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An Example of Relevant IS Research for Top Managers on IT Project Failure

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Abstract

This paper attempts to progress the development of a more relevant IS research tradition. It describes an application of Benbasat & Zmud's (1999) recommendations for conducting relevant research to explain how top managers influence IT projects to succeed. The findings challenged the main emphasis of the common IT prescriptions and explained why the success rates of IT projects has been so inconsistent. The research provides a example of how to overcome the fragmentation in the field and if it achieves its goal of influencing management and IT audiences, it will serve as an exemplar of relevant research. It is notable for its use of the pragmatic paradigm and collaboration leading its publication by Standards Australia.

Keywords

Relevance, top management support, IT project failure, IT project governance, pragmatic paradigm

INTRODUCTION

Benbasat and Zmud's (1999) have been strong advocates for more relevance in IS research and have suggested (2003) future research should build on a relatively narrow technical IS core. Lytinen and King (2004) echo the call for relevance but question the need for a defined core. Others have proposed broader frameworks capable of incorporating organisational issues (Alter 1999, Bacon and Fitzgerald 2001).

Davenport and Markus (1999) argue IS should lead in relevance by emulating colleagues in medicine and law rather than in the business schools. Lee (1999) develops this argument by pointing out that medicine and law are not natural sciences but professions and IS would do well to adopt the research goal of the professions – relevance. He explains relevance requires the production of knowledge about how to intervene in the world and change it to satisfy real world needs in contrast to the natural science tradition (which formulates, tests and validates theories about the relationships between independent and dependent variables).

However the call for relevance has not progressed very far and is sometimes confused as a choice between relevance and rigour. Benbasat and Zmud (1999) clarify that relevance is not an excuse to compromise rigour because “both academic researchers and consultants value the discovery and application of new ideas and solutions” but “it is the academic who is more concerned with issues of justification (i.e. ensuring what is being discovered and applied is in fact ‘correct’)” (Benbasat and Zmud 1999 p12).

This paper attempts to progress the development of a more relevant IS research tradition. It will start by describing how a loss of relevance of IS research in general may be affecting the survival of the field. It will then describe the loss of relevance of IS research in the area of IT project failure and describe how case study research was conducted to try to address both issues.

THE NEED FOR RELEVANCE

Fitzgerald and Adam (1996) have traced the emergence of IS discipline to the inability of computer technologists to overcome the tendency to view the issues from a narrow technical perspective (Currie and Galliers 1999). The IS literature as a whole is seen to be fragmented and without a common body of knowledge (Benbasat and Weber 1996). Hirschheim and Klein (2003) believe that significant communication gaps have arisen between IS academics, non-IS academics, practitioners and executives because there are few cohesive frameworks for understanding. The IS literature has become largely irrelevant to not only executives and IS practitioners but also to IS students (Davenport and Markus 1999).

Fragmentation

One of the reasons it has been difficult for the IS field to reach beyond its technical roots and embrace organisational issues is historical (Benbasat and Weber 1996). It has struggled to establish itself as a field

because of challenges from influential academics. Near its inception some claimed MIS is a mirage and “embedded in a mish-mash of fuzzy thinking and incomprehensible jargon” (Dearden 1972 p90). More recently Carr (2003) has kept up the assault with his claim that “IT doesn’t matter”.

Benbasat and Weber (1996) believe the need to establish legitimacy lead North American researchers to adopt Keen’s (ICIS 1980) keynote suggestions to borrow concepts and theories from reference disciplines such as the management, cognitive and organisational sciences and economics. However the widespread adoption (Orlikowski and Baroudi 1991, Goles and Hirschheim 2000) of different reference discipline concepts and methodologies has fragmented the field and lead to a loss of relevance (Benbasat and Weber 1996).

Loss of relevance affecting survival of the field

Fitzgerald and Adam (1996) believe that fragmentation has lead to an abundance of trivial academic research and a lack of depth and they state that a very large intellectual investment is now needed to understand the breadth of the IS literature. IS research is considered by many to be ineffective and difficult to apply (Lyytinen 1987, Fitzgerald and Howcroft 1998, Sauer 1999, Bacon and Fitzgerald 2001).

Benbasat and Zmud (1999) quote the dean of a business school to have said “As much as 80% of management research may be irrelevant” and show the criticism applies equally to IS research. Markus (1999) is another senior IS researcher suggesting loss of relevance is leading to a crisis in the field. Whole issues of leading IS journals have been devoted to the issue (MISQ 1999 23:1, JAIS 2003 4:5).

“If we focussed our research solely on IT, then the IS field as a whole could well be seen as irrelevant by senior business leaders in just a few short years.” (Myers 2003 p582)

A major problem is that the attempt to conceptualise complex organisational IS phenomena in mechanistic rational scientific terms “may never have been appropriate for the study of IS” (Hirschheim 1985, Varey et al. 2002 p234). Neither Lee (1999) nor Sauer (1999) believe that organisational issues are amenable to simplistic positivist factor research because there are a large number of highly interrelated factors that are difficult to mathematically isolate to ‘prove’ what causes certain outcomes. Robey and Boudreau (2000) have found that empirical findings spread over decades of research have not produced convincing evidence supporting simple causal models.

Varey et. al’s (2002) comments were also directed at the growing body of interpretive IS research that arose as a reaction to the shortcomings of positivist research (Walsham 1995). Fitzgerald and Howcroft (1998) point out that the interpretivist ideal of holding all interpretations as equally valid is not tenable in practice. They add that the situation is exacerbated because IS researchers are polarised and “operate in blind and slavish adherence to the extreme poles of their particular research approach”. Davenport and Markus (1999) have pointed out that in this environment not even IS students are required to read IS research.

Current status of the issue

The issue of relevance in IS research would not be important except that signs suggest (Cox 2003, Ein-Dor 2003) that investments in IT should increase from the current levels of \$1,000,000,000,000 per annum (Seddon et al. 2002). To unlock the fifty-fold improvements in productivity promised by the information age (Covey 2004), IT continues to represent for many organisations, their main source of opportunity (Clegg et al. 1997). However the goal has been elusive and evidence suggests organisations are consistently repeating the same mistakes (Collins and Bicknell 1997).

Boards are looking for guidance (Young and Jordan 2002a) and IS research clearly has a role to play. Unfortunately because IS research is lacking in relevance, especially to this wider audience, it is failing to contribute where it could add the most value.

A recent discussion on ISWORLD suggests the problem may have become worse. The discussion started with the suspicion that “the IS community is diminishing not only in terms of people interested in IS but also in our perceived influence to society and business” (Lytras 2005). Over 32 contributions followed: questioning why IS and computing were in different departments, defining the scope of IS to be about the effects of IS on organisations, people and society (and therefore different to computing), noting the difficulty of research because of the complex phenomena and fractured nature of the field, and there was an extensive side-discussion on the relative merits of system thinking. Lytras noted the introversion of many of the responses pointing out that “nobody in the real world cares about the distinction about IS and CS” yet even he did not feel that the business orientation of IS would be the key issue of the next decade. Very few people gave the impression they felt that

relevance to those “funding” (Wynn) or “controlling resources” (Murphy) was important (Bider) and no one suggested any guidelines for conducting more relevant research. The discussion was particularly disheartening because one contributor, without reference to relevance, suggested the issue should have been resolved when the definitive IS curriculum was agreed in 2001 (<http://www.computer.org/education/cc2001/>).

IT PROJECT FAILURE

Research into IT project failure is a specific example of how IS research has lacked relevance. IT project failure has been an issue almost since the dawn of business computing (Caminer 1958). It has been studied intensively for the past 40 years (Lucas 1975, Lyytinen and Hirschheim 1987) but it remains a poorly understood phenomena (Sauer 1993, 1999). An enormous number (Lyytinen 1987, Yardley 2002) of largely untested methodologies (Checkland 1981, Strassmann 1995) have been proposed and adopted (Clegg et al. 1997). However the failure statistics have not improved significantly and strongly suggest that IS research on project failure is ineffective and irrelevant (Willcocks and Margetts 1994, Standish 1999, Keil and Mann 2000, Standish 2003). Kraemer and King (1986) have found the dimensions of computing policy thought to be important (user involvement, steering committees, policy boards) have no consistent impact on the success of computing.

Over-emphasising project management

The majority of IT researchers and practitioners have persisted with technological / engineering conceptions of the problem (Currie and Galliers 1999). They continue to focus mainly on factors they can directly influence (Schmidt et al. 2001) even though there is strong evidence that it is better understood as a soft organisational issue (Lyytinen 1987, Bussen and Myers 1997)(Sauer 1999). Thomsett (1989) has suggested that IT advisors are having difficulty expanding their technical world-view to embrace organisational issues and other issues outside their direct influence because they are unwilling to undermine their expert status.

Grindley (1995) reports that since 1980, meeting project deadlines has been the number one or number two concern of IT directors. There is an increasing concern to try to pick the right projects but there seems to be almost no awareness of the need for managers to focus on their role in realising the benefits of IT investments. Even the project management literature recognises the emphasis on on-time on-budget to be misguided (Hinton and Kaye 1996, Baccarini 1999, Cooke-Davies 2002, Smyrk 2002).

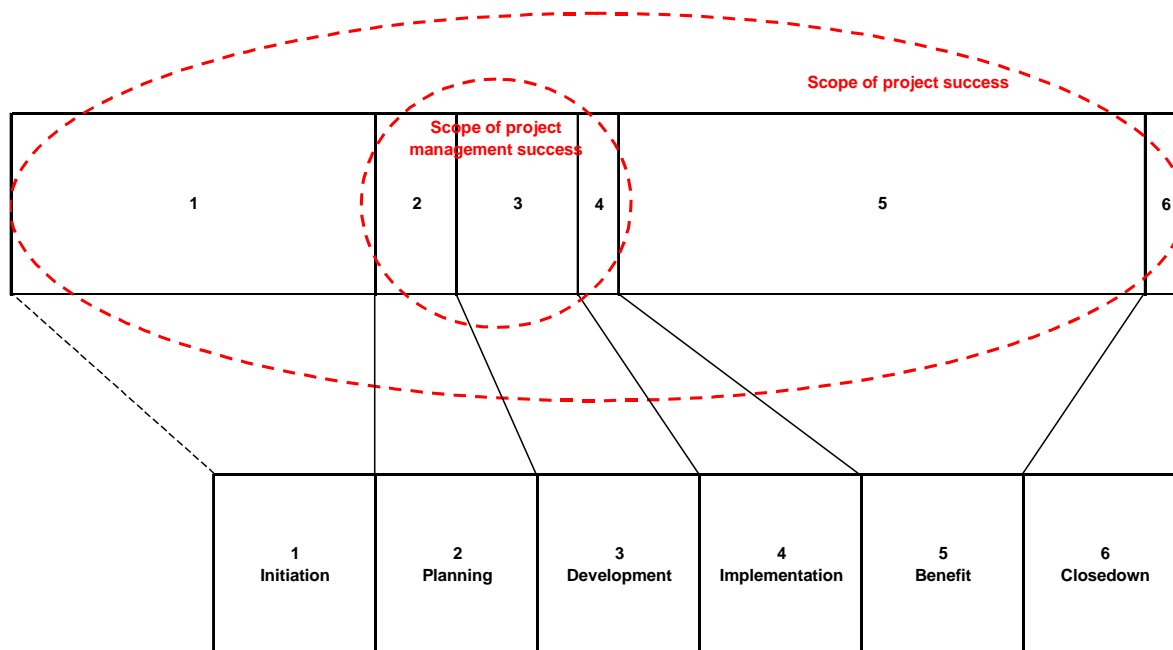


Figure 1: The scope of project management and project success

De Wit (1985) has made it clear that project success is fundamentally different to project management success. The difference can be seen in Figure 1, an adaptation of Yardley’s (2002) six stage conception of a project lifecycle (initiation, planning, development, implementation, benefit, closedown). Project management success only relates to the three smaller stages in the lifecycle of a project (planning, development and implementation),

while project success relates to all six. Project management success is relatively unimportant because the majority of the benefits of an IT investment are realised after a project team has disbanded.

Markus' research (Markus and Keil 1994, Markus et al. 2000) strongly reinforces the message that project management measures of success are not the most appropriate focus for business users. She has shown there is not a strong relationship between project management success and project success or between project management failure and project failure (ie. one does not lead to another). She also points to change agency as a more appropriate focus for business participants in IT projects (Markus 1996).

Ignoring organisational factors

Lucas (1975) was one of the first to warn that the emphasis on technical issues and project management was misguided. He pointed out as early as 1975 that "the major reason most information systems have failed is that we have ignored organizational behaviour problems ...".

Alter, a former business executive now working in IT, has developed a useful framing of all IT systems in the context of the business (Alter 1999). He says "the challenge of applying IT successfully falls as much on business professionals as on IT professionals ... highly technical work such as programming usually takes up less than 20% of the effort". Most members of the Communications of the Association of Information Systems (CAIS) editorial board agreed with his broadening of focus but some argued his definition was not broad enough to include inter-organisational, institutional, industrial, national or international IS phenomena (Alter 2003a). Guthrie made the observation that "specialization makes us more distinct but cross-functional research makes us relevant" (Guthrie 2003 p559). However this discussion has had little mainstream influence with "research directly related to systems in organizations ... underrepresented in our IS journals and research" (Alter 2003b).

Ward et al. (1996) argue that the focus should be on benefits management. Brynjolfsson and Hitt (1998) have shown it takes a relatively long time to realise benefits from an investment and they have suggested that more benefits are obtained when top management can sustain a vision of what will be achieved (over 1-8 years) because they need to tolerate a period of initial decreased performance. Interestingly, this implies it is mainly not the CIO nor IT managers who need to keep the faith but top management.

Weill (2004) echoes this conclusion with his finding that firms with good IT governance have more direct involvement of top managers. He states that the CIO is a necessary but not sufficient condition for effective governance and states the challenge is to find the right role for each senior manager. He estimates the relative importance of various senior managers as follows: Chief Information Officer (1.0), Chief Financial Officer (1.2), Business unit CIO (1.3), Business unit leader (1.6), Chief Operating Officer (1.7), Chief Executive Officer (2.1) (Weill 2004 p126-127).

The need to focus on top management support

It would seem that all these warnings and insights have been largely ignored. It is intuitively obvious that organizational issues are strongly influenced by top management and research has long confirmed top management support as one of the most critical factors for success (Garrity 1963, Rockart 1988, Henderson 1990, Bassellier et al. 2001, Schmidt et al. 2001). Most IT practitioners feel that the commonly held heuristic for top management support could not be very far from wrong (Markus 1983) yet their advice to senior managers goes largely unheeded (Emery 1990).

The generic advice to senior management is simplistic with prescriptions for communication, enthusiasm, involvement and participation (Schmitt and Kozar 1978, Izzo 1987, Lederer and Mendelow 1988, Jarvenpaa and Ives 1991). Some recommendations are very demanding of top management time but the justification for the effort is couched in terms of improving technical quality or user satisfaction (Brandon 1970, Dinter 1971, Doll 1985, Izzo 1987) rather than any objective of direct concern to senior managers (Young 2005). Managerial involvement is generally promoted as inherently good (Mähring 2002a) yet there is clear evidence that inappropriate managerial support leads to dysfunctional behaviour, escalation and failures (Keil 1995, Collins and Bicknell 1997). The common advice is further undermined by examples of following the prescriptions without realising any benefits (Markus and Keil 1994).

"With technocrats, the only three things you can be sure of are: nothing would get finished on time, it would always cost vastly more than predicted and it would never do what it was promised to do" (Young and Jordan 2002b).

Much research revolves around a theme for top managers to contribute to strategic direction setting (Garrity 1963, O'Toole and O'Toole 1966, Rockart and Crescenzi 1984, Lane 1985, Doll and Vonderembse 1987) There have been few significant advances on this beyond recognising the need to facilitate operational involvement (Rockart 1988, Henderson 1990). Sharma and Yetton (2003) have clarified that top management support becomes more important as more organisational boundaries are crossed (task interdependence) but so little progress has been made that Bassellier and Pinsonneault (1998) have had to revisit the literature to try to define how to measure senior management support.

The sobering conclusion is that decades of neglect have left us knowing relatively little about what top management support is and how it is provided (Bassellier and Pinsonneault 1998, Mähring 2002b). Researchers have yet to determine “if, when, how much, and what type of executive support is likely or organizationally appropriate” (Jarvenpaa and Ives 1991). It would seem that because the emphasis of research on IT project failure has remained at the level of project management (Lyytinen 1987), the research that has the most potential to improve success rates is perceived to be irrelevant and is failing to engage top management audiences (Thomas et al. 2002). There seems to be little or no awareness of the need for top managers to focus on benefit realisation.

CONDUCTING MORE RELEVANT RESEARCH

The review of the literature on IT project failure has established it as an area in need of more relevant research. The remainder of the paper will describe an attempt to meet both this need and the need raised in the earlier discussion to develop a more relevant IS research tradition. It will start by reviewing Benbasat and Zmud's (1999) guidelines for conducting relevant research. It will then show how these guidelines were applied in a research program investigating the relationship between top management support and IT project failure. In doing so, it introduces the pragmatic research paradigm. Considerations of length prevent an exploration of the findings in any detail so the paper refers the reader to the original research (Young 2005) and concludes with some comments of how relevant research might be conducted in general.

Guidelines for relevant research

Benbasat and Zmud (1999) have suggested that relevant research starts with the selection of a topic that is a long-term critical success factors, an ongoing issue to which we have been unable to find answers and likely to be important in 3-5 years. They believe relevant research should produce recommendations that are implementable in practice, synthesise a body of research or stimulate critical thinking.

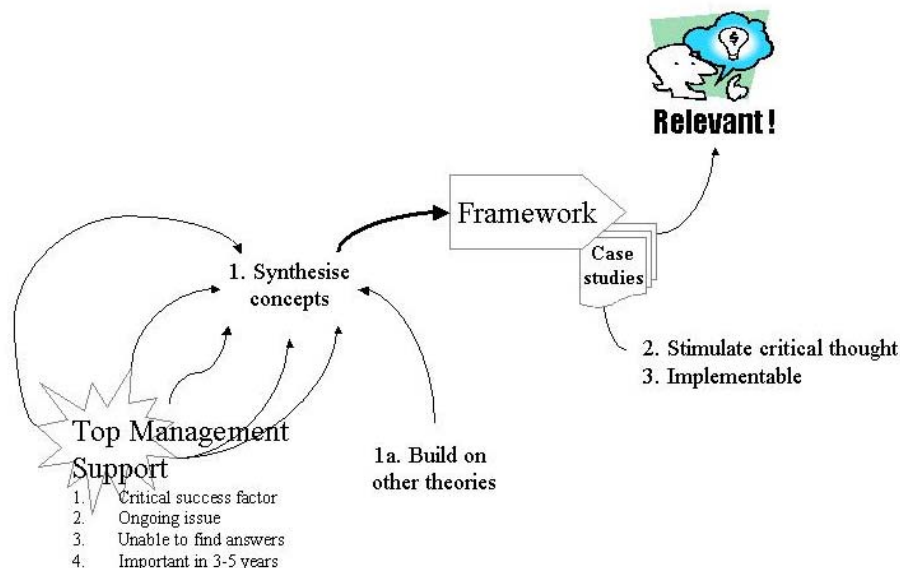


Figure 2 Dimensions of relevant research (based on suggestions by Benbasat and Zmud 1999)

Benbasat and Zmud's (1999) general approach is illustrated schematically in figure 2. They recommend the first step should be to synthesise concepts and build on other theories to “develop frames of reference that are intuitively meaningful to practitioners”, to “reorganise phenomena such that they seem less complex” (Davis

1971). This approach responds to the call by Orlikowski and Iacono (2001) to build on others work and promote a cumulative research tradition.

Benbasat and Zmud (1999) suggest the next step should be to conduct research to test frameworks, stimulate critical thought and find implementable solutions. They particularly encourage the use of descriptive case studies because they are often effective means to communicate contributions to practice.

Synthesising Concepts

Davenport and Markus (1999) have commented that a cumulative research tradition is a potential disadvantage in the IS field because it operates in an era of rapid business and technology change. However Young (2005) found that following Benbasat and Zmud's (1999) guidelines for choice of research topic negated the issue. He found that long-term issues such as top management support required a deeper level of conceptual development that is not significantly affected by rapid business or technology changes.

Young (2005) believes that one of his major contributions was not to discover new critical success factors, but to synthesise a fragmented body of knowledge. His specific contribution was to recognise the source of much confusion were the multiple definitions of success and failure (Seddon et al. 1999, Seddon et al. 2002, Delone and McLean 2003) being used dysfunctionally to hide project underperformance. He argued that for progress to be made, an appropriate definition of success had to be adopted at top management level. He developed an IT governance framework for testing by synthesising the multiple definitions of success and failure and synthesising the corporate governance, risk management, project management and IS literatures.

The relevance of this contribution is that it provided a crucial conceptual tool to help stakeholders, top managers in particular, understand how to prioritise the plethora of advice and influence projects to succeed. It is not possible to go into detail but the key concepts were that IT projects deliver value mainly by enabling improvements in work practices (Alter 1999) and that the benefits are delivered more by operational staff than by project teams (Cooke-Davies 2002). It showed that IT project governance is necessary to facilitate the realisation of benefits (by driving through process change while managing risk).

The influence of the research question and research purpose

Young's (2005) research question was to try to explain how top managers influenced IT projects to succeed and his research objective was to influence top management behaviour and improve IT project success rates. This formulation followed Benbasat and Zmud's (1999) call for IS research to be "biased toward the future", Goles and Hirschheim's (2000) suggestion research be focussed on the likely outcomes and Tashakkori and Teddlie's (2003) advice that the usefulness or value of research be determined by the test of time and practice. Collectively this implied conceptual frameworks needed be developed in collaboration with practitioners and thought leaders because without industry support, there is less chance of influencing practice.

At first glance this might seem to be an imposing requirement but Young (2005) found that this requirement is easily met if the research is focussed on being relevant. His main collaboration partner was Standards Australia who immediately recognised the enormous potential of "cracking the problem of IT failure". They helped in discussion to redirect the research question from the original topic of 'IT project risk' to the much more topical theme of 'IT project governance'. They then convened a specific working group of around 170 IT practitioners, industry representatives and academics to focus on ICT project governance (IT-30). The relevance of the topic was confirmed because the common objective of the working group was to ensure findings were useful and likely to influence practice to produce better outcomes.

It is interesting to note that the literature reviews and the interim results of the research were used to inform IT-30 discussions but that the input was not always well received. Many members of IT-30, particularly some of the IT practitioners, did not share the view that alternative approaches were necessary. The discussions were valuable because they exposed commonly held convictions that were contrary to the findings in the literature and helped shape the final presentation of the findings to maximise its potential to influence.

The working group was invaluable because they not only participated in an informal Delphi survey to help identify potential case study organisations, but they also provided contacts to gain access to research organisations. Access to organisations was much less successful without direct industry referrals and the quality of the research was significantly influenced by the high levels of collaboration.

Young's experience suggests that the degree of industry support is an important indicator of relevance and quality of research.

The influence of the research paradigm on case study research

The IT project governance framework was tested using case study research following Benbasat and Zmud's (1999) second guideline. The choice of the case study methodology was uncontroversial because top management support deals with organisation phenomena where the boundaries between the phenomena and context were not clearly evident and because a 'how' question was being asked about a contemporary set of events over which the investigator had little or no control (Yin 2003). However the choice of the case study methodology did not determine the research paradigm (Tashakkori and Teddlie 2003).

Fitzgerald and Howcroft (1998) explain that paradigms can be understood at varying levels of abstraction: at the level of ontology (relativist vs realist), epistemology (subjective/interpretivist vs objective/positivist), methodology (qualitative/exploratory/inductive vs quantitative/confirmatory/deductive) and axiology (relevant vs rigorous). This is helpful because it makes it clear that the outstanding paradigm issues are the level of ontological stance (the assumed nature of reality) and epistemological approach (determining valid ways of gaining new knowledge).

Ontology and epistemology have been the subject of much debate in the IS field, but a plurality of researchers have now recognised that the debate over paradigms no longer serves a useful purpose even though it may have some value in clarifying the issues (Wynn 2001, Monod 2003, Mingers 2004a, Weber 2004). Young's (2005) research confirms it is more important to determine the research purpose and the research question (Newman et al. 2003, Tashakkori and Teddlie 2003). He found however, the choice of research paradigm was still important because it determined how rigour should be assessed.

The relative strengths and weaknesses of the interpretivist and positivist epistemologies are now relatively well understood, and Young (2005) made the assessment that they were inadequate for his particular research question. He argued that neither the positivist or interpretivist paradigms had helped IS researchers explain how top managers influence IS project success/failure (Sauer 1999), that positivism had a long track record of failure, and there were no reasons to believe interpretive research would be any more successful (Sauer 1999). He found interpretivism favoured description over generalisation (Fitzgerald and Howcroft 1998) and would not overcome the problem of a plurality of definitions of success and failure.

Young (2005) believed the research paradigm had to be able to deal with the values of top managers in particular because the research objective was to try to influence their behaviour. He found critical communicative theory (Varey et al. 2002), critical realism (Mingers 2004b) and pragmatism (Goles and Hirschheim 2000) to be alternative paradigms that had the capacity to handle ethical/value issues. His assessment was that they are all suitable paradigms in the sense that they are oriented toward producing knowledge that will help direct change (or emancipation) but that they were differentiated by their ontologies (conceptions of reality).

Critical communication theory is based on a subjective understanding of reality. Young (2005) argued that top management support had been found to be important across many cultures (Lyytinen et al. 1998) and was therefore unlikely to be a social construction alone. In addition to this, top managers tend to use rational objective justifications for their actions (Shapira 1995) and believed they could influence their world. Young's conclusion was that the paradigm was inconsistent with the top management mindset and not appropriate for his research. He felt that to influence top managers, it was more sensible to use a conception of reality that corresponded to their beliefs.

Critical realism is based on an objective reality and claims to endorse or be compatible with a relatively wide range of research methods (Sayer 2000). It simply uses existing positivist or interpretive approaches to explain empirical phenomena and relies on abduction to propose higher-level mechanisms and structures to explain value judgements and other emergent phenomena (Checkland 1981). However, its methodological weaknesses lead Young (2005) to reject it for his research. He found (1) there are no established ways to validate any proposed structures and (2) no formalised ways to choose between alternative explanations. In addition to this, he found a preconception that critical research is orientated toward emancipation because it follows interpretivism in favouring less empowered voices. Young found it to be unnecessary to add the hurdle of having to convince reviewers that top managers needed emancipation. He also thought it would weaken his objective of influencing top management behaviour because even proponents of critical realism acknowledge that critical realist writing is difficult to read (Sayer 2000).

Young (2005) found pragmatism to be axiologically and methodologically stronger. The overriding issue in pragmatism is whether something is useful or not. The values of the researcher and those shared by their community play a large role in the selection of research topics and the determination of usefulness (ie. the

interpretation of the relevance and importance of results) (Tashakkori and Teddlie 2003). Goles and Hirschheim (2000) make the point that 'useful' should not be understood to be utilitarian because pragmatists infuse 'useful' with value. Pragmatism adopts to the values of the researcher and their community to determine relevance, not just the values of the less empowered. This approach increased the chance the research would achieve its objective of influencing top management behaviour and was therefore the paradigm of choice.

Pragmatism is unlike any of the main scientific paradigms because its objective is not to describe reality as such. Pragmatism is more interested in knowledge and how to use it (as in the professions). Pragmatists accept Hume's finding that causality is impossible to prove and in place of a theory of causality there is a theory of probabilities (Maxcy 2003). It corresponds closely to the ideas of relevance espoused by Lee (1999), Davenport and Markus (1999) and Alter (2003b) where truth is defined not in terms of descriptions of matches between ideas and things but in terms of predictions.

Pragmatic methodology evolved from triangulating information from different data sources. The appropriateness of method is determined pragmatically by whether it achieves its purposes (Tashakkori and Teddlie 2003). An emerging strength of pragmatism is the attention to inference - methods to interpret results, make sense of findings and drawing conclusions. Pragmatic research attempts to increase interpretive rigour by synthesising and transcending qualitative concepts (credibility and transferability) and quantitative concepts (internal validity and external validity). Rigour is determined by both quantitative criteria of external validity (use of theory and replication logic in the research design (Yin 2003)) and qualitative criteria of transferability (thick descriptions of context). In addition to this, pragmatic research quality is assessed by relevance – how the research is likely to be perceived by the key audiences.

Young's (2005) concluded that pragmatism was the paradigm of choice even though there appeared to be no precedents in IS research. Pragmatism had become the paradigm of choice for the many mixed methods researchers that have worked throughout the 20th century (Tashakkori and Teddlie 2003). The research methods are not controversial because they synthesise and follow the same criteria of rigour in the qualitative and quantitative paradigms. Axiology was the main area of difference and it appeared to be a major strength because it guided the choice of the case study methodology and provided a basis to determine rigour.

Case study research

Considerations of length prevent discussion of the outcomes of the research in any detail. In total five case studies were conducted following the pragmatic paradigm. They were unlike most case studies on IT failure because the scope included the period of initiation and extended many years after the actual implementation and because they considered the impact of top management as well as all the other commonly emphasised critical success factors. It is worth noting that the extensive industry collaboration shaped the final format of the case studies. They were written in a style that practitioners would read and also satisfied the academic rigour required of a PhD i.e. enough detail to allow readers to form their own interpretations.

The findings challenge the main emphasis of the common IT prescriptions. It provides strong evidence that top management support is more important than all the other critical success factors. It suggests most effort to date has been misdirected and explains why the success rates have been so inconsistent. The IT governance model developed for the research was compared to rival models and found to offer a superior explanation of how top managers influence projects to succeed (Young 2005).

It is too soon to evaluate whether the research will meet its objective of influencing top management behaviour to improve IT project success rates. There is some reason to be optimistic because the research has been accepted for publishing by Standards Australia. There is further cause for optimism because its completion coincides with a worldwide demand for higher standards of corporate governance and a desire to focus on more than compliance. The results have already been presented at a number of industry conferences and a number of organisations have requested customised workshops to discuss the implications of the findings.

At this stage it is fair to claim the research is better for having subjected itself to the pragmatic test of proving itself in practice. The initial sponsorship by Standards Australia, the planned commercial publishing of results and the reception of the results to date confirm the research has some relevance. If it eventually influences top management behaviour and improves IT project success rates, the research has the potential to become an exemplar of relevant research. Currently \$90-\$120B pa is estimated to be wasted on IT projects that do not deliver any benefits, and at least \$3.2B of this is wasted in Australia (Young 2005).

CONCLUSION

This paper has documented an attempt to develop a more relevant IS research tradition. It describes how the research in IT project failure has lacked relevance for critical stakeholders and overlooked important insights. It has then shown that it has been possible to follow guidelines suggested by Benbasat & Zmud (1999) to overcome these issues. It illustrates the synthesis of concepts to develop frameworks that overcome fragmentation and reduce the burden of understanding. It describes the importance of choosing an appropriate research paradigm to test frameworks. It introduces the pragmatic paradigm and justifies its use on the basis of its ability to deal with different values (axiology) and its guidelines to determine rigour. It concludes by summarising the outcomes of the research, demonstrates that relevance can be achieved without compromising rigour and shows how relevant research has the potential to make a significant contribution to practice as well as theory. It suggests by implication that tests of relevance include strong industry sponsorship and commercial publication.

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