependent, underscoring the view that there are no unique equilibria or deterministic solutions in this process. Two aspects of history dependence are particularly important in this study. First, following Amburgey, Kelly, and Barnett (1990), we assume that organizations have change clocks that are reset each time there is an innovation. For some time following a significant innovation, the effort and resources that normally would go into refinement and innovative search are devoted instead to getting the organization to function using the innovation that has been adopted. Thus, there is a small window of time when there is no search or change following each innovation. If the organization is within this window of inertia, it will not search or change in the current period. The second consideration highlighted by the argument that experiential learning models are history dependent is increasing competence: the well-known learning curve. It is well-established in the literature that, over time, organizations improve their performance with new technology, but at a decreasing rate (Yelle 1979; Argote, Beckman, and Epple 1990; Argote and Epple 1990; Epple, Argote, and Devadas 1991). Thus, we see an immediate reason why organizations may be reluctant to innovate: They will lose the competencies they have built by using the current technology. Indeed, this notion is at the heart of Tushman and Anderson's (1986) distinction between competence-enhancing and competence-destroying technological change. Thus, when organizations innovate, they do not perform as close to the true underlying potential with the new practices as they did with the old practices. The results are organizational myopia (Radner 1975) and competency traps (Lave and March 1975; Levinthal and March 1981; Levitt and March 1988). Inferior alternatives in which the organization has competence are preferred to superior alternatives in which the organization lacks competence.

Finally, the argument that organizational learning is target oriented highlights the importance of aspiration levels (March and Simon 1958; Cyert and March 1963; Mezias 1988; Glynn et al. 1991; Lant 1992) in mediating the execution of change routines. The assumption that change is more likely when performance is below aspiration level has been a central tenet in the organizational learning literature. When performance meets or exceeds the aspiration level, change is less likely (March and Simon 1958; Cyert and March 1963); if change occurs under conditions of success, it is a largely serendipitous grab at an opportunity that is perceived as extraordinary (Levinthal and March 1981; Harrison and March 1984; Marcus 1988). In addition, once it has been admitted that aspirations adapt to performance (March 1981; Levitt and March 1988; Glynn et al. 1991; Lant 1992), the picture is complicated considerably. The questions of how quickly aspirations adapt to performance, the pattern of subjective success and failure this generates, as well as the association of particular routines with this pattern of success and failure become crucial to understanding organizational outcomes (Levinthal and March 1981).

Deriving Simulation Algorithms

Search

Organizational search is modeled as the execution of a series of steps in the simulation program as depicted in Figure 1. Details of the operationalizations of the assumptions reported in all of the figures in this section are described in the Appendix. The steps are as follows:

1. The possibility of search is checked against the unit's change clock (Amburgey et al. 1990). If technological change occurred recently, the resources normally devoted to search are instead devoted to getting new routines running; thus, there is no search in the current period.

2. The variance of organizational search processes is updated. We assume that the variance of innovative search increases with the time since the last technological change; this is equivalent to assuming that there is an exogenous increase in the value of technological innovations available to a unit as a function of the time since it adopted its last innovation (Tushman and Anderson 1986). This implies that the probability of discovering a new technology that represents a real improvement over current practices increases as a function of time since the last technological innovation. We also assume that the variance of refinement search decreases with the total number of refinements already made to current practices; this is equivalent to assuming that there are decreasing returns to refinement (Levinthal and March 1981).

3. The cost of search for the unit is determined as a function of the amount of search done by the unit in the recent past. If there has been some search, the cost of search decreases with each recent search, but at a decreasing rate; this is equivalent to assuming that there is a learning curve for search (Yelle 1979; Levinthal and March 1981). When there has been no search, the ability of the unit to conduct search decays. As with the decrease of cost with each search, we assume that the cost of search increases at a decreasing rate with each unit of time that the organizational unit does not search.

4. The unit assesses whether search has been associated with success or failure in the recent past; based on this decision, resources devoted to search are increased or decreased. The rules by which this decision are made are presented in Table 1; the basic strategy is that when search is associated with success it is increased, and when it is associated with failure it is decreased. We follow Levinthal and March (1981) in having the unit make three separate decisions based on the rules in this table in each period. The first concerns overall resources devoted to search. The second concerns resources devoted to innovative search, and the third concerns resources devoted to refinement search.

5. The organizational unit determines if performance was above or below aspiration level in the last period. Following Cyert and March (1963) and Levinthal and March (1981), we impose the following rules: if performance meets or exceeds the aspiration level, then the unit devotes more resources to innovative search than to refinement search. Conversely, if performance is below aspiration level, then the unit devotes more resources to refinement search.

6. The number of searches of each type are determined by dividing the resources that have been allocated for each type of search by the cost of each type of search. Each search is a random draw from a distribution of technological opportunities. Innovative opportunities are new technologies with values that are uniform on a symmetric interval around zero; the range of this interval increases with the value of current technology and time since the last innovation. The value of refinement opportunities are multiples of the value of current technology uniform over a symmetric interval around one: The range of these multiples decreases with the number of refinements already made to current technology.

7. The program exits the search routines.

Performance

The determination of the organizational unit's performance in the simulation is depicted in Figure 2. The steps in the program are described as follows:

1. The main source of ambiguity, the exogenous drift in the value of the underlying technology, is determined. Basically, this involves the determination of two random quantities: the magnitude of drift, depicted on the left, and the direction of the drift, depicted on the right. The usual conceptualization of this ambiguity has been in terms of range rather than direction (Levinthal and March 1981; Lant and Mezias 1990, 1992); thus we assume that drift is equally likely to result in increases or decreases to the value of the current technology of the unit. Absent any action by the organizational unit, and a random walk on the potential value of the underlying technology is created. This complicates the operation of an experiential learning organization by introducing variation in performance that is unrelated to the actions of the unit. This operationalization of environmental ambiguity follows Levinthal and March (1981), and the values of these quantities are set to correspond to the mean drift in their model.

2. The actual value the organizational unit derives from its current practices is a function of where they are on the organizational learning curve. If this is a period where they have adopted a technological innovation, i. e. new practices, they move

the bottom of the learning curve. In periods subsequent to the first period after adoption, they move up the learning curve; in keeping with empirical data, we assume that improvements to performance as the unit gains experience are at a decreasing rate.

Table 1: **How Resources Devoted to Search are Changed in Response to Experience**

	Performance Relative to Target: Meets or exceeds: 'success'	Performance Relative to Target: Falls below: 'failure'
Search resources increased in the last period	increase	decrease
Search resources decreased in the last period	decrease	increase

Based on the rules in this table, each unit makes three separate decisions in each period. The first involves total search resources, the second innovative search resources, and the third refinement search resources. Equations 2 and 4 of the Appendix specify these decisions.

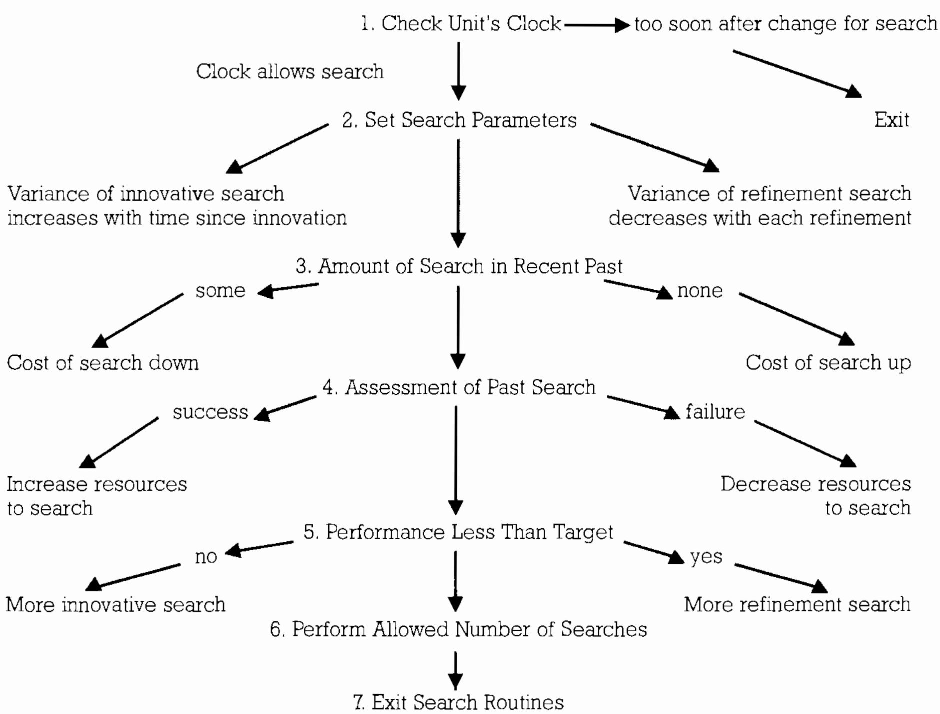

Figure 1: Flow Chart for Search

3. The performance of the unit is determined by taking the value of how well the organization did with its current routines and subtracting the resources spent on search in the current period.

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4. The adaptive aspiration level is computed; as we model it, the aspiration level changes over time in a process that is both incremental, i. e. anchored on the aspiration level in the previous period, and adaptive, i. e. responsive to experience (Glynn et al. 1991; Lant 1992).

5. The program exits the routines for determining performance.

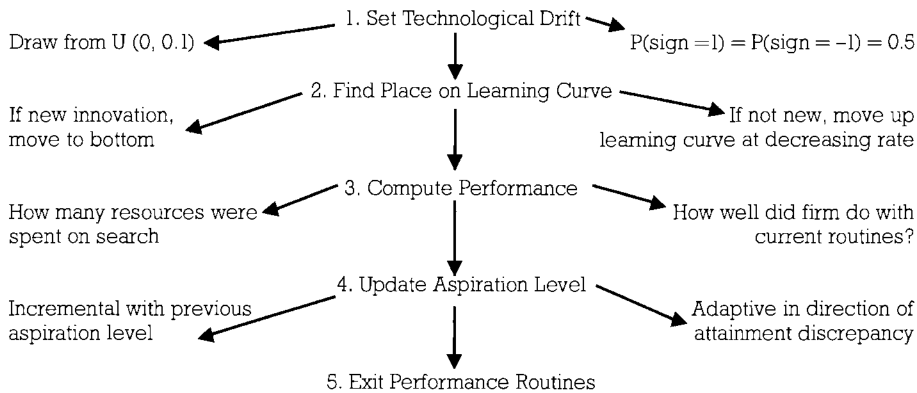

Figure 2: Flow Chart for Determining Performance

Change

The determination of organizational change in the program is depicted in Figure 3 and outlined below.

1. The possibility of change is checked against the organizational unit's clock. As with search, if the unit has only recently changed, then a subsequent change is not permitted.

2. Performance is compared with the aspiration level. If performance meets or exceeds aspiration level, then the probability of change is a function of the quality of the opportunities the unit has found in executing its search routines. If performance is below aspiration level, then the probability of change is an increasing function of the amount by which the unit has fallen below aspiration level. Following Lant (1992) and Glynn et al. (1991), we call the difference between performance and aspiration level the attainment discrepancy (AD); hence, the notation in the figure is meant to convey that the probability of change is a function of the attainment discrepancy *(f(AD))*.

3. In keeping with our probabilistic model of organizational processes, we model the change decision as a random variable. Whether the unit will actually change in this period is a binomial random variable with the probability of success equal to the probability of change. If the draw from the binomial is a 'failure,' then the unit does not change in the current period, and the program exits the execution of change routines. If the draw from the binomial is a success, then the unit proceeds through change routines.

4. Given that the binomial process allows the possibility of change, the unit must still determine if it has discovered an opportunity, either a technological refinement or a technological innovation, which it believes is a preferred alternative to

current practice. As in Levinthal and March (1981) we assume that the value of alternatives to current practices is known with some error. Based on this comparison the unit may decide that there is some preferred alternative, which it will adopt. If the preferred alternative is a technological innovation, then the technology of the unit has undergone a major change; if it is a technological refinement then the technology of the unit has undergone an incremental change. Conversely, the unit could decide that none of the opportunities discovered through search are preferable to current practices, and no changes are made.

5 The program now exits the change routines.

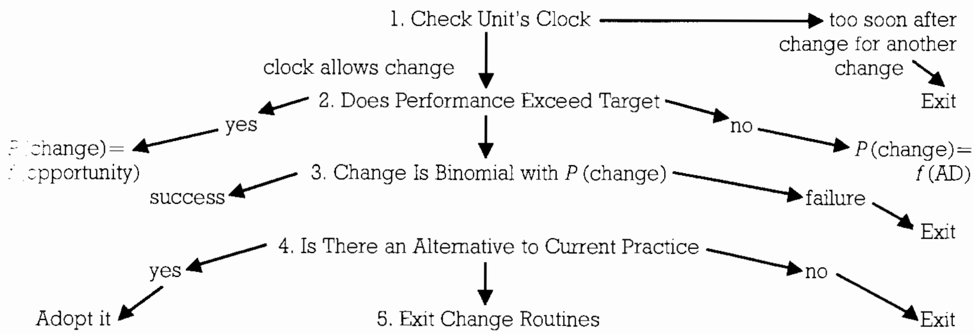

Figure 3: **Flow Chart for Determination of Change**

A Sample Analysis

In order to illustrate the potential usefulness of the simulation model we have developed to understand the management of technology, we will use the model to answer some sample questions. Since our focus is on demonstrating the utility of the model rather than a particular substantive question, we used the following heuristic to choose the research questions for illustration. We argued that organizational units could be represented as a series of routines and specified that these routines could be placed in three broad categories: search, performance, and change. For each of these categories of routines, we presented a flow chart representing a series of decisions that constituted execution of the routine, Figures 1 through 3. We will provide examples of the kind of questions that the simulation might answer by varying parameters governing the execution of the first step in each of these routines. Despite the use of this heuristic, the research questions that result do have substantive importance: The first concerns the effect of organizational inertia or inaction in the search for technical opportunities. March (1991) has described this as the dilemma of managing the trade-off between exploration and exploitation in choosing technologies. The second concerns the role of organizational ambiguity and its effects on innovation outcomes. There has long been a recognition that search and change processes are affected when the assumptions that available choices are clear and that their consequences are known are relaxed (March and Olsen 1976; Hannan and Freeman 1984; Tushman and Anderson 1986). Thus, in addition to demonstrating the use of computer simulation methodology to study technology management, the results reported in our analysis also contribute to important areas in the literature.

The way the research questions were actually derived from the Figures was as follows:

1. From Figures 1 and 3, the first parameter of interest governs the amount of time following the adoption of a new technology during which the unit will not consider adopting a subsequent new technology. We will present results showing how the outcomes change when the number of periods of inaction or inertia following adoption of a new technology is altered. In the low inertia condition, units wait only two periods following the adoption of a new technology before beginning the search for new technologies and considering the possibility of adopting a new technology. In the high inertia condition, units wait ten periods before beginning the search for new technologies and considering the possibility of adopting a new technology.

2. From Figure 2, the first parameter of interest is the technological drift. Following Levinthal and March (1981) we described this as a measure of ambiguity, because it produces variation in the value of the technology that is unrelated to the actions of the unit. We will present results showing how outcomes change as this level of environmental ambiguity changes. In the low ambiguity condition, the value of the current technology can drift up or down one tenth (10 %) of its current value in each period. In the high ambiguity condition, the value of the current technology can drift up or down one third (33 %) of its current value in each period.

Because we are specifically interested in the longitudinal dynamics of the management of technology, we run the simulation for fifty time periods. The learning model of organizations has focused attention on stochastic variation, which is an integral part of our model; thus, it is necessary to run the model multiple times to get a sense of the central tendency of outcomes. The outcomes we report are the average of fifty units tracked over these fifty time periods. Each unit is initialized as if it had adopted a new technology in period 0. Except for the parameters of interest to the two research questions highlighted for illustration, all parameters are set to the same values as in Levinthal and March (1981). While our program is wholly original and in a different language, we follow the Levinthal and March (1981) program for reasons of cumulative theory building. Many of the decision rules are identical, and we made changes only where we believed they offered a clear improvement.

Two outcome measures are reported by the program:

- First, we observe the mean total innovative changes (subsequent to the period 0 innovation) made by an organizational unit in the population. This measure gives an idea of how many units have adopted a new technology as the program progresses through fifty periods.
- Second, we observe the mean total refinements to current technology made by an organizational unit in the population. Each time an innovation is adopted by a unit, the mean refinements to current technology are reset to zero. This measure gives an idea of the propensity of an average unit in the population to refine current technology.

Results will be presented in four figures. The first will compare the mean number of innovations per unit in the low versus high organizational inertia conditions. The second will compare the mean number of refinements to current technology in the low versus high organizational inertia conditions. These figures allow us to address the question of how changing the level of inertia following innovation by the individual units affects the mix of innovation and refinement by units. The third will compare the mean number of innovations per unit in the low versus high environmental ambiguity conditions. The fourth will compare the mean number of refinements per unit in the low versus high environmental ambiguity conditions. These figures allow us to address the question of how changing the level of environmental ambiguity affects the mix of innovation and refinement.

Results

The effect of changing the length of the inertial period following a unit's adoption of a new technology is depicted in Figure 4. For the first eleven periods, there are no innovations in either condition. The explanation is identical to that given above for Figure 4; the assumptions of a period 0 innovation, depleted opportunities, and inertia combine to prevent innovation in earlier time periods. Interestingly, the pattern of innovation among units in the high and low inertia conditions begins to separate immediately, with units in the high inertia condition being more likely to innovate; this difference persists through period 40. This result may seem counter-intuitive until one considers the reinforcement of behavior inherent in a model of experiential learning. When inertia is lower, units begin searching for innovation opportunities much more quickly following a previous innovation. Given the assumption that innovation opportunities are depleted by an innovation, these units will begin spending resources on innovative search at a time when opportunities are relatively poor. Thus, they will be less likely to be rewarded with discovery of a worthwhile innovation. At the same time, expending resources on search lowers their performance, and this lower performance is associated with more innovative search. The reinforcement routines depicted in Table 1 imply that the unit will lower its propensity to engage in innovative search as a result. The higher inertia units take much longer following an innovation before they begin to search again. By the time they resume search, the opportunities for innovation are much better; the relative likelihood that they will discover a favorable opportunity prior to a disappointing one is higher. Thus, they are considerably less likely to experience negative reinforcement for innovative search. The end result is that they are more likely to innovate than low inertia units. In essence, their search pattern is a better fit with the hypothesized distribution of environmental opportunities.

The effect of changing the length of the period of inertia, following the adoption of a new technology, on the average number of refinements to current technology made by a unit is depicted in Figure 5. For the first three periods, there are no refinements in either condition. In period 4, units in the low inertia condition, which do not search or change for 2 periods following innovation, begin to refine current technology. The mean number of refinements climbs rapidly, and, on average, the low inertia units have already made more than one refinement by period 10. In period 12, units in the high inertia condition, which do not search or change for

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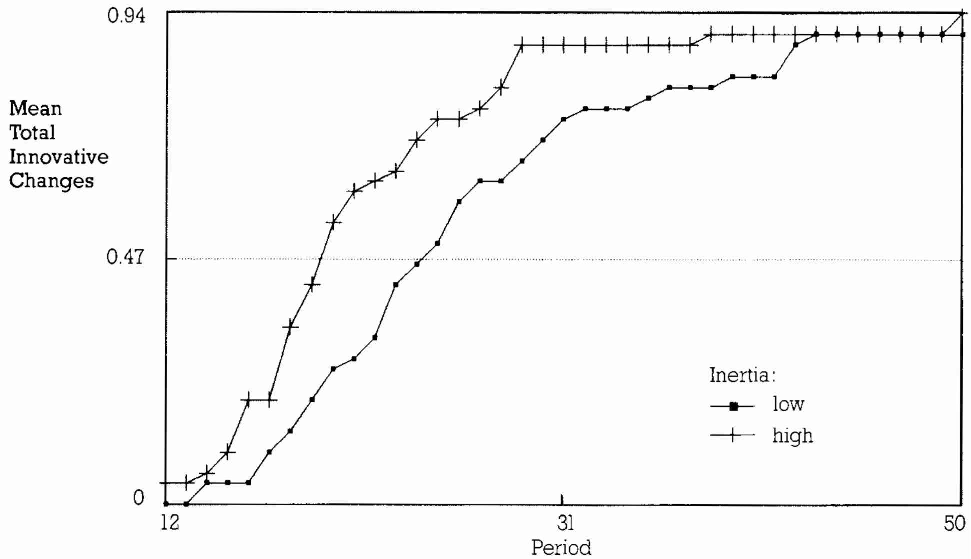

Figure 4: Mean Total Innovative Changes by Inertia Condition

10 periods following innovation, begin to refine current technology. However, the number of refinements quickly peak among the high inertia units; as shown in Figure 5, they are quick to discover worthwhile innovations, resetting the mean number of refinements to current technology to zero. Thus, the low inertia units retain their lead in the number of refinements through about period 45. However, by the close of the simulation, two phenomena bring the mean number of refinements by units, in both conditions, closer. First, innovation opportunities become so good that even those units in the low inertia condition that experienced reduced innovative search propensity finally begin to discover worthwhile innovations. This slows the increase in the mean number of refinements to current technology in this group. Second, the units in the high inertia condition are far enough along in the lifetime of the innovations they adopted through about period 30 that the mean number of refinements to current technology begins to climb for that group.

The effect of changing the level of environmental ambiguity on the average number of innovations adopted by a unit is depicted in Figure 6. For the first eight periods, there are no innovations in either condition. This follows directly from the assumptions that all units experience an innovation in period 0, that the pool of available innovations is depleted following an innovation, and that units are inert for some period of time following an innovation. Initially, the pattern of innovation among units in the low and high ambiguity conditions is not radically different. However, by about period 20, units in the low ambiguity condition begin to make substantially more innovative changes than units in the high ambiguity condition. This difference persists for the remainder of the 50 periods. This difference suggests that higher ambiguity makes it more difficult for units to discover innovative opportunities. This finding is consistent with ecological literature linking greater uncertainty with lowered probability of organizational change (Hannan and Freeman 1984). It is also consistent with organizational learning literature linking

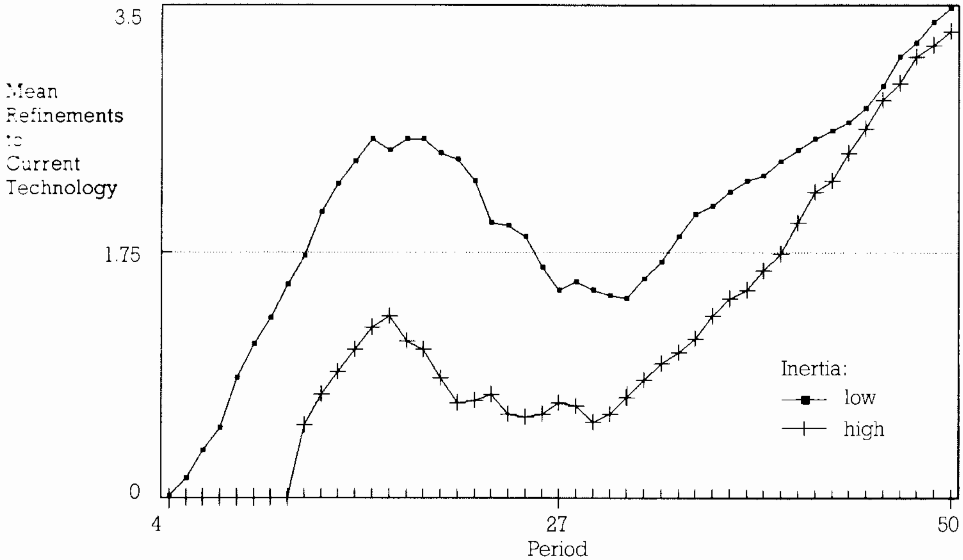

Figure 5: **Mean Refinements to Current Technology by Inertia Conditions**

decreased responsiveness with the noisy signals generated in more ambiguous environments (Lant and Mezias 1990, 1992).

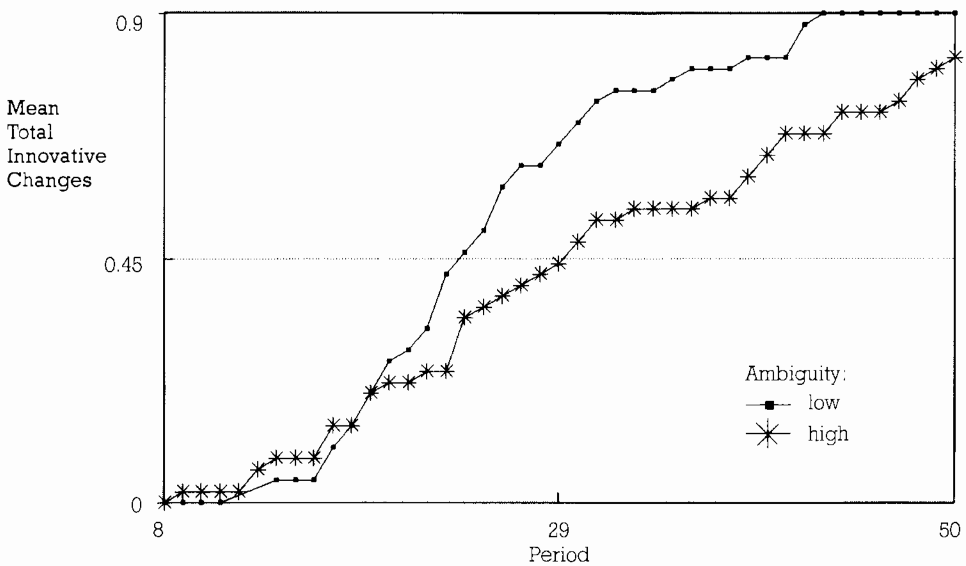

Figure 6: **Mean Total Innovative Changes by Ambiguity Condition**

The effect of changing the level of environmental ambiguity on the average number of refinements to current technology made by a unit is depicted in Figure 7. For the first three periods, there are no refinements in either condition. This follows directly from the assumption that units are inert for some period of time following the adoption of a new technology, which took place in period 0. Initially, the pattern of refinement among units in the low and high ambiguity conditions is not radically different. However, by about period 20, the average number of refinements to current technology made by units in the low ambiguity condition falls below the number made by units in the high ambiguity condition. This is a direct corollary to the increase in innovation by these units since the number of refinements to current technology is reset to zero every time a unit adopts an innovation. This difference persists for much of the remainder of the 50 periods. By the end of the simulation, however, several phenomena start to bring the lines close together again: First, the units in the high ambiguity condition start to innovate; this causes the number of refinements to current technology to fall to zero for those units. Second, those units in the high ambiguity condition that have not yet innovated start to reach the point of diminishing returns on refinement; the rate of increase in the number of refinements for these units decreases. Third, the units in the low ambiguity condition, which began innovating en masse by about period 20, have worked with the new technology long enough for the mean number of refinements to begin to increase.

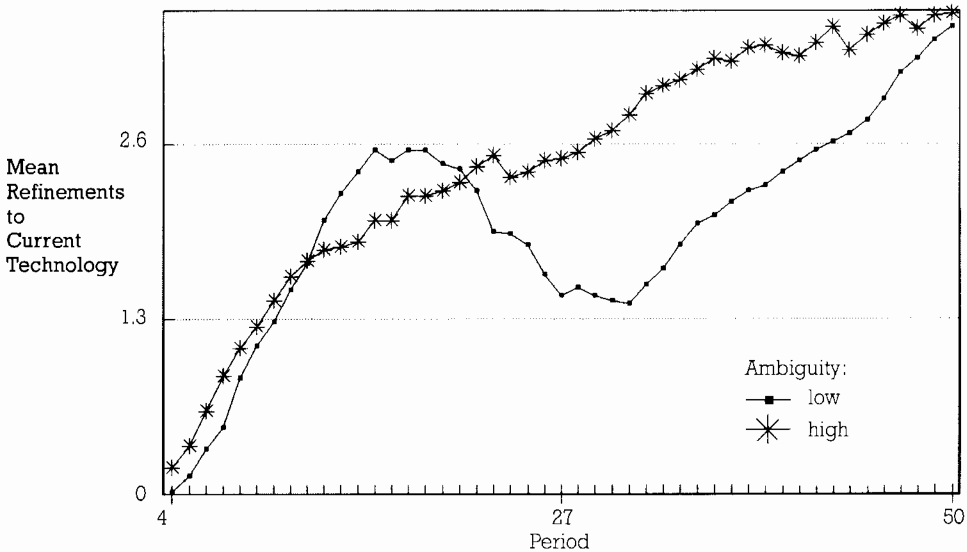

Figure 7: Mean Refinements to Current Technology by Ambiguity Conditions

Sensitivity Analysis

Of course, the questions of generalizability and external validity of the results are important. The use of a simulation model was chosen to underscore our belief that the roles of change and routines have been relatively underemphasized in the literature of technology management. We attempted to make our simulation as descriptive as possible and included empirical measures of parameters whenever possible. In addition, in order to help build the literature using simulation methodology to study technology, we built most of the program as a replication of the work of Levinthal and March (1981). This strategy of replication can directly address the question of generalizability. For technical reasons, we used uniform distributions to model search opportunities while Levinthal and March (1981) used normal distributions. The fact that our results are qualitatively similar to theirs increases our assurance that they result from an underlying logic of organizational routine and not the choice of distributions of search opportunities. We believe that a strategy of replication and extension in applying simulation methodology to questions of technology, as we have done here, has merit. In order to encourage others to do so as well, we will discuss some straightforward extensions of the analysis presented here in the conclusions.

If our focus had been on the research questions addressed in the results section, it would be incumbent on us to further demonstrate the robustness of the reported results by assessing them in a series of sensitivity analyses. Thus, we would vary other parameters of the simulation in order to gauge the effects of our independent variables, ambiguity and inertia, more thoroughly. We will not report such a sensitivity analysis because the results are only for illustration; however, we can suggest examples of additional analyses that might be run in order to enhance confidence in the reliability of the reported results. First, operationalization of the parameters of the simulation implicates any number of important assumptions. For example, a key assumption of our model is that the pool of innovation opportunities increases with time since the last innovation. By changing a parameter of the simulation (cf. equation 1), the rate of this increase could be manipulated to see if the substantive results as reported are altered. Second, various mathematical formulae are the basis for the decisions that constitute the routines of the units. The form of these mathematical relations could be altered; for example, multiplicative relations (cf. equation 2) could be made additive. These two examples illustrate the range of possibilities for sensitivity analyses that can be performed to ensure that results are stable and reliable.

Discussion and Conclusions

We used a simulation methodology to explore the theoretical implications of following a routine-based strategy for the management of technology in business units of large, bureaucratic organizations over time. An organizational learning perspective was adopted to conceptualize the dynamics of the innovation process. This perspective suggested a focus on three classes of routines: search, performance, and change. We assessed the effects of different assumptions about the inertia of organizational units and the level of environmental ambiguity on the decisions of units to refine or innovate. The findings illustrate how simulation methodology can be used to address questions of potential importance in the management of technology. First, we found that organizations were more likely to innovate in environments characterized by lower ambiguity. Interestingly, the propensity to refine current technology was unaffected by environmental ambiguity. Second, a longer period of inertia following adoption of an innovation was associated with higher levels of subsequent innovation. It was also associated with a lesser propensity to make refinements to current technology. Of course, the focus of this paper was on the methodology of simulation and not on the substantive findings of this archetypical application. However, these preliminary findings do raise interesting questions that could be explored in future research more focused on the questions we used as exemplars. We believe that the findings are suggestive of the utility of simulation as a methodology; consequently, we close by developing several recommendations for managers and researchers.

Recommendations for Researchers

The clearest implication for future research is the contention with which we began this study: Simulation methodology can be a valuable tool for the study of the technology management process. To facilitate applications of simulation methodology, we will outline more specifically some possibilities for research on the management of technology that follow directly from the simulation model we have presented here.

An obvious path for future research would be to study slight variations on the model presented here. One example might be a comparison of different strategies for the management of technology that are pursued by the units in the simulation. For example, the routine strategies pursued by the units here could be taken as a baseline strategy. This baseline could be compared with alternative designs engineered with the idea of increasing the frequency of innovation. Three quick examples give an idea of the range of possibilities (Mezias and Glynn 1993):

1. Units might pursue a deliberate strategy of devoting more resources to the pursuit of innovations, what March and Simon (1958: 184–188) called the institutionalization of innovation.

2. Noting the reluctance of large bureaucratic organizations to innovate (March and Simon 1958: 173; Kotter and Schlesinger 1979), some analysts have argued that such institutionalization tends to produce refinement rather than innovation (Brown 1991: 103). Following from this, managers of technology might pursue a strategy, designed specifically to overcome the limitations of institutionalization, by pursuing radical breakthroughs. The vehicles for achieving this might include the creation of units that do not follow the same routines as the rest of the organization, such as spin-off, skunkworks, special ad-hoc temas, or autonomous innovation work groups (Kidder 1981; Burgelman 1984; Kanter 1985; Takeuchi and Nonaka 1986).

3. Managers of technology might adopt a strategy that is considerably less intentional in the pursuit of innovation. Such a strategy might be based on a view that emphasizes modeling the search for new technology as a stochastic process. This view suggests that increasing the variance of the pool of search opportunities will increase the expected value of improvements to be found (Kohn and Shavell 1974; Levinthal and March 1981). The implications for the management of technology would include a focus on loosening organizational controls (Angle and Van de Ven 1989: 679), what Levitt and March (1988) call imperfect routine maintenance. Recommendations for the management of technology following from this view might include pursuing parallel programs and allowing redundancies (Quinn 1985: 128), adopting a portfolio approach (Kanter 1985: 46), and using organizational slack to allow for more loose coupling (Weick 1979) or as a buffer against tight control (Cyert and March 1963; Bourgeois 1981).

Simulation of the effects of these alternative strategies on the actual outcomes of units in an explicitly longitudinal setting might be a good way to pursue further development of theory in this area.

A more ambitious, but quite valuable, direction for future study would be to use simulation methodology to address questions of level of analysis. Taking the model presented here as a baseline, there are some obvious changes in level of analysis. First, the individual team members that comprise the units and their relationship with the unit as a whole could be modeled explicitly. Work by Carley (1992) on the relationship between learning in teams and the turnover of personnel is an example of how a simulation model of these processes might be designed. Second, the larger organizational context of which these units are a part could be modeled explicitly. Relationships among units and between units and 'headquarters' could be given explicit form and implications for the management of technology derived. Third, the organizations comprised of these units might be placed in a specific interorganizational context. Here questions of the imitability of technology and its effect on the development and diffusion of innovations could be addressed explicitly (Lippman and Rumelt 1982). Also, competitive conditions and their effect on patterns of technological development (Sahal 1981; Anderson and Tushman 1990) as well as the ability of organizations to devote resources to the pursuit of both refinements and innovations could be modeled explicitly. A similarly ambitious and equally valuable direction for future study would be to address how computer simulation methodology could be teamed with in-depth, field research processes. More specifically, we believe that computer simulation can begin to map the routine processes that underscore technological change. However, how these processes originate or change is assumed rather than studied. In-depth research, tracking technologies through organizations, across organizations, and across industries might begin to untangle the origin and nature of these routine processes.

It is our belief that pursuit of any of these proposed research projects using this simulation model would have three principal strengths. The first would be an emphasis on a clear set of organizational routines as a model of the management of technology. Explicit exposition of assumptions, while difficult and perhaps overly simplified, makes an understanding of the outcomes of the management of technology under different conditions more comprehensible. Second, the particular specification of routines built into the simulation model presented here has the advantage of a stochastic component that we believe has descriptive validity. The inclusion of this stochastic component increases the confidence that the findings extend beyond the somewhat deterministic rendition of technology management required by any mathematical representation. Finally, the simulation model and sample analysis presented here have been explicitly longitudinal, which is important for the future development of theory in the management of technology (Scott 1990: 137). These three advantages outline the promise of using simulation methodology for theory development. Closer study of a clear set of routines in a setting that is both stochastic and explicitly longitudinal offers a real possibility for advancing our understanding of the management of technology.

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Recommendations for Managers

We derive two general implications for managers from our study. First, we believe that our study suggests ways in which simulation methods can provide a valuable tool for analyzing organizational strategy and policy with regard to technology, particularly in terms of assessing both intended and unintended effects. Second, the results of our illustrative simulation point to more substantive issues concerning the effects of organizational inertia and environmental ambiguity on technological innovation.

The clearest way in which simulation methods can assist technology managers is by aiding the processes of strategic decision making and planning. Once managers have reached a point in either of these processes where they believe that they have a working model of their situation, we suggest that creating a computer version of this model would be worthwhile. Having done this, they can examine the effects of varying both the variables under managerial control as well as assumptions about parameters of the model that are exogenous to the technology management process. The long-run implications of different choices can be examined using simulation methodology at a lower cost in terms of resources and time than actually implementing these same choices. Moreover, a computer simulation can easily be adjusted to allow managers to determine the effects of exogenous parameters as well as examining how these interact with different managerial actions.

The second point involves more substantive questions of how different organizational factors and environmental characteristics may affect technological innovation. The illustrative simulation we presented offers one piece of counterintuitive advice: higher organizational inertia may lead to more technological innovation, while lower organizational inertia may result in more refinements to current technology. This suggests that management may well be cautioned about the advisability of searching too quickly for more innovation opportunities in the wake of a recently adopted innovation; under these circumstances, search will be costly and may be directed at a relatively poor pool of opportunities. In addition, the illustrative simulation indicates that conditions of high environmental ambiguity may make it difficult for organizations to make technological innovations, particularly in the long run.

At the same time, it must be acknowledged that the simulation methodology is not without its limitations. More specifically, the simulation tends to treat organizations as homogenous units, without conflict, and to simplify the complexities that underlie organizational learning and decision making (Glynn, Lant, and Milliken 1994). These constraints on the methodology notwithstanding, however, simulations offer a feasible way of exploring the consequences of different types of innovation policies. They also offer a powerful tool for the development of theory and substantive implications for the management of technology. In both applications, using computer simulations to study the management of technology suggests that an important role of organizational leaders is to manage the interaction of rule-based organizational systems with complex environments. Simulations can be a valuable tool for studying variations in rules and different assumptions about environmental contingencies.

Issues for the 21st Century

As a study of how computer simulations might be used to study the management of technology, this article is suggestive of how technology (i.e., simulation methodologies) can be employed in the study of the management of technology. As such, it raises some expectations and concerns for researchers and managers who may employ the methodology and/or be affected by the findings. Keeping pace with continuing developments and improvements in computer programming and related technologies will present challenges and opportunities. With more sophisticated—perhaps even more 'user friendly'—modeling techniques, some of the current limitations of simulations can be overcome. For instance, new possibilities may emerge for modeling organizations as complex systems that interact with other organizations and their technical environment. Such simulations would yield important information on technological forecasts, competitive positioning, and the diffusion of technologies for strategic planning and the management of technology. We might even see commercially packaged simulation methods widely available and accessible to nearly every executive, much like the ubiquitous 'spreadsheet' software of today. Certainly, with the ever increasing complexity of technologies, organizations, and environments, such tools offer some welcome assistance in the management of technology. However, questions arise concerning the limits of simulation technologies as well as their potential risks.

Somewhat paradoxically, while computer simulations may extend the horizons of managerial information processing, they are themselves bound by the managers' information and cognitive processes. While we are all quite familiar with the 'garbage in, garbage out' constraint of any computerized program, this idea is no less applicable to simulation methods. Simulations focus on the routinized and rule-bound aspects of organizational dynamics. However, their most powerful use will come in helping managers to seriously explore change—alternatives to current routines, how to adapt to changing environments, and how to develop structures that can cope with a wide range of environmental contingencies. If the methodology is decoupled from the managerial insight and intuition that underlies these routines, it might be useless or even misleading. Articulating the premises and algorithms which drive the simulation is critical; without this understanding, simulations can become meaningless or even misappropriated tools that can be used to rationalize or even compensate for poor strategic planning. Perhaps the biggest danger will come about if the methodology is used with the sole intent of justifying the status quo. Computer simulation will do little to advance the management of technology by being used as yet another tool to stifle change and support the continuation of current routines. Perhaps the greatest misuse of simulation methodology would be to build oversimplified scenarios based on narrow interpretations of the present with the intent of demonstrating that favorable outcomes will result from continuing current policies.

Appendix

Choice of Parameter Values

Copies of the actual program, which was written in Turbo Pascal, can be obtained

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by writing to the authors. Two basic considerations were most fundamental in the choice of parameter values. First, for reasons of cumulative knowledge building, many of our assumptions about search processes and parameter values correspond to Levinthal and March (1981). This contributes to cumulative knowledge because our program is based on their program and results. Our results can be seen as a direct extension of theirs. The cumulative relationship between Levinthal and March (1981) and this study is further reinforced because this is an entirely original program written in Turbo Pascal. Since Levinthal and March (1981) used Basic, similarities in the conclusions demonstrate that they do not depend on choice of computer language. Second, we relied on Tushman, Newman, and Romanelli's (1986: 710) characterization of incremental adjustment: 'A popular expression is that almost any organization can tolerate a "ten percent change." . . . these changes are still compatible with the prevailing structures, systems, and processes.' Thus, parameters meant to capture routine adjustment were set at 10 per cent based on an empirical tendency for such adjustments to be near that level.

We also decided to initialize the simulation as if each unit had been founded in the period prior to the first. Thus, time since adoption of the current technology is set to zero, and the search and change clocks of the unit are reset to zero. The unit is moved to the bottom of its learning curve on the current technology. Also, the unit incurs maximum search costs, and since it has no prior experience with either innovative or refinement search, it does not have the requisite experience to begin lowering the costs of performing them.

Operationalization of Search Routines

The discussion of the operationalization of search routines will follow the flow chart for search given in Figure 1. The window of no change imposed by the search clock of the unit is set to two periods; prior to the third period following the adoption of an innovation, the unit does not search. An amount equal to the total resources devoted to search in the period of innovation is deducted in each of these periods but there is no search or change. The rationale is that all of the resources devoted to search are now devoted to implementing the new innovation.

Both innovative and refinement search are draws from uniform distributions. Innovative searches are draws from a uniform distribution with a range that is proportional to both the underlying potential of the current technology and the square of the time since adoption of that technology. More specifically, the range of innovative search, R^{i}_{it}, is defined as follows:

$$(1) \quad R^{i}_{it} = \pm(Ptl_{it} + \tau^{2})$$

Ptl_{it} is defined as the underlying potential of the technology used by unit i at time t; following Levinthal and March (1981), it is set to 50 in the first period. τ is defined as the count of the number of periods since the adoption of the most recent innovation. Thus, the mean value of technologies discovered by innovative search is always zero, since the distribution is symmetric around zero, but the variance increases with the range. More specifically, the variance of a uniform distribution is equal to the square of the range of the distribution divided by twelve. Since the range increases with the square of time since the last innovation, so does the variance. As a result, the probability that the best technology discovered by innovative search will be an improvement over current technology increases with time since adoption of the current technology.

Refinement searches are draws from a uniform distribution with a range that is proportional to the value of the underlying potential of the current technology. More specifically, the range of refinement search, R^r_{it}, is defined as follows:

(2) $R^r_{it} = (1 \pm \delta_{it}) \times Ptl_{it}$.

Ptl_{it} is as defined above. δ_{it} is defined as follows: in the period immediately following an innovation, δ_{it} is set to 1/8 and remains at that value until three refinements have been adopted. From that point onwards, δ_{it} is set to $1/(TR^2_{it})$, where TR_{it} is the total number of refinements made to current technology since adoption by the unit. Thus, the mean difference in the value of technology discovered by refinement search and current technology is always zero. However, the variance of this difference decreases with the number of refinements to current technology that have already been adopted. Thus, the probability that a refinement will offer an improvement over current technology decreases with the number of previous refinements to current technology.

To initialize the simulation, the costs of both innovation and refinement search are set at the levels used by Levinthal and March (1981). However, we departed from their strategy of leaving the cost of search constant for the remainder of the simulation because it would have the effect of making search increasingly cheap in real terms as the organization grew. While this effect may not have been important in their simulation, which they ran for only twenty periods, we felt that in a longer time frame this might present a problem. For this reason, we fixed the cost of search as proportional to the value of the current technology. Following Levinthal and March (1981), the initial value of the minimum cost of innovative search is set at 0.0135×50; the initial value of the potential of technology, and the minimum cost of refinement search is 0.01×50. Units start out with a cost of search equal to the minimum cost of search raised to the power of 3/2. Initial values of the minimum cost of search are less than one resource unit. For values of cost of search less than one resource unit, the cost of search is multiplied by ten, raised to the exponent appropriate to their experience, and then deflated. This avoids the problem that squaring quantities less than one would have the opposite effect of that intended. With each search they perform, the exponent on the minimum cost of search decreases one half of the remaining distance between its value and one. This results in the cost of search decreasing with the number of searches, but at a decreasing rate. When a unit does not search in a particular period, the cost of search increases at the same rate that it decreases when the unit does search. Thus, the functional form of decay along the search cost curve is the obverse of the functional form of the decrease in search cost.

The assessment of past search proceeds as described in Table 1. The decisions involve three variables: Total Search Potential, TSP_{it}, Innovative Search Potential, ISP_{it}, and Refinement Search Potential, RSP_{it}. Following both Levinthal and March

(1981) and the 10 per cent rule, we operationalize increases and decreases to search resources as follows: if the assessment of total search is that it has been associated with failure, then TSP_{it} is reduced by 10 per cent; if it has been associated with success, then TSP_{it} is increased by 10 per cent. The assessment of innovative search and refinement search are operationalized identically. Actual resources available for search are defined by two equations. The first defines innovative search resources, ISR_{it}:

(3) $ISR_{it} = TSP_{it} \times ISP_{it} \times TP_{it}$

where TP_{it} is defined to be the actual performance the unit achieved with its technology in the most recent period. The second equation defines refinement search resources, RSR_{it} in the same way:

(4) $RSR_{it} = TSP_{it} \times RSP_{it} \times TP_{it}$

where TP_{it} is as above.

Actual resources devoted to search depend on the assessment of performance in the following way: if performance meets or exceeds aspiration level, then $RSR_{it} = RSR_{it}$ and ISR_{it} is left as it is. Conversely, if performance is below aspiration level, then $ISR_{it} = ISR_{it}$ and RSR_{it} is left as it is. This assessment rule replicates Levinthal and March (1981) and roughly satisfies the 10 per cent rule since raising the level of resources to this power is equivalent to an adjustment of 9.1 per cent. The allowed number of searches is determined by taking the resources to be devoted to each type of search, dividing by the cost of that type of search, and rounding to the nearest integer. After this transformation, the final amounts of all three search resources are recomputed so that ISR_{it} equals the number of innovative searches times their cost, RSR_{it} equals the number of refinement searches times their cost, and TSR_{it} equals the sum of ISR_{it} and RSR_{it}. The unit takes a number of draws from the appropriate distribution equal to the number of searches. To operationalize myopia with respect to new technology, the value of innovative draws are deflated by raising them to the 0.75 power. The unit stores the best result of either type of search for a later comparison with current technology.

Operationalization of Performance Routines

The discussion of the operationalization of performance routines will follow the flow chart for performance given in Figure 2. As indicated in the figure, the value of technological potential drifts in each period, the draws from drift are uniform on the interval (−0.1,0.1), thus the value of technological potential in the current period is in between 90 and 110 per cent of the value of the technological potential in the previous period.

Movement on the learning curve is as follows: the technological performance of the unit, TP_{it}, is a function of the underlying value of the potential of the current technology. The form of this relation is as follows: $TP_{it} = Ptl_{it}^{\lambda\tau}$. The subscript τ on λ is meant to signify that it is a function of time since adoption of the current technology, not the count of simulation periods signified by t. In the period of adoption of the new technology, λ_0 is set at 0.75. In all subsequent periods until adoption of the next technology, the value of λ is set by the following incremental formula:

$$(5) \quad \lambda_t = \lambda_{t-1} + ((1 - \lambda_{t-1})/2).$$

Thus, in the first period following the adoption of a new technology, the technological performance equals the potential of the technology raised to the 3/4 power. In each subsequent period where a new technology is not adopted, the power to which technological potential is raised increases one half of the distance between the previous exponent and one. In keeping with empirical data, the rate at which performance increases as a function of experience decreases as a function of time. Refinements change the value of the underlying potential of the current technology but do not affect the learning curve; thus, the exponent to determine performance is unaffected by refinement. Innovation, here defined as the adoption of a new technology, automatically resets the exponent to 3/4, and movement on the learning curve begins anew.

Performance, P_{it}, is defined as follows:

$$(6) \quad P_{it} = TP_{it} - ISR_{it} - RSR_{it}.$$

Thus, performance is determined by how well the unit does with its current technology minus the costs of all innovative and refinement search.

Aspiration levels, AL_{it}, are set using the attainment discrepancy model (Lant and Mezias 1990, 1992; Glynn et al. 1991; Lant 1992):

$$(7) \quad AL_{it} = \beta_0 + \beta_1 AL_{i,t-1} + \beta_2 (AL_{i,t-1} - P_{i,t-1})$$

The parameter β_1 determines the level of incrementalism in aspiration level updating while the parameter β_2 determines the responsiveness of the process to performance feedback. The actual values of the β's used are uniform on the range of the highest and lowest values of each parameter estimated by Lant (1992). Not only are the values of these parameters empirically derived, but the randomization across the range of values ensures that the results are robust to the choice of any value in this range.

Operationalization of Change Routines

The discussion of the operationalization of change routines will follow the flow chart for change given in Figure 3. The change clock of the unit is set to two periods; for two periods following the adoption of an innovation the unit cannot change. Performance is assessed relative to target by comparing P_{it} and AL_{it}. If performance equals or exceeds the aspiration level, the probability of change depends on whether the best alternative technology found through search is an innovation or a refinement. The differentiation ensures that the perception of an extraordinary opportunity that is the driving force of slack change (Lant and Mezias 1990, 1992) is sensitive to the different distributions of the underlying opportunities represented by innovative and refinement search. For innovations, the probability of change equals the difference between the performance with current technology and the best technology found through innovative search divided by the number of periods since adoption (cf. equation 1). If performance equals or exceeds the aspiration level and the best alternative technology found through search is a refinement, then the probability of change equals the best refinement draw divided by the total number of refinements to current technology squared or 8 if there have been fewer than three refinements (cf. equation 2). If performance is less than aspiration level, then the actual amount by which performance falls below aspiration level is divided by the minimum value for attainment discrepancy among all fifty units, i. e. the largest negative attainment discrepancy, to obtain the probability of change.

To determine whether a unit changes in a given period, a draw is taken from a binomial distribution with the probability of success equal to the probability of change determined in the previous step. If that draw is a failure, then the unit exits the change routines. However, if the draw is a success then the unit proceeds to evaluate the available alternative technologies found through search. If no alternative superior to current technology is available, then the unit exits. Superiority is determined by subtracting current technology from the available alternative; any value greater than one resource unit precipitates adoption of the alternative. If the alternative is an innovation, then the unit is coded as having innovated in the current period. If the alternative is a refinement, the unit is coded as having refined its technology in the current period. Then the program exits the routines for change, increments the period, and starts the whole process over again for each unit.

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Open Peer Commentaries on 'Computer Simulations in the Management of Technology'

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I found this to be an outstanding article that successfully grapples with an extremely difficult topic. The concept of developing a management of technology theory based on a simulation model struck me as unrealistic at first sight. However, the authors make an excellent case for their concept and methodology.

I particularly applaud the organization of the paper which begins with an overview of computer simulations in general and progresses to its use in a management of technology environment. The simulation environment is carefully defined and the authors do a superb job describing how the simulation algorithms might model a firm's adaptation of a given technology. Consideration is appropriately given to the decision processes faced by most firms, including the firm's ability to react to sequential technological change. That is, firms will be less likely to embrace new change immediately after undergoing a change. Also included is the concept of decreasing returns which can result from utilizing increasing amounts of the same technology.

The paper focuses primarily on the development of a simulation model to capture technology adoption processes. This effort meets with complete success. I do suggest, however, that subsequent extensions of this work elaborate on the concern expressed in the paper regarding the role of assumptions in the development of the model. Further, an addition of a simulation routine considering the explicit impact of potential technological bandwagon effects might be included.

Using and Understanding the Model

The concept of using technology to study technology, while intriguing, is not without its problems. Certainly, the most significant possible drawback to the use of a simulation model in general is that someone must first develop the assumptions under which the model is run and developed. The authors consider this possibility and that the use of a simulation might lead to useless or even harmful results and decisions. While the possibility of poor decision making exists when reaching any decision, the use of a computer model may actually accentuate it. Additionally, the authors point out that the model can be used to hide facts from decision makers and this blindly support the status quo.

To avoid this issue, model builders will hopefully perform rigorous simulation validations, resulting in the identification of acceptable Type I (the correct hypothesis is wrongly rejected) and Type II (a false hypothesis is accepted as true) error levels. However, a third, more severe error, is still possible. Referred to as a Type 0 error, this error occurs when the model builder misses the mark completely by asking the wrong questions (Pidd 1992: 108). It is possible to look to past technological breakthroughs to gain insight into the Type 0 error. The author's expectation that theory development simulation software might become as pervasive in the future as spreadsheet software is today provides an excellent example.

Early spreadsheets were rightfully hailed as a marvelous tool for managerial planning. However, users of the spreadsheets found that often the output of the spreadsheet appeared more credible than should have been the case. The spreadsheet process showed only the results, not the details and assumptions behind them. Consequently, the spreadsheet results could be seen as less fallible than was really the case and decision makers were tempted to accept the output as truth, particularly if these results tended to support a favored position. The resulting Type 0 error led to several celebrated situations where inaccurate spreadsheets were used to reach erroneous decisions that later proved harmful. Simulation modeling is certainly more complex and less intuitive than the typical spreadsheet. As a result, managers may blindly use them, ask the wrong questions, and in a fashion similar to an erroneous spreadsheet, make faulty decisions. This situation can quickly lead to a reluctance by decision makers to use the model, but can be overcome as suggested by Heinze:

> 'Policy makers too often are intimidated by the screen of technical jargon, and do not ask the penetrating questions that they should. There is no model that cannot be explained reasonably and simply if the author desires. The burden of proof of the worth of any model must therefore be on the author, rather than those who are attempting to understand it. Otherwise the worth of such models can never be accurately judged, and the gulf between model makers and decision makers will never be bridged.' (Heinze 1982: 39)

The authors point out this possibility and perhaps correctly do not extend this line of thought; however, as is the case with any powerful tool, a simulation is only as good as the user or designer. This is especially true when attempting to use this tool for theory development since the user is not fully sure what to expect. The bottom line is that the designer of the model must build in appropriate routines for the end user to make validation possible and easy to carry out.

Riding the Bandwagon

Observation of the technological adoption process of firms suggests that many firms adopt a new technology simply because it is in fashion to do so. This represents the 'bandwagon effect' in which new technologies or innovations are adopted due to external enthusiasm (Abrahamson and Rosenkopf 1993: 487–517). The bandwagon impact may be particularly important when considering technical innovations surrounded by substantial ambiguity in that managers may rely less on their own insights and more on the perceived knowledge of other technology users. The bandwagon concept should prove to be a fertile and interesting area for an extension to this initial research.

The concept of using a simulation to develop management of technology theory is an exciting one and is well executed in this paper. The authors are to be complimented on a reasoned, well-researched article that certainly advances management of technology concepts. However, as the authors themselves point out, this concept is a double edged sword that can be lead to inaccurate results if carelessly used and perhaps worse, can be deliberately used to avoid needed change.

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Limitations and Risks of Numerical Simulation Models—the View of an Atmospheric Scientist

In the field of technology management, computer simulations are a tool which is still in its infancy. In contrast, the atmospheric sciences were, after the military, some of the earliest users of programmable computers and they now rely heavily on numerical simulation models. Because computer simulations are so widerspread in the atmospheric sciences, and because they now have a history of about half a century, the values as well as the limitations and risks associated with this tool have been experienced (sometimes unexpectedly), explored, and discussed in some depth in this community. It is an interesting perspective for me as a meteorologist to look at a field which is—maybe—at the beginning of such a development.

The earliest and still one of the most important applications of numerical modeling in meteorology is weather forecasting (Haltiner and Williams 1980). Modified versions of numerical weather prediction models, so-called general circulation models, are used for studying climate change (Schlesinger 1991). They are now merged with models of the ocean circulations (Trenberth 1993). Computer models are also widely used in the simulation of dispersion and transformation of atmospheric pollutants, from the effluents of a single stack, to regional air quality models, and models of the global atmospheric chemistry (Zanetti 1990).

Terminology: Model, Simulation, Numerical, Computer

I think it is useful to start the discussion with some remarks on terminology. How do the terms 'model', 'simulation', 'numerical', and 'computer' relate to each other? The starting point is always a model. The model is our simplified and probably flawed representation of reality. If we want to transform the model into a computer program, we have to formulate the model quantitatively, preferably as a set of equations and associated rules which form the algorithm. If the problem is not too complicated, these equations can be solved analytically, i.e., the dependent variable(s) can be expressed as an analytical function (formula) of the independent variable(s). Sometimes, this formula is so complicated that we want to use a programmable computer to evaluate it for a certain range of the independent variable(s) and parameters, but this does not lead to a numerical model. We speak about a numerical model when numerical techniques are used to solve the equations. Very often, especially in the atmospheric sciences, these equations are differential equations with time and space as the independent variables. The differential equations then have to be transformed to finite difference equations which are the core of the numerical model. The implementation of the numerical model on a computer, using a programming language, finally creates the computer model. The final step of 'coding' is, however, the least complex one (though achieving good performance and avoiding errors may require considerable technical skill), and therefore we prefer the term 'numerical model' to 'computer model'. Finally, the term 'simulation' usually points to a process evolving in time, and indicates that the model aspires to be similar to the real process.

Problems Associated with Assessing the Quality and Reliability of a Computer Model

According to the article by Mezias and Glynn, the use of computer simulations in management is still in an experimental stage, and they are trying to convince their community of the usefulness of this tool. As an outsider to this field, I cannot comment on the perspectives in terms of performance and possible refinements of these types of models. I can, however, point out some major risks and difficulties associated with the numerical modeling of complex processes. The authors are obviously aware of them and have included some warnings in their paper. From the background of the developments in my field, I think, however, that the risks can hardly be emphasized enough, and tend to be ignored once numerical models have reached a certain level of popularity. I am aware that some of the following remarks are less valid in the relatively simple model used by the authors. However, if the aim is to model the complex process of technological development, the future models will certainly be highly complex, too.

The key issues certainly are the quality of the model and the quality of the model input, and how we deal with the imperfection of the simulations.

In the early stages of development, a simulation model may be very crude and practitioners may hesitate to use the model output as they feel that their conventional, often intuition-based methods are superior to the model. With further improvement, and as new people appear who have never worked without the model, the opposite may happen: blind belief in the model. As a result, careful comparison of the model output and reality may be neglected, and if such comparisons are difficult or impossible, users may even mistake the model for reality. I have seen model developers claiming that their model is 'complete' in that all important processes have been included, while this obviously was not the case.

An essential step in achieving confidence in a simulation model, and also in improving such a model, is the comparison of the model output with reality. However, this is not at all trivial. We can hardly judge the performance of the model on the basis of common sense and simple reasoning (except if the model is very wrong) because, typically, such models are applied to situations which are too complex for such judgment (that is why we wanted a computer model in the first place). One of the most severe limitations of testing a model is that the model is designed so simulate a subset of the processes occurring in reality—a complete 'world model' is beyond reach. Observations and field measurements will therefore always be affected by features that the model was not designed to resolve, and so the question of the representativeness of the observation arises. To give a meteorological example: numerical weather prediction models have a horizontal resolution of the order of 100 km; the measurements at a meteorological station are, however, affected by hills, forests, water bodies, cities, and so on, with spatial scales of only a few kilometers. In the management area, the development of a unit may be influenced, for example, by the exchange of a key person. This unforeseeable situation is therefore not included in the model. 'Blindered' modelers may sometimes dismiss observations contradicting the output of their model as being 'not representative,' as it is almost impossible to determine the representativity of an observation without additional field work. The issue of the accuracy of the measurement adds to this problem.

Another essential problem in model development is that if the model has reached a certain level of complexity, it can never be fully tested. Imagine a model with 10 parameters (be they intrinsic, adjustable model parameters, or be they input variables such as initial and boundary conditions) each of which can have 5 different values. Then there are 5^{10} or about 10,000,000 different situations—it is obvious that such a parameter space can never be really explored unless one model run takes only the fraction of a second (10,000,000 seconds are about 115 days!). This problem applies also to sensitivity studies. I agree with the authors that sensitivity studies are very desirable and valuable, but often it is impossible to make them really comprehensive.

As every programmer knows, large computer codes are prone to bugs. Most of these bugs are eliminated during the testing phase, but some will probably remain. Experience shows that they will not severely affect most of the results, but one can never be 100 percent sure that this is the case in the specific result one is looking at.

Though it is common to speak about the validation or verification of a model, we have to admit that a complex model can only be subjected to some testing which makes its validity more or less plausible; we are not able to prove its general validity.

The more complex a model is, the more input is needed. Often it is difficult to obtain appropriate input and the performance of the model may be limited by the quality of the input and not that of the algorithm. This situation points back to increased field work which may have been neglected in phases during which understanding could have been obtained more easily and faster from modeling work. The fact that academic rewards and career advancements are mainly based on the number of publications, without much consideration of how much work was necessary to obtain the results, contributes to such effects.

Some Remarks on Techniques in Numerical Models

Let me briefly touch a more technical aspect of numerical simulation models which seems not to be considered by the authors. The numerical integration of time-dependent equations requires the specification of the length of the time steps. The so-called truncation error, i.e., the error introduced by the discretization, becomes larger with an increasing time step. The problem can be illustrated using the simple situation that the rate of change of a variable is proportional to its actual value (this is the differential equation of exponential growth or decay). If the amount of change during one time increment is determined using the value at the beginning of a time step, the amount of (a positive) change is obviously too small. Under certain circumstances, a small increase in the time increment will not simply cause a small increase in the truncation error, but—if a threshold value is exceeded—it will trigger a so-called numerical instability, resulting in the exponential growth of error terms which will destroy the results (Pielke 1984). This was the reason why the first attempt of a numerical weather prediction by Lewis Richardson in the 1920s failed. In short, every numerical simulation should take care that a time step is used which ensures a stable and sufficiently accurate solution.

Another technical aspect which may be of interest in the context of the article under discussion is the potential of fuzzy methods. Methods based on the theory of so-called fuzzy sets have been successfully applied to situations in which (relatively simple) human decisions are to be replaced by computerized ones (Kosko 1992). Fuzzy methods replace the binary logic by a continuous band between 'yes' and 'no'. I imagine that this technique could be quite appropriate to describe the behavior of such organizational units that are the subject of the article.

The question remains, however, of how well the processes to be simulated in the context of technological development and its management can be quantified–fuzzy methods are also quantitative methods. It is certainly no coincidence that the authors do not give units of their variables–but if there are no units, how can they be quantitatively determined?

Computer Simulations and the Public

In communicating model results to lay people, the problem is not so much one of making the results understandable, but rather the misuse of technology (in this case high-performance computing) to mislead the public or other lay audiences about the reliability of the results. Sometimes the argument used, at least implicitly, is that 'such a sophisticated model, run on such a powerful computer, cannot be wrong, and in addition, non-specialists lack any basis to raise doubts about it.' Batches of punched cards, stacks of magnetic tapes and printed tables were once used to impress people. Nowadays, colourful pictures, often 'animated' as movies are used to give the impression of infallibility and accuracy. A better use of technology (computer technology in this case) would be to develop tools which make uncertainties–e. g., as obtained from sensitivity studies–more readily comprehensible.

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Examining the management of technology is important and is becoming even more important because information technology (IT) influences all levels of organizational activities. At the operational level it influences the performance of work tasks; at the middle management level, planning and controling task performance, and at the upper management level it is a strategic tool. This is also a period of fast technological change so there is a need of theory building for studying interdependencies between technology and the individual, technology transfer, and the sociology of technology.

The article by Mezias and Glynn presents a novel approach using computer simulation as an explorative research methodology to study the management of technology. It should be observed that this study is focusing on theory building and not on theory testing, contrary to the traditional use of mathematical research approaches.

The primary contributions of the paper according to the authors are twofold: they argue that computer simulation methodology can advance theory building in the study of technology management, and they demonstrate the applicability of simulation methodology by modeling the management of technology as routine based. I agree with the relevance of studying the management of technology and the need for theory building, but I might argue against the applicability of the simulation model described in this paper, although not against using simulation techniques as such.

Computer simulation, its pros and cons generally, and especially for this study, are clearly and honestly discussed. The major strengths of simulation are described as capabilities for performing dynamic, complex, theoretical analyses, for capturing two central but methodologically bothersome features of technology, uncertainty and complexity, and for modeling dynamic, interactive, and complex processes with stochastic disturbances or random error. The authors state that the strengths of computer simulation methodology coincide with the need for theory building in the field of technology management.

Computer simulation is considered a particularly appropriate methodology when the system under study is complex, when there is a need to conduct sensitivity analyses concerning the behavior of the system, and when performance assessments are measurable in quantitative terms. Organizations, where technology management belongs, certainly are complex structures. The authors have not performed any sensitivity analyses because the applications were for illustration only. Nevertheless, they do suggest what type of analyses might be useful. However, the quantitative measurement of how well organizations manage technology is something I would question. There are so many qualitative aspects involved in measuring the behavior of organizations and people. Quantitative measurement is certainly informative but not comprehensive enough.

The authors also discuss the limitations of simulation. The basic issue is the researcher's assumptions. In the paper, they clearly state the assumptions which form the foundation for the simulation model. Knowledge of the assumptions gives the reader a fair chance to judge the appropriateness of the model from his/her point of view. Before discussing my judgment, I will first pursue the authors' point of view. They point out that a computer simulation is not a substitute for empirical research, but a complement to it. I would certainly agree with this. Whicker and Sigelman (1991: 17–18) state that computer models can simulate 'any system that can be represented by symbolic terms and logical processes.' This statement is certainly true but I question the possibility of representing organizational processes by symbolic terms and logical processes only.

The authors directly address the issue of using simulation methodology to build theory. On the positive side, there are savings in cost compared with many other methodologies such as case studies or experiments. On the negative side, they list three problems put forward by Whicker and Sigelman (1991: 30–31). Incidentally, using an introductory book on simulation like this one as a central reference, could also be questioned. The problems they mention are omission of details, the difficulty of communicating effectively with those unfamiliar with computer simulation, and the effort to try and verify the model. None of these problems is specific to simulation, but to all mathematical modeling.

As mentioned above, the very basis for model building is the researcher's assumptions about appropriate underlying theory. In this research, the assumptions are about technology and organization. The authors adapt an organizational rather than a purely technical perspective on technology. They identify two types of technological change, incremental and radical, with different challenges to the management of technology. They exclude from the model issues such as technical complexity, compatibility with established organizational practices, the source of change, whether technical or administrative, and whether it comes from outside the organization or is internally generated. These exclusions can be quite limiting with regard to the organization which is being modeled. Another limitation in the usefulness of the model lies in the type of organization modeled, a unit of a large bureaucratic firm. The problems studied might be more accentuated in this type of firm but organizational structures including the management style are changing to more flat, flexible, and ad hoc types. Thus the model will become of less practical value in the future. The assumptions taken and those excluded are based on theories, but these chosen theories are not the only ones.

The most central assumption about organizations is viewing them as experiential learning systems which can alter their routines in response to past experience. They are considered as 'routine-based, history-dependent, and target-oriented' (Levitt and March 1988). Three typical categories of routines are identified; search, performance, and change.

The model and its design are described in detail. The model is then used to answer some sample questions and the results are analyzed. All this is done in accordance with how simulation models should be developed. The problem, as I see it, is that the results are the results of running the simulation model and these are not related or compared to what happens in real organizations in corresponding situations. In other words, the verification of the model and its behavior is not done properly in order to propose the model as a practical tool for managers. So far, it is a purely theoretical model. However, the authors do discuss the importance of the generalizability and external validity of the results. Although they have tried to address this issue by including 'empirical measures of parameters whenever possible' and by building 'most of the program as a replication of the work of Levinthal and March (1981)', these measures are not enough to verify the useability of the model, as I see it.

With my computer science background I have few 'complaints' about the computer simulation model and the design process. On the other hand, there is very little new about the simulation model from the computer science point of view. My information systems background poses further questions, but it is in this context that the article offers new knowledge to the research field.

Mathematical research, including computer simulation as dealt with in this article, is theoretical research. Such research is based on assumptions. Mathematical models which are designed, seldom have direct connection with reality. Their behavior is usually correct and can easily be verified by other researchers. Therefore the validity of the model depends on how the assumptions are selected and how the results are interpreted. The central question, from the practical point of view, is how well the model reflects reality.

In this research, the assumptions are theory based. However, the simulation models organizational processes which are real-world events. To verify the model I would have appreciated some empirical validation of both the assumptions on which the model is based and the results of the sample questions.

'A computer simulation takes a complex set of assumptions, simulates a set of organizational processes, and represents the implications of these processes for organizational outcomes.' These are the ground rules and the basic limitations of simulations. As already mentioned, I am dubious about the sample results. Whereas they reflect the behavior of the simulation model, it is not possible to say anything about the behavior of the organization. The sample results represent implications for organizational outcomes, but only to a certain extent. Organizational processes are performed by people who react on the results, but how they will react is impossible to predict just by means of computer simulation.

Some assumptions are also questionable. The model is based on two types of technological change, radical and incremental, each posing different challenges to the organization. I do not agree with how these types of changes are characterized (see Tushman and Nelson 1990; Henderson and Clark 1990), the first being considered primarily positive and the other one rather negative or at least demanding to the organization. Much research shows that even minor changes can be negatively received, while radical changes may be well accepted by the users, depending on how they were developed, implemented and introduced (see e. g. Bjorn-Andersen et al. 1986; Bodker et al. 1987; Nurminen 1988). Although the modeling in this case is not influenced by these characteristics, but the interpretation of the results might be.

The basic assumption which, for me, is the most confusing is the simplification of organizations into experiential learning systems which change their routines in response to past experience. This is a very deterministic view which can be modeled by simple feedback loops. Organizations certainly can change their routines, if they so decide, but they may decide against it, and this non-deterministic interpretation is not modeled. Organizations are goal-searching systems. The deterministic interpretation, however, parallels them with self controling/regulating systems (such as the thermometer or the heart, for example), which I do not agree with. Organizations are composed of people and are therefore self steering. Self-steering systems are the only systems which include creative thinking, situational understanding and unbounded intellectual goals. All human qualities are essential in an organizational context as soon as we start talking about learning (Jarvinen and Jarvinen 1993, re: Aulin 1989).

In this case, the model simulates a set of organizational processes. A question that can be asked then is: If organizational processes are to be modeled in a realistic way, how much should be excluded? Is it enough to build in some stochasticity? What about the influence from the outside world? I am not saying that simulation is not good, but, in order to be useful, it must be followed by empirical research, e. g. field studies, and be validated in real organizations.

In conclusion, this is an interesting article in which the topic is certainly relevant and where simulation is used in an exploratory, unusual way to study research questions.

Simulation methodology is shown to advance theory building in the management of technology, although testing the theories has to be postponed and supported by empirical research. The findings illustrate how simulation technology can also be used to address questions of potential interest to practitioners.

The authors have interesting recommendations for future research, many of which address my reservations. They suggest comparing different strategies for the management of technology and studying variations on the model. Such variations could mean considering the consequences of more flexible organizational structures and management style. The idea of using simulation methodologies to address questions of level of analysis; individual, group, organizational, and inter-organizational levels, is interesting, but probably quite a difficult and demanding task. To me the most important extension of the research would be to combine computer simulation methodology with field research.

The model is purely theoretical in its consideration of why managers' interests might be rather limited. In the future, it could be developed into a decision-support tool for analyzing organizational strategy and policy with regard to technology management. As the authors point out, computer simulation methodology is useful for such analysis; exploring change alternatives, adapting to changing environments and developing coping structures and strategies. It is, however, important to remember that **the model is not the organization** and therefore the interpretation of results is crucial.

Organizations are complex structures and modeling them requires both high-quality hardware and software, including good design tools, but also knowledge and interest. Technological development will improve the first mentioned components and these in turn may reduce the limitations of simulation. The research presented in this paper promises improvements in the last components too.

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The Stimulation of Simulation

Mezias and Glynn have targeted a vastly under-utilized research methodology in their paper on the use of simulation for theory building about the management of technology. They argue that simulation is amenable to capturing the uncertainty and complexity that characterizes technology. The authors first introduce the general aspects of simulation in addition to its strengths and weaknesses as a tool for theory development. The second part consists of an overview of the management of technology in organizations and the use of experiential learning systems as a model for the management of technology. In the third part, they translate their descriptions and assumptions into a simulation model. The results of running the simulation experiment with the model to answer a particular set of questions are discussed in the fourth section. A fifth section is devoted to recommendations for future research and managerial considerations.

Methodological and theoretical themes are engaged productively by the authors to serve as an example for researchers interested in the use of simulation singularly or as a complement to other methodologies in the study of the management of technology. Mezias and Glynn challenge researchers to think about how technology is conceptualized by exploring different theories with simulation.

My comments primarily address concerns for researchers pursuing simulation to explore technology management with the metatheoretical assumptions that theories of the management of technology might embrace; the use of simulation with case contexts to build theory; and the use of visualization to facilitate the analysis of simulation output. The first two areas are potential consequences of pursuing simulation work to investigate different theories of the management of technology. The consideration of visualization to analyze simulation output data is driven by the criticality of both the model output and its analysis as part of any simulation modeling study.

The paradigm membership of a theory of the management of technology is important insofar as it constrains the kinds of questions the theory addresses. There has been considerable interest in the perspectives which shape researcher's inquiries into organizational topics. Burrell and Morgan (1985) advanced a two-dimensional typology of paradigms on the basis of assumptions about social science and society. Notwithstanding debates over the basis of their scheme, it is nonetheless useful as an heuristic device for bounding the class of questions posed by a theory. Assumptions about the nature of social science include ontological, epistemological, human nature, and methodological assumptions. Assumptions about the nature of society are reflected in the regulation-change dimension. These two dimensions yield the four-fold typology of functionalist, interpretivist, radical humanist, and radical structuralist paradigms.

Mezias and Glynn employ an essentially functionalist theory of technology management which emphasizes the maintenance of the status quo. Ontologically, the theory entertains realism through its structures, independent of the individual, which exert their presence in decision processes. The theory is epistemologically positivist because it seeks explanations and predictions with causal relationships between its components. In terms of human nature, the theory is determinist to the degree that it acknowledges the effects of situational factors. Methodologically, the theory is nomothetic with reliance on systematic protocol and techniques.

With different metatheoretical assumptions, theories of the management of technology might be classified into other paradigms using the subjective-objective and regulation-radical dimensions of the Burrell and Morgan (1985) typology. Both functionalist and radical structuralist theories of technology management would be similarly positioned with objectivist assumptions about social science but located at opposite poles on the regulation-radical change dimension. A radical structuralist theory would be realist, positivist, determinist, and nomothetic. It would approach technology as a dominating force in an effort to change it. An interpretivist theory would be nominalist, determinist, and ideographic. A radical humanist theory of technology management would be nominalist, voluntarist, and ideographic. Both interpretivist and radical humanist theories of technology management might engage positivist science in simulation work with multi-actor environments.

A metatheoretical accounting of the technology management theory chosen for simulation requires consideration of the perspective taken, the possible questions to be asked, and the general content of the answers that may be offered. The simulation of functionalist, radical structuralist, interpretivist, and radical humanist theories of technology management is an open challenge for those looking to extend the work of the authors.

Theory development and testing with simulation entail reference to cases in some form through model validation. A model operationalizing theory diminishes the complexity of the situation at the risk of neglecting aspects of the case contexts that may help us understand technology. We need to carefully examine the extent to which models universally represent theory; incorporate contextual aspects of cases, and still retain features of the theory.

A crucial part of any simulation work is model validation which requires reference to empirical referents in case contexts. A problem confronting the modeler, is the extent to which the theory is generalizable and context-specific. Casti (1992) described theory construction as the formalization of an observed system N into a set of symbols and strings of a formal logical system F. A theory in the limit should approximate F. Case and modeling studies express F and approximate N differently. In a case study, F and N can be related in two ways. In one approach, the formal logical system F or theory is used to interpret the observed system N. In the other approach, an observed system N is the basis for building theory as the formal logical system F. Discrepancies between N and F can be reconciled through the comparison of F and N with the revision of interpretations of N with F or the revision of F derived from observations of N. In modeling studies, there is a danger that F will be translated into a simulation model without any requirement of an observed system. Each version of the model may be a different operationalization of the theory. Discrepancies between the simulation model results and F can be resolved through revisions in the simulation model. Discrepancies could arise between the theory (F) and the model as well as between the model and the observed system (N) obtained from a case context.

Recent literature in the organization area deals with the problems of theory building and testing. Bacharach (1989), Weick (1989), Eisenhardt (1989), and Osigweh (1989) underscored problems relevant to building a context-sensitive model with a theory distanced from empirical referents. Bacharach (1989) emphasized the standards of falsifiability and utility to assess the degree to which a theory explains and predicts phenomena. Since simulations stress falsifiability and the utility of case studies, simulation of a case context would strengthen the validity of a model. Bacharach's (1989) perspective on theory construction would be helpful in evaluating the potential for a model and hypotheses to explain and predict the management of technology. Weick's (1989) metaphor for the systematic permutation of ideas for theory development provides general guidelines for developing theory. Eisenhardt (1989) highlighted the use of cases and their theory building, descriptive, and theory testing implications relevant to the simulation of specific contexts. Osigweh (1989) analyzed the difficulties in making theory simultaneously generalizable and context applicable. His argument for the optimization of the generalizability and context-applicability of theory motivates an evaluation of the extent to which a model of a case context can satisfy these criteria. Comparisons and contrasts between these perspectives suggest a fundamental dilemma between the generalizability of a theory and its use in building a model to accommodate a particular context. The linkage of context-specific variables with the theory may preserve both the generalizability and connotative precision of the theory.

The authors presented, but did not emphasize, the graphic display of the simulation output. As a set of techniques for representing and viewing complex phenomena, visualization can complement numerical analysis of data. Visualization is a rapidly growing area in the physical and life sciences. The representation and communication of information through visualization seeks to exploit our ability to think visually so that different perspectives and insights may be gained. Most research on organizations has not taken advantage of visualization tools for the analysis of data. Technology may be a sufficiently complex phenomenon to motivate an assessment of its visualization potential. As the dimensionality of phenomena increases, it becomes more difficult to make sense of the numerical data. Duncan (1979), in his review of qualitative methods in strategic management, noted that policy research questions are complex, with a large number of variables. The same questions which Duncan claims the qualitative approach asks are applicable to the use of visualization. These questions are: (1) What's going on here?; (2) What are the forms of the phenomenon; and (3) What are the variations in the phenomenon? The question of whether or not visualization can complement quantitative and qualitative methods revolves around its potential to help in theory building and theory testing. Technology researchers should seriously consider the prospects for visualization to help them gain a better understanding of, and insights about, technology and its management.

There are problems with using standard statistical techniques to explore large dimension data sets. The dimensionality of such a space may be sufficiently large to thwart any direct visual inspection of the data. Additionally, the equidistance of the points in the data space are problematic for structure analysis techniques based on distance. Those interested in the principles behind the display of data might find Tufte's (1990) six design themes promising. Escaping flatland; micro/macro readings; layering and separation; small multiples; color; and narratives of space and time can increase the number of dimensions that can be represented on plane surfaces and the data density of displays.

The use of simulation and visualization has been greatly facilitated by advances in computing technologies. The orders-of-magnitude improvements in computational power makes both techniques increasingly accessible as tools in the modeling and analysis of complex phenomena. Visualization environments are greatly varied and span workstations, local area networks, and supercomputers for the division of tasks across different platforms for specific applications. Simulation,

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as the authors' point out, is quite likely to become more user-friendly and easier to program through visual programming tools, object-oriented programming languages, and better simulation languages.

The reservations I had about the paper are secondary to the contributions that it makes to theory building through its advocacy of simulation. There seems to be an implicit unqualified optimism in the use of the terms 'management' and 'technology'. If one subscribes to a type of technological system with tightly-coupled components and complex interactions predisposed to normal accidents, as posited by Perrow, then we might elect not to manage them; alternatively, we might dispose or even avoid them. Insofar as the authors present their work as a demonstration of how simulation can be used to assist in theory building, reference to actual cases of technology management would have strengthened model validity. Although computer simulations are claimed to be relatively inexpensive and expedient ways to explore the complex issues and alternative approaches implicated in the management of technology, a baseline reference would have strengthened their position. The scarcity of simulation studies in the organization area is a function of a number of factors, among which is the time and investment required by researchers to learn the tools of the trade and experiment with the model. Computer time may be cheap but human capital is more expensive.

Weick (1985: 133), in an examination of the resilience of organized anarchies and loosely coupled systems, advanced two caveats relevant to the consideration of visualization by those doing simulation (1) 'accuracy is less important than animation' and (2) 'be willing to leap before you look . . . If you look before you leap you may not see anything.' The first caveat emphasizes a visual approach to convey information which may not be numerically obvious. The second caveat advocates that researchers should experiment with visualization without any guarantee of the results, with the possibility that something interesting may occur that a prospective examination would not have foreseen. The same holds true for simulation. Researchers investigating organization phenomena other than technology should also take heed of the Mezias and Glynn paper for its demonstration of the value of simulation for theory building and theory testing.

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Authors' Response

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We are gratified by the wide-ranging and multi-disciplinary responses our article has evoked. Clearly, the applications–and implications–of simulation methodology are reverberating throughout scientific communities as diverse as management, organization theory, information technology, and atmospheric science, as represented by our four commentators. We believe that this positive reception reinforces the appropriateness and timeliness of bringing simulation methodologies to the study of technology.

We appreciate all the commentators' thorough and perceptive observations about simulation methodology in general, and about our article in particular. To us it seemed that all four agree in spirit with our proposals about the use of simulation methods; indeed there seems to be consensus that computer simulations represent an important tool for researchers concerned with the management of technology. Taken together, the commentaries provide useful ideas on extending the premises of our article and guiding future research efforts in this area. The commentaries appeared to focus on three major areas: (1) strengths and limitations of simulation methodology; (2) theoretical development and implications of simulation modeling; and (3) practical utility and interpretation of simulation results. We will use these three issues to frame our response.

Strengths and Limitations of Simulation Methodology

To borrow a term from Professor Christoph, 'using technology to study technology', raised several important methodological issues. While some concerns are endemic to any research methodology, others seem unique to simulations. Of special concern for simulations are the questions of model validation raised by commentators Paterson and Eriksson. We share their concerns about the correspondence between the abstractions of a simulated model and the events of real world phenomena. As both these commentators point out, only field research can provide some evidence to validate the model; we agree. However, while field work and case studies will inform and guide the usefulness of our models, there are inherent limitations on the extent to which the validation of simulations is possible. Professor Seibert is quite eloquent on this point. After indicating the major risks associated with any numerical modeling of complex processes, she suggests that the simulations' inherent advantage of rendering simplicity to complexity also constraints its ability to be fully validated. As she points out, we typically turn to simulation precisely in those situations that are too complex for common sense and simple reasoning. As a result, the most we can hope for in validation or verification of a complex model is some limited testing to determine the extent to which certain aspects of the model are plausible. Overall proof of the general validity of such complex models is not possible. Paradoxically, then, while more complex simulation methods may more closely approximate the complexity of technology management, validation of the simulated model may prove more elusive. This validation problem is exacerbated by the fact that under conditions of high technological or organizational complexity, exactly those conditions that would be hardest to validate, there may be increased need and value in using simulation methods.

Of course the issue of a model's isomorphism to reality is not unique to simulation-derived models. As Kerlinger (1973: 456) indicates, '[t]he subject of validity is complex, controversial, and peculiarly important in behavioral research.' Using Thorngate's (1976) concept of commensurate complexity, Weick (1979: 35) states that 'it is impossible for a theory of social behavior to be simultaneously general, accurate, and simple.' We characterize simulation methodology as having a particular mixture of methodological strengths and weaknesses. The bases of the method are impersonal and rule-bound assumptions that render it simple, relative to the complexity of real world problems in the management of technology. The precision of the calculations dictated by the algorithms renders it quire accurate. However, this simplicity and accuracy are bought at the price of modest general relevance, and even this modest level of external validity can only be obtained by extensive sensitivity analysis and the careful use, where possible, of empirically derived parameters. Taken together, the characteristics of this methodology suggest that a useful direction for future work would be to couple simulation models with other approaches. One potentially valuable linkage might be with field methods that tend to enhance generality. Accordingly, research on the management of technology might benefit from a portfolio approach which contains multiple methodological and design perspectives, each with their inherent strengths and limitations, but each with the potential to balance the other. We view simulation methodology as an important component of such a portfolio. As we suggest in the article, a computer simulation is not a substitute for empirical research, but a complement to it. Thus, rather than trying to prove the overall validity of a simulated model, a more manageable approach might be repeated testings of its consistency with demonstrated 'real world' cases, or as Professor Eriksson suggests, comparing the output of the model with reality.

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Theoretical Development and Implications of Simulation Modeling

An issue stated in our paper and mentioned by four of the commentators is one that we believe deserves additional emphasis: simulation modeling is a useful methodology for theory building and development, not theory testing. Theories generated by simulation methods might, in turn, be used to formulate hypotheses which might then be tested through empirical research. In spite of the appeal of simulation output, in terms of its seductive veneer of significant findings and compellingly graphic results, caution is warranted so that we do not mistake the model **as** the organization, as Professor Eriksson reminds us. The warning is particularly important for simulations becuase they represent systems in terms of symbols and logic (Whicker and Sigelman 1991). Thus, there is an acute need to heed the advice that 'the map is not the territory' (Weick 1979: 249). In evaluating simulation research, it is important not to confuse symbols with the thing symbolized, and by extension, not to confuse simulation with empiricism or reality.

Questions about the use of alternative theoretical paradigms were raised by commentators Paterson and Eriksson, who were concerned that our sample analysis may have been overly deterministic or too narrow in its typification of technology. First, we wish to clarify that our intent was only to demonstrate the utility of the simulation methodology rather than to address particular substantive questions on the management of technology. To conduct the simulation, we needed to make choices about theoretical assumptions for both technology and the organizational system we were modeling. This is not to say that alternative assumptions could not have been made and used to simulate the management of technology. In fact, several commentaries offer useful extensions of our simulation that might profitably be explored in future research on the management of technology. For instance, Professor Eriksson suggests that technology might be characterized in terms of its complexity, type (technical versus administrative) or origin (external or internal to the firm). Similarly, while our assumptions about organizations were based on a view of organizations as experiential learning systems, alternative theoretical assumptions are certainly possible and would constitute useful extensions. Other researchers have created simulation models based on the assumptions derived from different theories of organizations ranging from the loose coupling of 'garbage can' systems (Cohen, March, and Olsen 1972; March and Olsen 1976) to the tight coupling and rational intentions of organizational actors (Nelson and Winter 1982). While it might be argued that researchers in this tradition have tended to adopt more deterministic or structural views that overlook the impact of managerial action (Lant and Mezias 1992), some recent simulation studies have begun to include 'people' factors and other less mechanistic views (e. g., Carley 1992; March 1991).

While we are optimistic that future simulation research will continue to expand its choice of paradigms with which to model the study of technology, we also want to acknowledge that any approaches–be they deterministic or non-deterministic models–will necessarily be limiting. In her commentary, Professor Seibert cogently makes this point. She states that, in its endeavor to isolate and simplify complex organizational processes, a numeric model necessarily ignores other factors. This is, at once, both the strength and limitation of simulation modeling. Further, the nature of simulation is such that these ignored factors will tend to be those that are less amenable to symbolic representation and logical processes and thus more often than not, to more qualitative organizational and technological factors.

Practical Utility and Interpretation of Simulation Results

Several commentators were rightly concerned about the practicality and usefulness of the simulation model. Professor Christoph succinctly summarized the problem—'. . . managers may blindly use [simulation modeling], ask the wrong questions, and in a fashion similar to an erroneous spreadsheet, make faulty decisions'—as well as the solution—'. . . the designer of the [simulation] model must build in appropriate routines for the end user to make validation possible and easy to carry out.' Part of the problem, as well as the solution, lies within the technology itself; indeed, this point is echoed in Paterson's commentary in which he suggests the use of enhanced 'visualization' in order to help clarify and interpret the results of the simulation output. We support both these ideas and feel they represent important areas for extending the managerial frontiers of simulation methodologies.

In the skilled user's hands, simulation models can answer, in a time saving and cost effective manner, important conditional questions: What if this organization adopts one technology over another? What if we invest more resources in the search for new technologies? What if our competitor adopts a different technology? Professor Christoph is right to worry about the negative consequences of a bandwagon of research using simulation methodology in a fad-like way to study the management of technology. However, this problem is not unique to simulation methodology—any methodology has its limitations and will cease to yield insights when misused. The key to avoiding this outcome is to understand various methodological possibilities and their strengths and weaknesses. We hope that our paper, together with these comments and our response, can contribute to the greater awareness of the both the strengths and weaknesses of simulation methodology.

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Articles

Refocusing the Case Study: The Politics of Research and Researching Politics in IT Management

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Descriptors

case study
positivism
postmodern
critical
reflexive
power
politics
identity
information technology
management

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Abstract

Case study methods often generate highly readable accounts of complex organizational processes. They are, though, frequently criticized for their inability to produce results representative of a broad population of organizations. More recently, these methods have

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