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RESEARCH ISSUES

CONCEPTUAL CHALLENGES IN CONTEMPORARY IS RESEARCH[1]

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ABSTRACT

This paper, originally prepared as a keynote address for the 1999 Australasian Conference on Information Systems, critically examines some common assumptions underlying much IS research. The assumptions concern the scope and risks of IT projects, the rationales for and uses of IT, and the role of history and time in systems-related outcomes. Making different assumptions about these issues suggests the need for new approaches to IS research.

Keywords: Research design, unit of analysis, time, uses of IT, IT risks, data warehousing, ERP systems

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I. INTRODUCTION

In my twenty-some year career of IS research, I have explored three broad research domains:

- power, politics and MIS implementation;
- electronic communication systems; and
- organizational transformation and enterprise systems.

In the last domain, much of my research has been funded by industrial sources. For example, the Advanced Practices Council of the Society for Information Management, International (SIM), funded my research with colleagues on the topics of: change management issues in business process reengineering, corporate use of internet, intranet and extranet technology, and next generation issues in inter-enterprise systems. Baan Research funded an investigation of enterprise resource planning (ERP) systems implementation. And the Financial Executives Research Foundation funded studies of managing the risks associated with large IT projects and the uses and benefits of data warehousing.

As I reflect on these funded studies, it is clear to me that I started out with a set of assumptions about IT project scope and risk, IT rationales and uses, and history and time that were challenged by empirical findings. My "experiences" in these projects suggest the value of a very different set of operating assumptions. Believing that some of you may share my initial assumptions, I discuss them here, along with my reasons for questioning them now. I conclude with implications for future research and a list of some research opportunities opened up by different assumptions on the three research themes.

II. SCOPE AND RISK

At the beginning of my career as an IS researcher, I studied an information system in an organization I called Golden Triangle (GTC). Some of you are familiar with that story, in which I explained resistance to a financial information system in terms of the political relationships between financial and managerial accountants. Roughly twenty years later, I found myself conducting a case study in a company that was implementing the financial modules of an enterprise software package. Superficially, the situation in this case was quite similar to that at GTC so many years before, but there were a number of striking differences.

The case of GTC involved implementation of a financial software package (called FIS) that handled the general ledger and produced a variety of financial statements for both financial and managerial accounting purposes (Markus, 1983). GTC had a multidivisional structure, in which divisions and operating groups (groups of divisions) had profit and loss responsibility and broad autonomy over such issues as accounting policy and systems selection. The company was formed by merging several other large companies. At first, the corporate controller had little influence over accounting matters within the formerly autonomous organizations. Over time, through repeated organizational restructurings, the corporate accounting unit took over some of the decision authority that had previously devolved to the operating units. FIS played an important role in this partial re-centralization of accounting. It provided for a common chart of accounts across all divisions and operating groups. Although the package had been designed for traditional file structures, GTC extensively modified it to work with a single database management system. The net effect of this change was to give corporate headquarters direct access to detailed financial data and statements at the division and group level, whereas previously headquarters had only had access to summarized information. With FIS, corporate could exert more control over the operating units than without FIS.

Twenty years later, I studied Microsoft Corp.'s implementation of SAP financials (Bashein, Markus, and Finley 1997) Microsoft had grown rapidly since its founding and by 1995 had 60 subsidiaries around the world, each with its own chart of accounts. Corporate consolidation was a nightmare—it took two weeks to complete (longer at the first close of the year), and corporate financial personnel estimated that they produced and distributed some 350,000 paper financial reports a year. The goals of the SAP implementation were several. One was to achieve worldwide financial integration. Therefore, SAP's financial modules were implemented worldwide with a common chart of accounts. The second was to enable do-it-yourself, paperless financial reporting. The corporate finance group wanted to get out of the financial statement preparation and distribution business. They planned to provide access to canned financial statements and ad hoc reporting capability via the company's intranet. A third objective was to showcase the Windows NT operating system. While the finance group stuck to their policy of not modifying SAP code for functionality, they did install the system on a beta version of NT, thereby creating the then world's largest SAP/NT installation.

The situations at GTC and Microsoft were similar, but the differences in scope and risk were vast. GTC's financial information system project turned out to be challenging for both technical and organizational reasons. Technically, modifying the package to work with the IMS DBMS was more difficult than planned, and the IS project leader left partway through the project. The project considerably overran its schedule (2½ years to the planned 1 year), and moderate difficulties were experienced during shakedown, largely attributable to poor training of accounting personnel. Organizationally, use of the system was strenuously resisted by the divisional accountants over a period of several years. The resistance was attributable to the fact that the system had been designed to provide headquarters personnel with access to detailed financial data and had *not* been designed to provide the divisions with adequate profitability analysis capability. (The divisional accountants had not been invited to participate in package selection or implementation.)

Despite these challenges, the business risks associated with FIS were relatively minor. The impacts of the system (increased corporate "interference" and increased workload associated with data input and manual integration with cost systems) were largely localized to hands-on divisional accounting users of the system. Had the development project failed, the corporation would have suffered some financial loss, but no lasting damage to its existence as a going concern.

By contrast, Microsoft's SAP implementation involved massive business and organizational risks. Had the world's largest SAP/NT integration failed to perform well, the company might have suffered irreparable reputational damage in one of its most important strategic thrusts. More importantly, Microsoft's integration of SAP data with the corporate intranet and end-user data analysis and reporting tools enabled major risks of inappropriate data access and use. Financial data and reports were potentially available to everyone in the company, making possible inadvertent disclosure or violation of SEC insider trading regulations. Microsoft took steps to control these risks, but the fact remains that the social scope of Microsoft's financial information system project was much greater than that of FIS at GTC, and its potential risk was much greater.

Today, it seems to me, systems projects in organizations are routinely more risky that those of a generation ago. Implementations of enterprise systems (like SAP) in large corporations have been known to cost in excess of $500 million, which is a massive investment even for the largest organizations. In addition, today's information systems are often integrated much more tightly with many other systems, increasing the chances that failure in one system will have catastrophic spillover effects (Perrow, 1984). For example, Microsoft's SAP project involved not only enterprise software, but also internetworking technology, data warehousing, and end-user computing tools. Finally, the increased power of today's technology and software enables systems to affect many more people and organizational processes than was formerly possible.

To summarize, I began this research with two implicit assumptions that now seem questionable. First, research scope and unit of analysis can be defined cleanly in terms of an IT project, a change initiative, or a technology such as ERP or data warehousing. Second, IT-related risks decrease over time as technology decisions are finalized, project milestones are passed, and bugs are shaken out.

Now, my assumptions are different. First, the appropriate units of analysis and boundaries for research in IS are often unclear and must be justified. Is it appropriate today to focus just on a "financial information system" (even one as "big" as SAP) without also studying the other systems and technologies to which it is intimately tied? My personal view is that Microsoft's implementation of SAP is much "bigger" in scope than many other SAP implementations because of its integration with data warehousing and the intranet. (On the other hand, it is also much "smaller" in scope, because it was a financials-only implementation.) Consequently, I wonder about the validity of studies that focus on single technologies like enterprise systems without also examining integrations with other systems and technologies. Put differently, in a study of enterprise system implementation, is an implementation of only the financial modules comparable to (and therefore co-analyzable with) implementations that involve the full suite of enterprise system modules? Is an implementation of an enterprise system with data warehousing comparable to an enterprise system implementation without data warehousing? And how do we take such differences into account when doing our research? Today's technologies force us Information Systems researchers to confront such questions squarely.

Second, the organizational risks associated with individual information systems and the societal risks of using information technology in general are rapidly increasing. The increase in risks can be attributed largely to integration of, and expanded access to, systems. These "facts" have important implications for IS research. Today, for example, our field tends to differentiate between risks associated with IS development (e.g., project failure) and risks associated with IS operation (e.g., security and reliability), which is how IS professionals manage them. But little has been done to understand IS risks across the total system lifecycle, which is how organizations experience them.

III. RATIONALES AND USES

The Microsoft case was part of a larger study of risks in large-scope IT projects (Bashein, Markus, and Finley 1984). Two other cases in this study involved enterprise software; a third involved data warehousing. In subsequent research projects for different funding agencies, I focused on specifically on these particular technologies. One study involved best practices in enterprise systems implementations (Markus and Tanis, 1999). The other focused on the uses and benefits of data warehousing (Bashein and Markus, 2000). At one level, these technologies are quite distinct. Enterprise software involves an organization's basic record keeping and transaction processing. Data warehousing addresses a company's needs for decision support and secondary data analysis. Therefore, I did not expect much synergy between these two studies.

In fact, I found the opposite. While interviewing company spokespeople about the reasons for pursuing data warehousing, I heard some of the same reasons that other companies gave for adopting enterprise systems. Kraft Foods and Cardinal Health, for example, wanted data integration across diverse business units and decided that the best way to meet their needs was selective transaction system redevelopment coupled with the use of data warehousing. By contrast, Microsoft and ALARIS selected enterprise software as their primary strategy for achieving data integration, but also used data warehousing to support their needs for data analysis and reporting. In yet a third variation, MacNeal-Swendler adopted enterprise software with the belief that it would satisfy their needs for data integration. It did not do so, however, and (10 years later) the company eventually solved the problem with data warehousing. In these instances, companies gave identical reasons for selecting technologies that we information systems specialists believe to be suited to very different ends. This is troubling from the perspective of research organized (as my two projects were) along technology lines.

Not only do companies give the same reasons for adopting different technologies, they also give disturbingly different reasons for adopting the same technology. Nowhere was this clearer to me than in my study of enterprise systems. The prescriptive literature on enterprise systems argues strongly that companies should adopt enterprise systems for strategic business reasons such as presenting "one face to the customer," achieving worldwide "available to promise" capabilities, standardizing on best practices worldwide, or achieving dramatic reductions in inventory carrying costs. And some companies I researched or read about did in fact adopt enterprise systems for such reasons. Examples include Quantum Corp., Elf Atochem, and BICC Cables. However, a surprisingly large number of companies adopt enterprise systems largely for financial and technical reasons: Y2K compliance, inability of legacy systems to accommodate company growth, desire to migrate off mainframes, reporting difficulties due to lack of data integration across systems, high maintenance expense associated with legacy systems (Hirt and Swanson, 2000: Ross 1999). I have interviewed people who confessed that business issues and impacts never entered into their companies' enterprise systems adoption decisions.

These data are troubling for both practical and academic reasons. Practically, we know that top management commitment and involvement is a critical success factor in IS implementations. Intuitively, it seems obvious that top managers' commitment and involvement will be greater both in quantity and in quality, if they believe that a project addresses a strategic business imperative than if they believe that a project merely makes good financial and technical sense. Therefore, it is troubling practically to see organizations pursuing projects that aren't structured to gain managerially commitment.

Academically, these data are troubling, because the "experience cycles" of business-driven enterprise systems implementations differ so much from those of financially and technically driven implementations that the two can hardly be compared meaningfully. For example, in many financially and technically driven enterprise systems implementations the first major step in the project history is software evaluation (e.g., vendor-initiated sales presentations or a formal software "selection" process). By contrast, in business-driven implementations, the first steps usually involve consensus building and education. When BICC Cables decided to entertain the possibility of enterprise systems adoption, the company began a five-year process of organization development, of which software selection was the *last* step. First, they hired their first-ever CIO and created a task force to model the business processes (using a technique popularized by business process reengineering) at a typical local site. Next, they had each local unit validate the process model against their own operations. Only after they had established to nearly everyone's satisfaction that processes were common across the organization, did they move to the next step of searching for the best enterprise software package to meet these common needs. The software selection process, too, was performed by a committee composed of representatives from operations around the globe (Markus, 1999). Similarly, another company's business-driven enterprise systems implementation began with APICS education for senior executives and members of all business units worldwide.

In business-driven enterprise systems projects, the process looks more like large-scale organizational development or change management than it looks like traditional IS project management. Technically driven projects are much more conventional in life cycle. I believe this difference has important practical implications for business success with enterprise systems. But it also poses challenges to researchers: When companies are doing "the same thing" for very different reasons and in very different ways, are they really doing "the same thing"? I don't think so. Different goals and approaches make for different implementation processes and outcomes.

Similar concerns arose in my study of data warehousing projects (Bashein and Markus, 2000). My colleague, Barbara Bashein, and I interviewed in over 20 companies and studied 6 cases in depth. The prescriptive literature on data warehousing had led us to believe that *the* reason for building a data warehouse was to acquire the capability for data mining, which is the discovery of new empirical relationships (like the young father's apocryphal joint purchases of diapers and beer). By contrast, we found that few companies, at least today, use their data warehouses for data mining purposes. Further, we found at least three additional uses of data warehousing technology, all of which provide clear business value. The first such use I've already spoken of—to achieve partial integration of data and systems in settings where enterprise software was not a preferred solution. The second use was for traditional data analysis and reporting (that is, decision support with no "knowledge discovery" involved). A third use of data warehousing was as an engine for new data products developed to be sold externally. We have concluded that, at least at the current stage of its worldwide diffusion, data warehousing is not one "technology" but four. These different uses of data warehousing have different motivations, different implementation issues, and different potential impacts. To summarize, my research on “technologies,” such as data warehousing and enterprise systems, implicitly assumed that there are more or less distinct uses and motivations to use particular technologies (e.g., data warehousing) and that “different” technologies (e.g., data warehousing and ERP) have different motivations and uses. Today, it is clear to me that:

1. A single technology (e.g., ERP) can have many different rationales, associated with different implementation processes.
2. A single technology (e.g., data warehousing) can have many distinctly different uses.
3. Different technologies can have the same uses. These observations have implications for the IS field’s common practice of defining studies in terms of particular technologies rather than in terms of motivations and/or uses. Survey research on IT impacts in particular should attempt to distinguish among different patterns of technology use.

IV. HISTORY AND TIME

In my discussion of the data warehousing research, I made several references to time. I said that, today at least, not many companies use data warehouses for data mining. However, I hold open the possibility that they may do so in the future. In my investigations of data mining, it became clear to me that data mining (or knowledge discovery, as it is sometimes called) represents a major innovation in business decision making. It involves a way of thinking about data that is not commonly taught in business schools, let alone commonly adopted in businesses. Furthermore, many businesses have not yet adopted the statistical hypothesis testing and formal decision modeling approaches that *are* taught in business schools: most business decisions today are made on an ad hoc, intuitive basis. Regardless of whether or not the current situation is good, bad, inevitable, or fixable, the attempt to diffuse data mining throughout society is bound to be a long, slow process. It is a process likely to unfold over decades rather than months or years.

It seems counterfactual to speak today about the slowness of IT diffusion in a climate where we speak of “Internet time” and daily see IT-related changes in business and personal life. Yet, today’s innovations represent the fruits of 25 years of internetworking experience, not to mention over 50 years of experience with transaction processing technology. Today’s events are merely the current manifestations of a long developmental trajectory that has no foreseeable culmination.

This observation has two important implications for IS research. First, almost any research study is a best a snapshot of a moving target, and our specific findings (as opposed to theoretical generalizations) are almost certainly likely to hold only for short periods of time. So, for example, when Wanda Orlikowski studied a branch of an unidentified consulting firm, she found relatively low levels of adoption and use of a groupware technology (Lotus Notes) (Orlikowski, 1993). Today, all such consulting firms brag with justification about their use of knowledge management technologies. Wanda was not wrong in her observations, and the consulting firms are not wrong in their claims. The fact is that times have changed, as has the use of groupware at Alpha Corp. Similarly, the companies Barbara Bashein and I studied that were not doing data mining last year may do so next year. The empirical findings of IS research come with built-in expiration dates, and our field has yet to come to grips with the challenges of “findings freshness.”

A second implication of today’s events as a manifestation of a long developmental trajectory is that studies of IT use in business that do not take history into account are likely to make incorrect attributions of causes and effects. I continue to be amazed at the length of time over which organizations’ major IT-enabled strategic business thrusts unfold. For example, McKenney and colleagues start their story of American Airlines’ Sabre system in 1943 (development started in 1955) and end by acknowledging that the story is ongoing (McKenney, Copeland, and Mason, 1995). Lynda Applegate’s valuable series of cases show that data warehousing and decision-support at Frito-Lay evolved out of IT investments in infrastructure and hand-held computing for route drivers many years before (Applegate, 1997). In my own studies, informants routinely begin describing technology initiatives at some point years in the past—for instance, a long ago reorganization that decentralized computing, a merger that created needs for business integration, or an earlier failed IT project with similar goals.

It is undoubtedly true that business conditions and organizational motivations related to technology change over time. And it is equally true that technology itself evolves. These factors make it difficult to understand the effects of history on the present. But without historical context, our ability to understand the motivations behind, and impacts of, current technologies is limited.

Put differently, there is a high degree of "path-dependence" in organizations' uses of information technology. The problems they are trying to solve with technology today are not merely a function of currently available technologies and immediate business challenges and opportunities. They are also a function of the organizations' long-past decisions about IT management, IT spending, and IT infrastructure (Weill and Broadbent, 1998). For researchers, path dependence makes it enormously difficult to compare the experiences of two different companies presently adopting similar technologies. The challenge is to work with such intractable raw material to produce a cumulative body of knowledge about information systems.

To summarize, much IS research is apparently ahistorical, that is, it does not attend to the potential effects of time and history. The implicit assumptions of ahistorical research are that:

1. each new project/initiative is independent of prior projects/initiatives,
2. that history and path dependence do not matter to the outcomes, and
3. that what is observed today will continue to be the case tomorrow.

Instead, the following assumptions are more defensible in light of much IS research.

- First, new IT projects are often related to prior efforts, even though the prior efforts might have been called by different names. Thus, today's data warehousing projects often complement or replace efforts that were formerly called executive information systems or decision support systems. And, what is today's business-to-business e-commerce is importantly related to yesterday's EDI.

- Second, today's IT projects have long histories. It is frequently the case that today's successful "new" projects have their roots in "preexisting conditions" that originated far in the past or follow on failed or less successful attempts to accomplish the same goals with earlier technologies. So, for example, the data warehousing projects at Cardinal Foods and Kraft were motivated by mergers that occurred some years before the technology projects were attempted. And Microsoft had two failed attempts to implement ERP before they succeeded with SAP financials.
- Third, today's projects are often destined to have long futures, during which time the behaviors and outcomes observable today will undoubtedly change. McKenney's history showed that American Airlines' SABRE system did not always give American an advantage over competitors. And Applegate's history of Frito-Lay's handheld computer system demonstrates evolution, integration with new technologies such as data warehousing, and emergent uses.
- Fourth, organizations' choices of technologies and systems are constrained and enabled by business history and past technology choices. For example, the mergers at Kraft Foods and Cardinal Health created conditions that demanded systems and/or data integration. For another example, the history and culture at Kraft Foods made ERP an unacceptable way to achieve systems and data integration.
- Finally, organizations (sometimes) learn. Orlikowski's Alpha Corp is now heavily using Notes or a technology like it. The financial analysis at Kraft Foods may begin aggressively mining the data in their warehouses tomorrow.

V. IMPLICATIONS FOR IS RESEARCH

Table 1 compares common assumptions about IT scope and risk, rationales and uses, and history and time with the assumptions that seem more reasonable in light of my and others' research findings. I would distill this learning into two observations.

First, the effects of time and of systems integration render problematic the important research design tasks of defining the unit of analysis and setting study scope and boundaries. Several factors should be, but frequently are not, taken into account when we design our research projects: the influences of *prior* business and technology projects, the influences of *related* contemporaneous business and technology projects, the effects of learning, growth, and change on the outcomes of interest, and potential future lifecycle issues such as the changes that might occur during the shakedown and ongoing use of a particular focal technology. Taken together these considerations suggest that specific technologies or IT projects may not always be the most appropriate research foci or ways of bounding our research investigations. Instead, it may be better to use an alternative criterion for selecting what to study. Examples include common corporate goals or objectives for IT investments or common ways of using a particular technology or set of technologies.

Second, the rationales people and organizations give for their IT adoption decisions and the uses to which they put the IT they adopt are consequential for the outcomes they experience. Therefore, more IS research should explicitly examine the different rationales for adopting a particular technology and the different ways of using a particular technology.

Some readers may fear that I am advocating a purely idiographic approach to IS research, that I am promoting abandonment of the search for general and generalizable findings, that I am insisting upon the exclusive use of longitudinal research designs. I am not. I believe that we can effectively use historical and comparative analysis to find valid patterns in the complexity of organizational behavior with information technology. In the next section, I outline

Table 1. Assumptions about and Experience with Three Research Themes

Research Theme : Scope and Risks

Assumptions:

- The unit of analysis & scope of research can be clearly defined
  - project/initiative
  - technology (ERP or data warehouse)
- IT-related risks decrease over time as
  - technology decisions are finalized
  - project milestones are passed
  - bugs are shaken out

Experience:

- Unit of analysis & boundaries are unclear
- New technologies are integrated with old ones
- Access to technologies increases
- Risks of individual systems increase over time
- Risks of systems in general increase over time

RESEARCH THEME : RATIONALE AND USES

Assumptions:

- A specific technology/project has distinct motivations for use & distinct uses
- Different technologies have different motivations & uses

Experience:

- Single technology has many different rationales & implementation processes, e.g., ERP
  - technically motivated projects (Y2K)
  - business driven projects (ATP, one face to customer)
- Single technology has many distinct uses, e.g., data warehousing
  - routine management reporting
  - data & systems integration
  - data mining
  - new data products
- Different technologies have same uses
  - both ERP & DW used for data & systems integration

RESEARCH THEME: HISTORY AND TIME

Assumptions:

- Each new project/initiative is independent of prior projects/initiatives
- History (path dependence) does not matter to the outcomes
- What we observe today will continue to be true in the future

Experience:

- IT projects are often related to prior efforts
  - today's data warehouse replaces yesterday's EIS
  - today's ecommerce project builds on yesterday's EDI
- Today's projects have long histories
  - Cardinal Health's & Kraft's data warehousing efforts were motivated by merger histories
  - Microsoft failed in two attempts to implement ERP before succeeding with SAP financials
- Today's projects have long futures
  - McKenney's history of American Airline's Sabre
  - Applegate's history of Frito-Lay's HHC & data warehouse
- Organizations' choices are constrained as well as enabled by business history & past technology choices
  - Kraft Foods' mergers demanded integration
  - ERP was not an option (culturally) for Kraft Foods
- Organizations learn
  - Orlikowski's Alpha Corp is now using Notes
  - Kraft Foods may start data mining tomorrow

some of the exciting research opportunities that are opened up by embracing the challenging assumptions I have outlined above.

VI. RESEARCH OPPORTUNITIES

One promising new research direction for our field is an integrated approach to IT-related risk. Today, IS research differentiates between development risk (e.g., project failure) and operational risk (e.g., security and reliability). But organizations experience risks across the systems lifecycle, and decisions during development have consequences for operations. Further, IS research examines many different types of risk—separately. Privacy is a growing area of IS research, but privacy issues are rarely studied in conjunction with other important IT-related risks such as project failure. An integrated approach to the study IT-related risk has the potential to benefit organizations greatly.

A second promising research direction for our field is historical analysis. We've just had the 50th anniversary of the Association of Computing Machinery; we certainly have a wealth of historical experience with information technology and systems. Furthermore, we have a wealth of *prior IS research* that has much to say about the new technology developments of today. ERP systems can be viewed in part as extensions of TPS (transaction processing systems)—the subject of much prior IS research. Similarly, data warehousing and data mining can be viewed as the fulfillment of the promise of DSS and EIS. And Internet technology should be studied in the context of prior research on EDI and inter-organizational systems. As we study today's new technologies, we need to understand how they relate to the technologies and ideas of the past.

Comparative histories also offer a promising research opportunity. I've already mentioned the apparent differences in implementation process between technology-driven and business-driven IT projects. Another example involves comparing the progress of projects with substantially different starting conditions. For example, are there systematic differences in implementation process and outcomes in ERP projects in organizations with a history of IT decentralization compared to organizations with a history of strong central control over IT? Are there systematic differences in implementation process and outcomes in data warehousing projects in organizations with a high degree of systems integration than in organizations with fragmented systems? I believe the answer to these questions is yes, and I'd love to read the studies that address these points.

Another opportunity involves units of analysis other than technology, projects, and organizations. Alternative units include business processes, supply chains, value systems, and "strategic groups" (of companies). Finally, opportunities can be found in new criteria for selecting research sites and analyzing data. For example, what would we learn if we compared the IT efforts in a sample of organizations that shared a common goal or objective for technology, but differed in the technology employed? What would we find if we studied the outcomes of data warehousing projects controlling for the uses to which that technology had been put?

All in all, different assumptions about IT project scope and risks, about the rationales and uses of IT, and about history and time suggest many promising new lines of research for our field. I look forward to the fruits of the research conducted along these lines.

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