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A DUAL METHODOLOGY FOR CASE STUDIES: SYNERGISTIC USE OF A LONGITUDINAL SINGLE SITE WITH REPLICATED MULTIPLE SITES*

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This paper describes a case study methodology that combines a real-time longitudinal (three-year) study with nine retrospective case studies about the same phenomenon. These two kinds of case studies offer opportunities for complementary and synergistic data gathering and analysis. That is, specific strengths in each method compensate for some particular weakness in the other. For instance, the retrospective studies offer the opportunity to identify patterns indicative of dynamic processes and the longitudinal study provides a close-up view of those patterns as they evolve over time. The combination of the two types of case studies also enhances three kinds of validity: construct, internal and external. The author also discusses problems with and shortcomings of this dual methodology and suggests the circumstances for which the methodology is especially appropriate.
(METHODOLOGY; LONGITUDINAL; CASE STUDIES; FIELD WORK; RESEARCH; TECHNOLOGY DEVELOPMENT)

1. Introduction

This paper reports on a case study methodology used to investigate the process of transferring new technologies from their developers into the hands of their users. The methodology, a combination of a longitudinal case study of a single site and a retrospective analysis of case studies, has significant advantages, including some that are synergistic, over either component methodology.

The research methodology is the focus here, rather than the research topic (technology transfer). Therefore the substantive issues motivating the study in which the methodology was used are presented only very briefly before the paper moves to a description of the field methods used and of both the expected and serendipitous synergies obtained through the particular blend of methods. The third section of the paper presents some principal weaknesses and failings in the methodology and concludes with suggestions about issues and research contexts best suited to this methodology. The methodology draws on or is related to the work of numerous other researchers who have discussed both the hazards and the richness of qualitative data-gathering methods (e.g., Miles and Huberman 1984, Van Maanen 1988), who have developed systematic, rigorous approaches to developing theory through comparative case study (Yin 1984, Eisenhardt 1989), or who have demonstrated the value of similar approaches by their own field-based case research (e.g., Bower 1970, Pettigrew 1979, Bourgeois and Eisenhardt 1988).

The particular phenomenon studied here is the internal development and deployment of equipment, processes and software tools intended to increase the productivity of employees as they produce goods or services for their corporate employers. The research topic of new technology implementation is conceptually linked to a large

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body of literature about organizational innovativeness. However, much of this literature has focused on those characteristics of the organization as a whole that covary with innovative behavior—for example, the degree of decentralization, complexity or formalization (Kimberly and Evanisko 1981, Moch and Morse 1977, Burns and Stalker 1961). In contrast, one objective of the research described here was to evolve theory about the nature of the innovation process itself, reflecting the complex interaction of individual and group motivation and behavior with technical problem-solving. Empirical work supporting the development of theory about the process of innovation is relatively scarce outside of the engineering literature. (A similar observation was made by Van de Ven 1986.) A secondary objective was to produce some managerial insights into those factors aiding and impeding the transfer of the new technology from its inventors/developers to its users within an organization. These two objectives—one theoretical and one oriented towards practice—necessitated a methodology capable of capturing dynamic processes. Moreover, the process description had to be at a level of detail that could provide understanding of individual, work group, and organizational behavior, over time.

2. Methodology

A. *Major Methodological Choices*

Three major methodological choices were made to accomplish these two objectives: 1) to use process-focused case studies; 2) to include enough such studies (nine) that the potential generalizability of the emerging theory would be beyond that offered by a single implementation situation, as suggested by Yin (1984); and 3) to complement the retrospective cases with the simultaneous conduct of one longitudinal (three-year), real-time field case study. The novelty and benefits of the methodology described below lie in the synergistic interaction of these last two decisions, and therefore the discussion focuses mostly on them.

1. *Case Study Methodology*. The phenomenon being researched always dictates to some extent the terms of its own dissection and exploration. Since this study focused on a "how" and "why" question about a contemporary set of events (Yin 1984, p. 13), and, as noted above, addressed a process not yet thoroughly researched, a case study was the logical methodology. A case study is a history of a past or current phenomenon, drawn from multiple sources of evidence. It can include data from direct observation and systematic interviewing as well as from public and private archives. In fact, any fact relevant to the stream of events describing the phenomenon is a potential datum in a case study, since context is important (Franz and Robey 1984, Stone 1978).

Rather than seeking degrees of freedom from a large standardized data set, the case study tests "theory with degrees of freedom coming from the multiple implications of [the] theory. The process is a kind of pattern-matching" (Campbell 1975, p. 182). In the research on technology development and implementation discussed here, no single perspective, however numerous the observations from that one vantage point, would reveal the entire pattern. In order to understand all the interacting factors (for example, the criticality of the technology to the business, alteration of the technology during implementation or the actions of champions on behalf of the new technology), it was necessary that the research methodology slice vertically through the organization, obtaining data from multiple levels and perspectives. Therefore a case study approach was appropriate and was used. However, the methodology of the study was not limited to a single case.

2. *Multiple Cases.* Ä single case study is subject to limits in generalizability and several potential biases, such as misjudging the representativeness of a single event (Tversky and Kahneman 1986), exaggerating the salience of a datum because of its ready availability, or biasing estimates because of unconscious anchoring (see Jaikumar and Bohn 1986). Multiple cases augment external validity and help guard against observer biases. Yin argues that the logic underlying a multiple-case study approach is similar to that guiding multiple experiments and that each case should be selected so that it "either (a) predicts similar results (a literal replication), or (b) produces contrary results but for predictable reasons (a theoretical replication)" (Yin 1984, pp. 48–49). Since the objective of the research was to produce theory relevant to numerous different managerial situations and capable of explaining success or failure, a methodology including both literal and theoretical replication was required. As described below in some detail, nine retrospective case studies of internal technology transfer were conducted to fulfill this requirement.

3. *Longitudinal Study.* The most significant limitation of wholly retrospective research is the difficulty of determining cause and effect from reconstructed events. Moreover, although studies have shown that the participants in organizational processes do not forget key events in these processes as readily as one might suppose (Huber 1985), the participant-informant in a wholly retrospective study may not have recognized an event as important when it occurred and thus may not recall it afterwards. Therefore, whereas multiple retrospective studies increase the external validity of a research design, a longitudinal, real-time study can increase internal validity by enabling one to track cause and effect. For this reason, simultaneously with the nine retrospective studies, I conducted one in-depth, three-year study of the development and deployment of a technology. See Table 1 for a timeline and description of the data sources used in the 10 cases.

B. *Study Context and Structure*

1. *Retrospective Cases: Context and Structure.* As Table 1 shows, the first retrospective case study (Structured System Analysis, or SSA) included extensive data-gathering efforts in 1983–1984. First, a highly structured interview guide was developed in a pilot study of 25 computer programmers and then that guide was applied in 145 personal interviews with the computer programmers who constituted the population targeted as users of the SSA software and techniques. The data from these interviews at three different organizational sites provided a comprehensive view of users' opinions about SSA (e.g., strengths and weaknesses of the technology, influences on users' adoption decisions) at one point in time. However, the 145 interviews added only bulk, not depth, to the research database. That is, the information gathered in the full scale study was largely redundant with the insights I had already obtained in the 25 pilot interviews. True, I gathered enough additional degrees of freedom from the large-scale survey of 145 interviews to test those initial insights with statistical modeling techniques (Leonard-Barton 1987a). However, all the data (both pilot and full-scale survey) emanated from one perspective: the users'. I had to return to the organization for mostly unstructured interviews with the SSA developers and with other managers in order to understand the whole story from multiple perspectives. Therefore, in the next eight retrospective studies, conducted 1984–1986, I interviewed fewer respondents but drew them from populations representing multiple perspectives. These unstructured interviews were supplemented with a short (two-page) questionnaire filled out by two technology developers and at least two representative users at each of the eight study sites.

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TABLE 3
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Research Activities	Methodology	
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a. Efficiency	Low; danger of data overload; much unusable data	Relatively high; focused data-gathering
b. Objectivity	Danger of too deep involvement, developing unconscious biases	Danger of unconsciously accepting respondent bias
c. Pattern recognition	Microscopic examination of details of process	Recognition of overall patterns in process
2. Establishing Validity		
a. External Validity	Low generalizability	Relatively high generalizability; variety of situations
b. Internal Validity	Relatively high; good opportunity to establish cause and effect	Lower; potential confusion about cause and effect
c. Construct Validity	Opportunity to test sensitivity of construct measures to passage of time	Opportunity to validate stability of construct across situations

criteria enabled me to limit numerous potential influences originating *outside* the firm (such as the effects of external vendors, of external market competition and demand, or of historically different industry conditions), as possible sources of variance in project outcomes.

In addition, all nine cases were matched on three dimensions that controlled for irrelevant sources of variance originating *within* the firm. First, the technologies selected had all passed some baseline tests of technical feasibility. This provision eliminated from study any cases in which the failure to transfer to users occurred simply because the technology was technically infeasible. Second, all the technologies selected altered the work environment in some obvious (albeit not necessarily important) way. This selection criterion eliminated cases in which transfer success could potentially be imputed to users' unawareness of any innovation, i.e., that anything had been changed. For instance, a software compiler project was rejected because the technology transformed the internal workings of a computer, but without any noticeable alteration to the interface visible to users. Third, the transfer stages included in the study were consistently defined across projects. All projects focused on the time from the first establishment of technical feasibility until the user organization used the technology in full production mode, or in two cases of failure, until the project was cancelled. This definition set bounds on the transfer situation, confining it to a series of transactions between developers and the earliest users, thus providing additional control over undesired variation among cases.

While the majority of the cases were literal replications of the first SSA study circumstances, several were selected for the purposes of "theoretical replication," that is, so as to explore conditions under which technology transfer failed. In selecting the total ten cases, the original intent was to match five complete failures with five highly successful cases, and informants from the corporations were asked to make these judgments during the initial selection process. However, making such absolute distinctions between success and failure proved to be highly problematic for most of the cases. Of the ten, two were complete failures, two were initial failures (eventually revived), three were moderately successful and three were highly successful. (Mea-

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2. Establishing Validity		
a. External Validity	Low generalizability	Relatively high generalizability; variety of situations
b. Internal Validity	Relatively high; good opportunity to establish cause and effect	Lower; potential confusion about cause and effect
c. Construct Validity	Opportunity to test sensitivity of construct measures to passage of time	Opportunity to validate stability of construct across situations

criteria enabled me to limit numerous potential influences originating *outside* the firm (such as the effects of external vendors, of external market competition and demand, or of historically different industry conditions), as possible sources of variance in project outcomes.

In addition, all nine cases were matched on three dimensions that controlled for irrelevant sources of variance originating *within* the firm. First, the technologies selected had all passed some baseline tests of technical feasibility. This provision eliminated from study any cases in which the failure to transfer to users occurred simply because the technology was technically infeasible. Second, all the technologies selected altered the work environment in some obvious (albeit not necessarily important) way. This selection criterion eliminated cases in which transfer success could potentially be imputed to users' unawareness of any innovation, i.e., that anything had been changed. For instance, a software compiler project was rejected because the technology transformed the internal workings of a computer, but without any noticeable alteration to the interface visible to users. Third, the transfer stages included in the study were consistently defined across projects. All projects focused on the time from the first establishment of technical feasibility until the user organization used the technology in full production mode, or in two cases of failure, until the project was cancelled. This definition set bounds on the transfer situation, confining it to a series of transactions between developers and the earliest users, thus providing additional control over undesired variation among cases.

While the majority of the cases were literal replications of the first SSA study circumstances, several were selected for the purposes of "theoretical replication," that is, so as to explore conditions under which technology transfer failed. In selecting the total ten cases, the original intent was to match five complete failures with five highly successful cases, and informants from the corporations were asked to make these judgments during the initial selection process. However, making such absolute distinctions between success and failure proved to be highly problematic for most of the cases. Of the ten, two were complete failures, two were initial failures (eventually revived), three were moderately successful and three were highly successful. (Mea-

sures of success included informants' opinions during the unstructured interviews, their responses to scaled questions on the questionnaire, and archival data.)

As I had discovered in my first case study (SSA, conducted in 1983–1984), retrospective studies often leave one hungry for the details that enrich understanding of process. Informants were occasionally quite vague about the timing of events and therefore when archival data were not available, it was difficult to be certain of the direction of influence, i.e., which was cause and which effect. Moreover, sometimes informants were reluctant to talk about "political" issues, believing it unwise to dredge up old problems. Yet understanding the resolution of those issues was often critical to the theory-building process. Recognizing these disadvantages to reconstructing events after the fact led me to conduct the longitudinal, real-time, in-depth study simultaneously with the final eight retrospective studies.

2. *Longitudinal Study: Context and Structure.* The purest form of a longitudinal field study, namely daily participant observation, was not feasible. I could visit the research site only once or twice a week. Therefore most of the data was obtained through retrospective reports gathered shortly after events occurred. However, a longitudinal study involving a series of multiple interviews about recent events offered the obvious benefit of proximity in time to current events, and thereby increased the likelihood that I could determine the sequence and nature of events accurately. Moreover, there were many opportunities to collect data through personal observation at meetings and training sessions.

The context of the longitudinal research was the development and deployment of XSEL, an "expert system" (a type of artificial intelligence software), intended to help sales representatives at Digital Equipment Corporation configure (select components for and design) the very large semicustomized computer systems sold by Digital. Accuracy in this complex task is very important, for small omissions (e.g., omitting a connector cable) or incorrect assumptions about the compatibility of parts (e.g., communication hardware used to link computers) can render a computer system impossible to manufacture in the form ordered by the sales representative. (See Leonard-Barton 1987b for a detailed account.) Therefore XSEL could potentially save Digital much time, effort and money by helping the sales representative configure the system correctly at the time he/she placed the customer's order.

During the three-year period in which I followed the XSEL development and implementation (1984–1986), I gathered data with both highly structured and unstructured methods. The formal, highly structured efforts included: 1) three annual surveys (two telephone, one mail) of a representative national sample of about 100 sales representatives, with almost all of the questions replicated from year to year; 2) questionnaires filled out by members of the "User Design Group" (sales representatives who met every few months with software developers to review and guide the progress of the software); 3) a telephone survey of 46 Sales Unit Managers (the immediate supervisors of the representatives who were to use XSEL); 4) archival data; and 5) structured interview sessions with the developers to analyze the number and kinds of technical changes or additions being made to the software. (See Table 1.)

Unstructured data collection included notes taken during User Design Group meetings and meetings of the XSEL program office with various other groups important to implementation. For example, I traveled to a national meeting of the "Systems Managers," i.e., seven computer operators, each of whom was in charge of the machine dedicated in each sales region to the running of XSEL. Data were also obtained in unstructured interviews with very early XSEL users across the country and with people around the company whose work would be affected by the advent of

XSEL. These informants included people in the information systems office traditionally responsible for supporting software, and three of the seven regional sales managers whose opinions influenced use or rejection of XSEL by the sales representatives reporting to them. In addition, I held many informal meetings with the XSEL program office personnel and with their (sole) counterpart in the sales organization. Altogether these sources produced a plethora of data, which were paradoxically both the strength and the weakness of the in-depth longitudinal study. As discussed later in greater detail, the method promoted a rich, full understanding of the context and the process from the perspective of someone who has lived through events. However, it was clear even at the beginning of the study that such abundant data could also smother me, obscuring the very process patterns I wished to discern.

C. *The Synergy of Combined Methods*

Two sets of advantages are obtained from combining the longitudinal real-time study with the replicated historical cases: 1) specific strengths in the data-gathering process for each method that compensated for some particular lack or weakness in the other, and 2) complementary approaches in each method that, because of the synergy obtained by combining the two, enhanced three kinds of validity: external, construct and internal.

Because "all research methods are seriously flawed—though each is flawed differently" (McGrath, Martin and Kulka 1982, p. 15), the challenge to the researcher is to "choose—and often to devise—a set of measures... that together, transcend one another's methodological vulnerabilities" (McGrath 1982, p. 99). In data-gathering, the real-time study and the replicated retrospective cases methodologies have compensatory strengths in 1) efficiency and 2) objectivity.

1. *Data-Gathering*

a. *Efficiency*. The more that the in-depth, real-time longitudinal study approximates a true ethnographic, participant-observation methodology, the more the researcher sacrifices efficiency for richness of data. In such studies, delegating data-gathering leads to unacceptable losses in the investigator's grasp of important details. Further, in order to observe critical events, one must often spend an inordinate amount of time on noncritical ones and in building relationships with the people involved. (See also Van Maanen 1988 on this point.) Therefore the ethnographic study is deep rather than quick.

The three-year study described here did not achieve the depth of ethnographic immersion described by Van Maanen (1988) or undertaken by Barley (1986). However, the study did involve spending whole days and evenings at the site, because the phenomenon of interest, namely the development and implementation of new technology, could not be totally tracked through any particular set of meetings or known events. For instance, although the XSEL program office held meetings with a User Design Group of representatives from the sales force, these irregularly held meetings could not provide insight into all the dynamics of interest. Not only were many other types of meetings and sessions important, but many critical events occurred outside of formal situations.

Much useful data emerged from informal conversations at lunch, in hallways, and during breaks in formal meetings. For example, such conversations with a senior sales representative and (on a separate occasion) with a system manager, both of whom had extensive computer science training, revealed the depth of the shift in development methodology and architecture that the artificial intelligence approach to computer software represented for people trained traditionally. To a programmer accus-

tomed to the precise, sequentially ordered logic of COBOL or FORTRAN, the rule-based system created in XSEL appeared undisciplined, chaotic—in short, like poorly written software code rather than differently constructed code. Understanding this viewpoint led me to include questions in the three annual national surveys identifying a subpopulation of sales representatives hostile to XSEL because of what I termed their "signature skill," i.e., their close emotional and cognitive ties to traditional programming approaches. The two or three individuals who had initially alerted me to that reaction to expert systems became over the period of the study advocates of XSEL—thereby demonstrating that their original aversion was not necessarily permanent. Their attitudinal shift, which I followed informally, was mirrored over the three years in the annual national surveys. (That is, the subpopulation of sales representatives identified in the 1984 national sample as having programming as a signature skill changed from hostile to at least neutral attitudes towards XSEL by the 1986 survey.) Although following this kind of research thread in the longitudinal research was very important to the overall study, it was extremely time-consuming to attend so many meetings, go out to lunch together with the program office managers many days, or arrange to be available on the late Friday afternoons when the managers were able to take the time to sit back and reflect on the week's problems and progress.

In contrast, the replicated, multiple-site cases were relatively efficient. Most of the data for the retrospective studies were gathered in two almost simultaneous phases. First, I held unstructured interviews with developers of the technology and with the internal corporate users of the innovation, and tapped archival sources. This process generally took two very concentrated days on site plus some follow-up telephone calls. Interviews at the first several retrospective studies were much less efficiently conducted than those at the last, for (as noted below), findings and observations from the longitudinal study often suggested issues that had been inadequately covered in the initial interviews in the retrospective studies. Consequently, some of the follow-up telephone interviews were quite substantial. All of these interviews were tape-recorded, unless the informants objected to that practice. They were promised anonymity, in that nothing they said was attributed to them personally (verbally or in writing) to anyone else within or outside their organization, until and unless they gave me permission to quote them. No one had access to the tapes besides one research assistant and me.

In the second phase of the research, I mailed out the two-page questionnaire, responses to which provided standard outcome measures for all ten cases. When I received these back, I compared the opinions of the technology developers with those of the technology users, and telephoned for further discussion and clarification those few informants whose evaluations of outcome in a particular project were widely discrepant from each other's. Next I mailed back to one previously designated representative at each field site the draft narrative of the project events as we had reconstructed them, for review. This historical document usually underwent several iterations with informants providing additional details for the document, correcting inaccuracies in it and sometimes (unfortunately) altering their earlier statements even though all the quotations had already been individually released for inclusion in the document. This process of obtaining agreement that the story had been accurately (and completely) presented was the most time-consuming part of the studies.

b. *Objectivity*. In a real-time longitudinal study, the researcher is in danger of losing objectivity—of becoming too involved with the organization, the people and the process. Students of innovation are notoriously prone to a "pro-innovation bias" (Rogers 1983), and that likelihood is increased by proximity to the managerial

struggle to launch the innovation. One may be perceived as, and may in fact become, an advocate rather than an observer. In retrospective studies, it is easier to maintain both in appearance and in fact an appropriately open mind about the desirability of the innovation.

The retrospective studies could not counter any specific pro-XSEL biases I had developed over the years of association with the expert system. However, the retrospective cases at least alerted me to the possibility of a general pro-innovative bias, because in several of the replicated studies, opposition to the innovation was very well founded and articulately presented. I could not possibly dismiss it as merely a simplistic "not-invented-here" reaction. In assessing the anti-innovation stances in the multiple cases, I was continually prompted to re-examine my own attitudes towards XSEL. In short, the retrospective cases helped keep me "honest."

On the other hand, in the retrospective studies, the danger is not so much that one may surrender to ones *own* biases as that one may unconsciously accept those of the informant. The innovation is seen through the lenses of the informants chosen, and the researcher may take the story as told, without questioning interpretations. In such research, the interviewer does not have as much access to corroborating or refuting details as one does in an in-depth longitudinal study. Thus the researcher using retrospective reports to investigate historical events has to work harder to be a critical audience, aware of everyone's vulnerability to subjective perceptions (Sears and Freedman 1974).

The XSEL study provided some striking examples of political expediency which came to my attention only because of my deep involvement with the project, and that alerted me to the potential for similar events in the retrospective studies. For instance, I attended a dinner meeting one evening at which a manager spoke to the XSEL User Design Group. I took copious notes, writing down verbatim some quotations about actions this individual said he had taken in support of the innovation. These public statements turned out to differ greatly both from the reports of others and from written memos he had previously issued.

Experience with such self-serving revisionism led me to be cautious in the retrospective studies about accepting self-report as the only source of evidence about any incident or personal stance vis-à-vis the new technology. This caution was especially necessary in investigating mutual adaptation (i.e., the degree to which the technology was adapted to the organization during the development and implementation process and the degree to which the organization was adapted to fit the requirements of the technology; see Leonard-Barton 1988a). Participants differed in their perceptions of both the extent of adaptation and in their attribution of responsibility for those changes. Forewarned by my experiences with XSEL, I focused as much as possible on certifiable technical and organizational changes, e.g., the midproject capital investment in a new dryer for a polymer process, indicating an initial oversight in equipment needs or additions to a job performance checklist used to evaluate a buyer in the Purchasing Department. Such lists of documented changes helped guard against revisionist history.

c. *Pattern Recognition*. The replicated studies and the in-depth tracking of XSEL focused on the same issues. However, the difference in perspective was akin to that on a computer when one first views the pattern from some distance and then zooms in, enlarging some segment or line until the individual pixels are visible. This variance in depth of vision allowed me to identify some pattern or process of interest from the macro perspective of the retrospective studies and then, in effect, examine it in the XSEL study with a microscope either to dissect it further into component parts or to understand the forces that drove it.

For instance, somewhat related experimental findings in marketing (Anderson 1973, Oliver 1977) combined with empirical observations from the replicated cases suggested that inflated expectations about an innovation led, soon after initial trials, to discouragement and dislike of the new technology when the person trying it for the first time discovers how far short of his/her expectations it falls. The XSEL study allowed the opportunity to examine that drop at some depth, to identify those subgroups in the sales force most vulnerable to such initial disillusionment, and to understand through structured interviews why the drop occurred and how and why an innovation could recover. As noted earlier, members of one subgroup in the sales representative population, skilled software programmers, were initially critical and then changed their minds about XSEL; another group, skilled configurers, became increasingly critical over time. This detailed understanding of the user reactions enhanced the insights that I had obtained from the research in the replicated cases. However, keeping the macro perspective in mind was also valuable for the XSEL study. Recognition of the common patterns in innovation implementation led me to anticipate a bottoming-out of the negative reactions when the XSEL project was going through that initial drop in acceptance levels, because evidence from the other cases suggested that the downward trend could be reversed among at least some users, and indeed that is what occurred. User evaluations surveyed in 1986 were higher than they had been in 1985.

Similarly, although informants in the replicated cases often identified innovation "assassins," it was not clear from those retrospective accounts exactly how such an individual had destroyed or delayed an innovation by strategic inaction as well as by deliberate anti-innovation action. A close-up view of an innovation assassin at work in the XSEL case provided real insight into some of the ways an individual can block progress in the innovation simply by delaying action, while still professing support for it. Understanding those dynamics in turn inspired me to question informants in the replicated cases about critical individuals' *lack* of action as well as about their overt actions.

2. *Establishing Validity*

a. *External Validity*. Multiple case studies on a given topic clearly have more external validity, i.e., generalizability, than does a single case. For instance, the mutual adaptation of technology and organization that occurred within the XSEL project could have been driven by the nontraditional software development processes employed in constructing expert systems or by the unusual management decision to employ a User Design Group very early in the development project. If so, then many of my observations about XSEL would have little applicability to other technology development and implementation situations. By expanding the sample of cases to include hardware, chemical processes, and other software projects conducted in several different companies, I was able to offset somewhat the XSEL study's lack of external validity.

b. *Construct Validity*. Because a construct is an abstraction, its definition consists in part of "sets of propositions about (its) relationship to other variables—other constructs or directly observable behavior" (Selltiz, Wrightsman and Cook 1976, p. 173). Therefore one validates a construct by observing whether predictions made on the basis of those propositions about its relationship to other variables are confirmed when tested. Multiple sources of evidence, if they yield similar results, are evidence of a construct's convergent validity. If the construct as measured can be differentiated from other constructs, it also possesses discriminant validity (Campbell and Fiske 1959). The dual research methodology described here provides better

opportunity for construct validation than either design would alone, as the following example illustrates.

One construct that was of some importance to the theory under development in this study was the "communicability" of a technology, defined as "the degree to which a technology's operating principles (know-how) and underlying scientific principles (know-why) can be communicated to people other than its developers" (Leonard-Barton 1988b, p. 7). Multiple measures of this construct were possible, ranging from what are often considered "objective" measures (e.g., level of user documentation and availability of training) to more "subjective" measures (e.g., the ease of use, relative to the perceived skill level of the users). Informant responses to numerically scaled questions about communicability in the XSEL annual surveys and to less structured questions in the retrospective studies were consistent in their relationship to such other variables as evaluation of the technology by users and the relationships proved to be in the expected direction. Moreover, the construct was defined to be conceptually quite distinct from the related concept of preparedness: "the extent to which technology has shown proof of technical feasibility in a laboratory or in an operational setting" (Leonard-Barton 1988b, p. 7). However, as again indicated by responses in both the annual XSEL surveys and the retrospective case studies, both communicability and preparedness contributed to the users' perceptions of how ready the technology was for transfer. The two studies thus provided corroborating evidence of both convergent and divergent validity for the construct.

However, the special value of doing both kinds of study was the opportunity to recognize how sensitive were measures of the construct to the point in time at which they were administered. Communicability varied across time and relative to different groups of users. In fact, the construct had validity only if its measurement was associated with a given, consistently specified point in the transfer process—and for a particular group of technology receivers. The XSEL study illuminated these points, because it was possible to measure communicability at a number of different points during the implementation process. This opportunity for measurement at multiple stages in turn led to the need to select one defined point that could be identified in all cases and thus compared consistently across them. The point selected (the very first use of the technology in a routine production task) could be identified with a satisfactory degree of precision and marked the implementation stage of greatest theoretical interest. Thus, the longitudinal study aided in precise definition, and the retrospective studies demonstrated the consistency of the predicted patterns of relationship between the construct and other variables.

c. *Internal Validity: Cause and Effect.* One of the greatest advantages of the dual methodologies derived from the ability to move back and forth between the two, formulating theory in one setting and then immediately placing the embryonic ideas in the context of the other kind of study for potential disconfirmation. This cutting and pasting of ideas was particularly useful in establishing internal validity, for the combination of the two types of case studies provided better evidence for hypotheses about causal relationships between variables than either could have alone.

For example, in the first several retrospective studies, a direct casual link was made verbally by respondents between the innovation's level of transferability (preparedness and communicability) and the transfer effort's problems or outright failure. That is, informants in the organizations that were to use the new technologies told me that the technologies failed because they were released by the developers too soon, before they were proved technically (although one of the case selection criteria had been some proof of technical feasibility in the laboratory) and before adequate documentation and training were available. This cause and effect relationship seemed reason-

able and in keeping with marketing and diffusion literature.

However, in the XSEL case study, I saw that although the technology measured quite low on transferability, changes were being made on both the software system and in the sales organization where it was to be used, and these changes were moving the project out of the doldrums and, in some sites, towards success. Was this an anomaly? Or was there in fact an intervening variable between the independent variable of transferability and the dependent variable of success or failure? Judging from the XSEL case, initial lack of transferability was not a direct determinant of success or failure. Rather, the degree of adaptability in the technology and/or in the receiving organization intervened, and the relationship was best described by a mediated model (see Venkatraman 1989 and Leonard-Barton 1988a, b). In short, the observed effect of success or failure was directly caused by the degree of mutual technical and organizational adaptation undertaken during the project—in reaction to the initial conditions of the technology.

Once I had made this hypothesis, I then returned to the multiple sites in which I was conducting interviews and probed more deeply into the superficial cause and effect relationships that had been suggested to me. I found that in the more troubled projects, the developers had refused to adapt their innovation to the user environment and/or the users had refused to adapt their organization in any way. In contrast, the success stories included technologies that were initially of very low transferability but mutual adaptation of technology and organization had occurred.

This causal relationship is unlikely to have been so apparent to me from retrospective studies alone, because in the minds of some of the respondents, the direction of causality was confused. After a project became very difficult, respondents tended to reflect that the technology had never been "ready to transfer." Similarly, after a project was acknowledged to be a success, respondents apparently underestimated or even totally forgot the accommodations that had been made in order for the transfer to succeed. Yet when I ferreted out the changes in technology and/or organization that had been made (or in the less successful cases, evidence of inability or unwillingness to change), and fed the story of sequential events back to the respondents, they confirmed the causal relationship—sometimes acknowledging that their initial explanations to me had been "oversimplified." Of considerable interest was the tendency to take the changes for granted when they were made and to have never considered such adaptations when they were not. Without the longitudinal study, I might never have seen the intervening variable. Without the retrospective studies, I could not have confirmed the pattern.

3. Problems with the Methodology

No methodology is perfect. Some of the shortcomings of the one promulgated here are inherent in the methodology and others are attributable to inadequate operationalization. I turn now to discussing a few of the most important problems encountered.

A. *Problems Inherent in the Design*

The inherent limitations of qualitative case studies, such as the vulnerability of the data to subjective interpretation and the difficulties of compiling in one's head evidence about relationships among variables are well known and need not be elaborated here. Miles and Huberman (1984) provide a useful guide to overcoming some of these problems. The time-consuming nature of ethnographic studies and hence the labor intensity of the longitudinal portion of the dual design is self-evident. Finally, because a number of the potential biases inherent in both the longitudinal

and the retrospective cases have been explored above, I need not explore them further here. Rather, I will discuss two problems more particularly associated with the dual methodology, i.e., coordinating the different data collection approaches and dealing with the volume of data generated by the combined approaches.

1. *Coordinating Data Collection.* Among limitations inherent in the dual methodology described here, perhaps the most important is the difficulty of managing the data collection. Because many of the opportunities for synergy between the retrospective and the in-depth studies arise from the recognition of patterns or different perspectives on the same phenomenon, the research has to be very tightly coordinated. Case facts are open to interpretation. Therefore it is best to have at least two researchers, who can challenge each other's observations. Moreover, conducting an adequate number of multiple-cases at geographically dispersed sites to complement the simultaneous in-depth study is physically demanding, if not impossible, for one person.

However, confining all the mental analyses to one mind would be a more efficient way to obtain the synergy between studies. During the three years of these studies, I had the continuous assistance of one researcher, albeit that position was held successively by two different people. I found it difficult to transfer completely to that research assistant the emerging intuitions I was gaining from my greater involvement in the XSEL study. Such intuitions were important guides in identifying issues to be probed in the much more superficial and brief interviewing situations in the multiple cases. At each of the multiple case sites, although I conducted at least the first day of interviewing, the assistant was charged with responsibility for some follow-up and some of the narrative write-up and review. This data-gathering process sometimes left to the assistant the delicate task of probing for important politically sensitive events and critical personal relationships. Because of their relatively superficial exposure to the in-depth XSEL case, the assistants were less well prepared to make the mental contrasts and comparisons with the longitudinal data that generated important insights and questions to ask.

2. *Volume of Data.* A second, very important shortcoming of this design is the overwhelming volume of data generated—a hazard of all qualitative research, as Miles and Huberman (1984) point out. Although, as noted below, more disciplined data-gathering would have helped, it is difficult to identify critical data in a real-time longitudinal study, while one is in the midst of the research. Consequently, my files are full of notes, presentations made part-way through the research and questionnaire responses to several small-scale surveys that for various reasons have not yet been fully analyzed, much less rendered in publishable form. A certain amount of this thrashing about is probably inherent in any real-time, in-depth study and in fact in any case study. However, the interplay between the retrospective and the real-time studies fostered an exploratory attitude because of the pattern-matching opportunities mentioned above. Many of the small-scale surveys undertaken represented short-lived forays to investigate some particular issue that surfaced in the retrospective studies but could only be investigated in depth in real time (e.g., the impact of intentionally varied training approaches on users' expectations about the technology).

B. *Shortcomings Attributable to Operationalization*

The problems noted above may be inescapable attributes of the dual methodology described in this paper and may be only ameliorated at best by foreknowledge. The difficulties described below are traceable to minor perhaps suboptimal choices made throughout the research about where and how to focus efforts. A different researcher might not have encountered them.

1. *Structuring the Data Collection.* As noted above, the XSEL study generated a vast amount of data—but not necessarily a comparable amount of information. I made many attempts to impose some discipline and structure on the data-gathering process: questionnaires administered to trainees before and after using XSEL for the first time; to members of the User Design Group; and to the systems managers controlling the computers on which XSEL ran. The compilation of these small data sets, which was often inspired by frustration with the relatively superficial perspective provided by the retrospective studies, did generate deeper understanding of the innovation implementation process. However, with hindsight I do not believe the insights they provided were worth the time and effort expended on their careful design and execution. The numbers of people interviewed were really too small (10–30) for hypothesis-testing. These data sets were subsequently overshadowed by the two much larger surveys (the three annual nationwide surveys of the sales representatives and the 1986 telephone survey of Sales Unit Managers)—data-collection efforts that produced publishable quantitative data. Therefore much of the effort spent in carefully structuring these small sample surveys had low payoff. Unstructured interviews could probably have yielded information of comparable worth.

In contrast, the process of moving back and forth between the longitudinal and the retrospective cases in building theory, as described earlier, might have been more valuable had the approach been *more* structured. For instance, in subsequent comparative cases (conducted 1987–1989, after the research reported here), I was much more careful about interviewing people in exactly comparable organizational positions or roles than I was in the research described herein.

2. *Deciding on the Unit of Observation.* A large problem in the XSEL study was my indecision over what should be the unit of observation. Were the comments of individual users in the User Design Group meetings relevant, or should I instead track only the major outcomes of those meetings as critical events or decisions? (See Levinson 1985, for examples of this latter approach.) The indecision was in large part responsible for the accumulation of data at a level of detail that later proved to be unnecessary.

In the retrospective cases, time had sifted much data for me, in that informants focused on events and decisions they regarded as significant. It was important in the in-depth study, therefore, to select a level of observation that had some meaningful parallels in the retrospective studies. Some detailed individual observations could be aggregated to identify trends, a decision, or an event, but individual comments by people with only ephemeral roles in the innovation process were often useless, and, unless they had a significant effect (which a few did), could not be paralleled in the retrospective studies. In most cases, the event or decision-point about which the comment was made was the appropriate unit of observation. It might have been useful, therefore, to conduct a few more of the retrospective studies as references, before beginning the longitudinal XSEL study. The cost of such a strategy would have been to lengthen the total period of research.

3. *Rationale for Choice of Cases.* Another operational shortcoming was that my selection of complementary retrospective cases was driven almost entirely by a desire for more generalizability, within the constraints described earlier, so as to control for certain variables. Yin (1984) advocates selecting each additional case in a research program to address some very specific aspect of theory inadequately addressed in the previous cases. This approach suggests a much more painstaking sequential selection process than the one I followed, and would be especially appropriate if the researcher's theory is more fully developed at the outset than mine was.

4. Conclusion

Under what circumstances might the research methodology described above be useful? There are at least three factors to consider: the research topic; researchers' skills and preferences; and the availability of research sites. The methodology described here is suited for exploratory and hypothesis-generation rather than hypothesis-testing. Whereas that statement is generally true of case studies, it is particularly relevant for this dual methodology, because one of its principal strengths is the synergy obtained from observing phenomena through both the wide-angle lens of the multiple-site study and the close-up lens of the longitudinal. There are, of course, opportunities for hypothesis-testing embedded within the studies, but the overall design is most compatible to theory-building. It also seems particularly suited to studying *process* because of the opportunities for exploring dynamics both as historical patterns in the retrospective studies and as evolving patterns in the real-time study.

Because researchers have differing skills, it behooves us to consider the match between researchers and methodology. In recent years the research case study has gained both rigor and respectability. Noted experimental researcher Donald Campbell may have precipitated the trend by recanting his previous "dogmatic disparagement" (Campbell 1975, p. 191). However, recognition that case studies can be rigorous has not necessarily led to a concomitant realization of the skills needed to successfully interview and then analyze case data. The necessary interviewing skills might be compared with those of an investigative reporter. One needs to keep previous interviewee responses in mind while simultaneously probing with the current informant; one needs to be very aware of the significance of what is left unsaid as well as what is said, and so on. Analysis of the data requires a high tolerance for initial ambiguity, as one iterates toward clarity. Finally, these methods require sustained effort over long periods of time; therefore one must really enjoy fieldwork.

Certain field conditions must prevail for success. The longitudinal study requires the up-front commitment of an organization for extended cooperation. Yet the study sponsor can leave the organization, initial findings may threaten powerful individuals, or the company's fortunes may suffer such a blow that outsiders are no longer welcome. One longitudinal site alone therefore would be rather risky for a doctoral thesis; the combined methodology advocated here lessens that risk somewhat, but one would still be well advised to plan for research output as the study proceeds—not just at the end.

Moreover, this methodology can require that the researcher spend almost as much time and effort on setting organizational expectations and on fostering and maintaining his/her relationship with the organization as on the actual data-gathering. For instance, organizations accustomed to the quick turnaround of information available from surveys should be forewarned that the kind of research design outlined above does not yield immediate results. Organizations nevertheless expect some interim reports and feedback. In a very production-oriented business environment, a researcher who is a useful source of information in some way is more likely to be visible and to be kept informed when events occur that affect the study. Therefore, some researchers regularly send papers and other information that could be useful to the managers who are cooperating in the study. (Of course, any information sent to current or prospective informants can constitute an intervention; therefore one needs to be alert to the potential for biasing future responses.) As these examples may suggest, the labor intensity of this methodology can hardly be overstated.

Although the research methodology described herein evolved in the field, it was mostly designed before implementation. Its principal and innovative feature, the simultaneous use of retrospective case studies and a real time longitudinal study, was

a conscious choice made almost at the very outset of the research. As organizational studies continue to move from art towards science, we need many such methodological variants so that we can prevent our tools from dictating and limiting the nature of our insights. Campbell, Daft and Hulin note, "Theories that are at least in part based on experience, that take account of the ecology of organizational behavior, and that are intended to function as heuristics seem to be in the shortest supply" (1982, p. 146). The development of such practice-oriented theories necessitates both the refinement of existing field research methodologies and development of new ones.

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Appendix A

Examples of Measures of Dependent Variable

Partial list of questions mailed to all project developers and users. (The responses were discussed over the telephone.)

IN THE FOLLOWING QUESTIONS, I WILL REFER TO:
PROJECT_____DEVELOPERS_____
USER ORGANIZATION_____

1. To what extent do you feel this project met the objectives set for it? (CIRCLE ONE NUMBER FOR EACH CATEGORY OF OBJECTIVE)

	Totally Failed to Meet	Failed Somewhat	Met	Surpassed
Technical Objectives	1	2	3	4
Business Objectives	1	2	3	4
Budget	1	2	3	4 (Came in Under)
Time Schedule	1	2	3	4 (Finished Early)

2. Overall, how would you rate the success of this project? (CIRCLE THE NUMBER THAT BEST REFLECTS YOUR OPINION)

Total Failure		Partial Success		Total Success
1	2	3	4	5

3. AT THE TIME IT WAS INTRODUCED TO USERS, how far along was this technology on the scale below? (CIRCLE ONE)

Still an Unproven Concept	Concept Feasibility Demonstrated	Laboratory Prototype	Production Prototype	Production Model; Some "Bugs" Remaining	Ready for Use in Production
1	2	3	4	5	6

4. To what extent would this technology (if successfully implemented) give the company a proprietary advantage?

Not at All		Somewhat		A Great Deal
1	2	3	4	5

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