

11-2011

## Pulling the Plug: When to Call It a Day on Research Projects

Thomas O. Meservy

*Management Information Systems, University of Memphis, tmeservy@memphis.edu*

Fred Niederman

*Saint Louis University*

Follow this and additional works at: <https://aisel.aisnet.org/cais>

---

### Recommended Citation

Meservy, Thomas O. and Niederman, Fred (2011) "Pulling the Plug: When to Call It a Day on Research Projects," *Communications of the Association for Information Systems*: Vol. 29 , Article 24.

DOI: 10.17705/1CAIS.02924

Available at: <https://aisel.aisnet.org/cais/vol29/iss1/24>

This material is brought to you by the AIS Journals at AIS Electronic Library (AISeL). It has been accepted for inclusion in Communications of the Association for Information Systems by an authorized administrator of AIS Electronic Library (AISeL). For more information, please contact [elibrary@aisnet.org](mailto:elibrary@aisnet.org).

# Communications of the Association for Information Systems

CAIS 

## Pulling the Plug: When to Call It a Day on Research Projects

Thomas O. Meservy

*Management Information Systems, University of Memphis*

*tmeservy@memphis.edu*

Fred Niederman

*Saint Louis University*

---

### Abstract:

Over the years, the Information Systems discipline has produced advice on how to discover projects and research topics that are worth pursuing. However, little attention has been given to sharing anecdotes and developing heuristics of when to terminate research projects. This article captures the comments of successful IS researchers about when they have pulled the plug on research projects and some of the heuristics they look for when making that decision.

**Keywords:** IS scholarship, doctoral student tutorial

Volume 29, Article 24, pp. 427-440, November 2011

### I. INTRODUCTION

Over the years, we have frequently given consideration to issues in discovering projects and topics worth pursuing. However, in recent sessions with doctoral students, we have found a recurring and related question—how do you know when an MIS research project should be terminated as unlikely to yield sufficient return for the remaining effort?

It can be argued that the domain addressed by MIS has been marked by continual expansion. To a large degree initial scholars were concerned with the function of information systems within business organizations [Davis et al., 2010]. This is consistent with the emergence of MIS as a field of study largely within business schools and, to some extent, as the computerization of previously manual operations such as cost accounting and human resource tracking. However, over time, the domain of research in this field has extended past organizational artifacts and management of computing resources. Electronic commerce has served to push the field strongly to consideration of the nature of customers and the Internet experience [Chatterjee et al., 2002; Gefen et al., 2003; Pavlou and Fygenson, 2006]. Many studies in e-commerce (broadly defined) focus on the Information System as product—as a structure for auctions [Bapna et al., 2004] or social networking [Wasko and Faraj, 2005] and are not necessarily tied to considerations of how organizations providing products and services organize their ability to provide e-commerce. Information economics studies have moved in large measure to the effects of information and technology on markets and the economy in general [Ba and Pavlou, 2002]. Questions such as what are the effects of investments in IT on firm profitability or the effects of IT related announcements on stock prices also move the domain beyond narrowly focusing on the application of Information Systems within organizations [Hitt and Brynjolfsson, 1996; Im et al., 2001; Zhu and Kraemer, 2003]. We have also seen studies on the effects of “too much” Internet use or game playing on society [Gentile et al., 2011; Ha et al., 2007]; the nature of organizational or industry “ecosystems” [Tan et al., 2009], and the penetration into the technology market of FLOSS (Free Libre Open Source Systems) business models [Crowston et al., 2005; Fitzgerald, 2006].

Our purpose in this article is not to judge whether such continual expansion of the MIS domain is “good” or “bad,” though such a discussion would continue a long tradition of debate about the nature of MIS, but rather to point out that today’s junior faculty and doctoral students face an increasingly complex and extensive array of topics for their consideration. One might argue that this amounts to a kind of information overload. The array of topics includes those that have been very thoroughly examined, such as diffusion of technology and factors influencing use and success [DeLone and McLean, 2003; Venkatesh et al., 2003], and those that are emerging, such as crowdsourcing [Agerfalk and Fitzgerald, 2008] and green/sustainable IT [Watson et al., 2010].

While the nature of the topic might be expected to play a significant role in both choices of study to pursue and whether to continue, we were not convinced that this alone is sufficient to guide decisions for both taking up and abandoning projects. We observe that the involvement in projects has some elements of rational decision making, satisficing, and incremental decision making. Of course the rational involves scanning the entire range of possibilities to make optimal selections and satisficing involves searching until an acceptable option is found. We would argue that once begun, the decision to continue (if made intentionally) or discontinue a project will tend to resemble the incremental decision-making model. With any project, some is known and much is unknown at the beginning. With each step more information becomes available. Where such information about the project continues in an expected manner, a decision point regarding the project may not be reached. When such information becomes “disturbing” in one way or another, an assessment of possibly curtailing the project becomes likely, if not necessary. With the nature of the current job market as well as the continued expansion of the MIS field, there is increasing pressure on doctoral students and junior faculty to make good decisions in project selection but also to make good decisions in “letting go” of projects that will not yield sufficient results to justify continued investment.

In that spirit, primarily for the benefit of doctoral students and those relatively new to the field, we moved forward to query a wide array of senior and mid-experience level MIS faculty regarding their experiences with terminating projects. We discuss below the responses we received from these participants. We hope that the decision rules, examples, anecdotes, and other ideas might be helpful in guiding project management decision-making throughout the research lifecycle.

## II. BACKGROUND

The process of scientific research has been studied across a number of different fields. In this research, we introduce a simplified illustration of the research lifecycle which consists of several different steps that can be divided into three main stages: research inception, research maturation, and research validation. We illustrate basic activities in each of these phases in Figure 1.

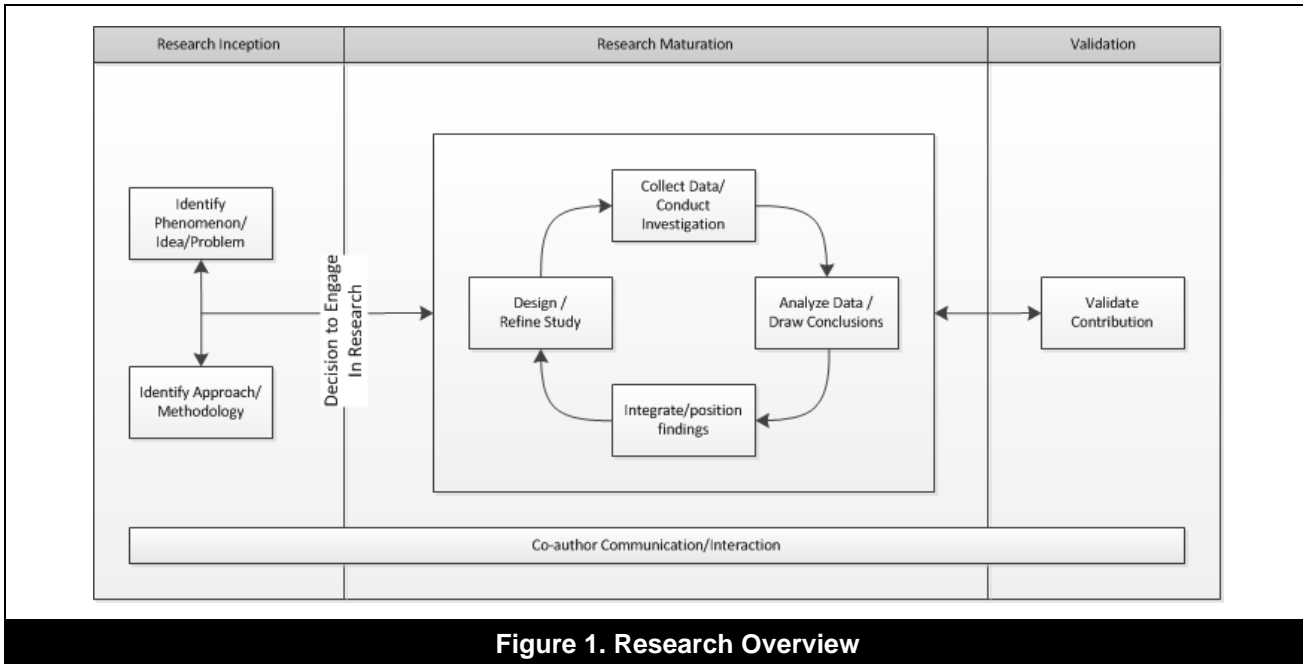


Figure 1. Research Overview

Basic steps in the *inception phase* include the identification of the phenomenon, idea, and/or problem and also identification of a general research approach or methodology. Existing literature, conferences, interaction with industry, discussion with colleagues, past research projects and findings, and the popular press, among others, are productive sources for new ideas. Different research approaches/methodologies may be better suited to describe/explain/investigate the area of research interest. A multi-methodological approach and triangulating data using multiple approaches provides a better understanding of the research interest. Popular approaches/methodologies that have been employed in Information Systems research include case studies, action research, survey research, design science, experiments, field studies, etc.

At some point researchers decide to engage in the project. We suggest that this decision often happens after ideas have been generated and the research approach selected. Following the decision to engage in the research, research cycles iteratively through a number of steps where the abstract ideas become more concrete and evidence is collected to support the research objectives. While the specific steps of the research lifecycle during the *maturation phase* vary, we suggest that most studies include four basic steps: (1) designing/refining the study, (2) collecting data/conducting the investigation, (3) analyzing the data that was collected and drawing conclusions, and (4) integrating/positioning the findings. We view these steps as iterative in nature, not only between the steps but also within each step; there may be many iterations required to refine/mature the research product. However, we also suggest that, although we have illustrated the steps in a sequential nature, they can transition between any of the steps at any time and activities can, and often are, carried out simultaneously.

Once the research has been conducted and findings have been captured in an artifact (usually in the form of a manuscript), the results are usually validated at some level. In the Information Systems discipline, research contributions are often validated through a blinded, peer-review process. It is important to recognize that this validation process, perhaps all too familiar, means a return to the maturation phase of the research.

The decision to participate in the research process is not a single static occurrence, rather it is evaluated multiple times throughout the research lifecycle. Additionally, research often consists of the combined collaborative effort of multiple researchers. In many projects, researchers may conceive of a research project together and follow that through to the publication and dissemination of results. However, researchers may engage or disengage a project for multiple reasons and this can occur at different phases and in different activities of the lifecycle. For example, a research team may find that additional expertise is needed to apply an appropriate data analysis approach to carry the project forward or the development of a sophisticated experimental instrument requires software development

expertise to construct or employ. Throughout the lifecycle, researchers/co-authors coordinate and share information which can be critical to a project's success.

In this project, we are primarily interested in the decision to “pull the plug” on research projects after there has been a decision to be engaged, that is, during the maturation or validation phases.

### III. PERSPECTIVES OF EXPERIENCED RESEARCHERS

In order to gather a broad array of opinions and experience, we solicited input from both senior IS scholars and also from a group of IS researchers who recently received tenure. In all, fifty-eight individuals were invited to participate via e-mail, and ultimately twenty-one actually did so.

Participants were directed to a survey where they were introduced to the topic and then asked to respond to three short, open-ended questions:

- Please describe a situation where you had to “pull the plug” on a research project.
- Are there any key indicators you look for when you “pull the plug” on a project?
- Please list any other broad thoughts about what you look for in deciding whether to continue or discontinue a troubled project?

Researchers employed a two-phase, qualitative approach based on the principles of grounded theory in order to draw out similarities between the open ended comments. See the Appendix for an overview of the qualitative approach employed.

In this section, we share the experiences and advice about when to “pull the plug” on research projects from scholars in the discipline. The thematic categories identified can be aligned with the phases of the research lifecycle that was previously introduced but could have alternatively been grouped by categories that have been studied related to IT project failure: project factors, psychological factors, social factors, and organizational factors [Keil, 1995]. Within each section, we also share key indicators that scholars looked for when deciding whether or not to pull the plug on projects.

#### Design/Refinement of Study

Early indicators of success of a research project are tied to identifying a compelling motivation for the study, assessing “the likelihood that the project will generate new and interesting results,” and designing a study that meets the intended objectives of the research.

*In the early stages (design, instrument development ...), I look for consistency with the purposes of the project. If it is clear that as you operationalize the project that it will not meet the purposes intended, then the project is stopped or “re-oriented.”*

We suggest that during the early stages of research “a convincing punchline to the study” should emerge. Similarly, the implications/impact of the research on both research and practice should be evident and compelling. Early in the project an answer to the “so what” question should emerge and face validity should be apparent.

*If, despite working on a project, a clear intervention or response to the “so what” question does not emerge, it is difficult to get intrinsically motivated given other projects. This is particularly true for projects at early stages where sunk costs are low.*

As a design proceeds and researchers with varying perspectives are involved, often there is compromise regarding the direction of the project and what steps should be pursued and how they will be enacted. Compromises between desires of different researchers can iteratively distance the project from meaningful contribution as suggested in the following:

*Students and I in a research team had trouble agreeing. There was so much disagreement that we designed the study and did a pilot, but there were so many compromises that the study lost meaning.*

If the research doesn't fit the mold of expected research there is a risk of acceptance by those who will later validate the contribution. One researcher conveyed his or her experience with almost pulling a plug on a project due to a unique research methodology:

*I have not had to pull the plug on a research project, but there was an instance I was close to doing so. The project was undertaken by an indigenous student exploring the narratives of successful indigenous women*





*in IT. Interviews were conducted and was framed around an indigenous research methodology as well as indigenous theories. The student's indigenous world view of scholarship was not congruent to what "western" scholarship expects. Unfortunately, there were already examples of completed Ph.D. theses by indigenous students who have taken similar approaches.*

Of course, as research studies are designed, they have to be positioned relative to the existing literature. Even if you have appropriately positioned the research study relative to the existing literature and have created a design that will address the phenomenon of interest, at times we discover similar papers that are already in print even prior to collecting data. One suggestion is to consider pulling the plug on projects if "half way through the project—before data collection—a similar paper is seen in print." However, often these projects can be reoriented or reframed.

Table 1 contains indicators related to the design/refinement stage regarding whether or not a project should be continued.

Table 1: Continuance Heuristics for Design/Refinement Stage Analysis/Conclusions
Is there theoretical promise?
What is the importance of the study—will it be helpful?
Will others see it as a significant contribution? Does it make some progress in a needed area?
Is there a better paper with the same message already published?

The following are some precautionary steps to minimize the probability and impact of problems in this stage happening to you:

- Before starting the project evaluate whether it has the potential to meet your publication goals.
- Write a short research summary defining the importance of the study, potential contributions, and how the research is positioned relative to existing theories.
- Early in the project, share/present these ideas to more experienced researchers to "test the waters" with your ideas and the salience of the potential contributions

### Data Collection/Investigation

The success of many research projects depends on the collection of data that will be analyzed in support of the research objectives. Depending on the research project, data may be generated, acquired from secondary sources, or collected from primary sources. Identification of viable sources of data is a crucial component that can influence the continuance of research projects. One researcher noted that, "when it became evident that it [was not] possible to find the data sources needed," the project was abandoned. Another researcher suggested that even after viable sources are identified that failure of subject participation "was the most common reason" for pulling the plug on research projects.

Information Systems research often requires collection of data from sources external to the researchers' institutions. Establishing these relationships can be time consuming, and turnover of personnel in these external organizations may impact the commitment of the organization to participate in the research. Further, organizations might impose additional constraints on the data collection process which may jeopardize the study. One researcher describes an experience with a multi-site case study:

*The research project was a case study about "successful end users" and their characteristics. This was a three organization case study involving over 120 end users. All three organizations had agreed to the conditions under which they would participate. Data gathering had started in all three organizations. One of the organizations asked to change the conditions under which the data was gathered which would make the data not comparable to the other organizations in the study and therefore unuseable. This substantially weakened the study. It was decided to "pull the plug."*

Changes to company procedures that impact data collection don't always indicate that the data source or the project should be abandoned. However, according to the following researcher, clever approaches may need to be employed:

*Getting data is often a key issue. If the project requires data, then the critical issue is to obtain it. Sometimes, it can be done in clever ways; in other cases, it cannot be done, so terminate the project. In my career, I had a project that looked like it would fail because the company that had agreed to a procedure to collect data changed their mind because they feared a bad effect on customers receiving the request. However, a clever suggestion by my coresearcher overcame the objection and the field experiment worked*

well. Using his procedure, we were able to collect data, and the resulting publication had a significant effect on professional practice.

Even after data collection is complete the sponsoring organization may change policies, procedures, or simply withdraw support of the project. Another researcher describes:

*I have had several situations where sponsoring organizations have backed out, even after the research was complete. One recent one was case research on [a large oil company]'s internal Wiki. A change in management, an organizational reshuffle, and suddenly our sponsor was no longer in a position to sponsor the project.*

Table 2 contains indicators related to the data collection stage for whether or not a project should be continued.

**Table 2: Continuance Heuristics for Data Collection Analysis/Conclusions**

General lack of research support during the data collection stage
Poor participation from organizations or other participants
A promised organizational sponsor backs out

Precautionary steps to minimize the probability and impact of problems in the Data Collection/Investigation stage happening to you include:

- Consider multiple contexts/sources for data collection.
- Identify direct benefits of the research for the sponsoring organizations and/or participants.
- Define minimum and ideal sets of data needed to address the original research questions; be open to collecting serendipitous data that enriches, extends, or modifies your research process.
- Be flexible to the constraints/interests of sponsoring organizations and/or participants, but act to the extent possible to gather the data needed to fulfill your original research intentions.

### Data Analysis/Conclusions

At times the decision to pull the plug does not occur until the project has progressed to the stage of analyzing data and drawing conclusions from the data. In quantitative studies it is not until this stage that you discover support, or lack thereof, for the hypothesized relationships.

*As part of our introductory Ph.D. course, the class does a research project together. We did a lab experiment one year and found no significant differences between the two treatments. We tried adding in various control factors, recoding the data to test a different dependent variable and everything we could think of, but there were no differences. We pulled the plug because there was nothing to report.*

Similarly, in other research projects it may not be until after considerable efforts has been expended that it is discovered that the results are not interesting or compelling enough.

*Recently, I had worked on a paper with a doctoral student that involved a meta-analysis of an important literature stream. The project was labor intensive and involved over a year of data collection (categorization of research articles on a template of constructs). Unfortunately, the data analysis did not result in compelling results and after extensively analyzing the data and going through iterations of the paper, we were unable to make a compelling story. We then decided to position the paper for a lesser journal, but the momentum had been lost. All involved parties got busy, and the paper was sidelined and (perhaps) is at the stage where it is shelved.*

When research results are not compelling or when they are inconclusive researchers often consider additional explanations, hypotheses, or try to collect additional data. However, even these efforts may not produce sufficient results and may lead to the same conclusion.

*Results were inconclusive, impossible to write it up in an interesting way. We did an experiment on Group Decision Support back in the early 90s and manipulated anonymity (anonymous versus non-anonymous), constructive vs. criticizing task, and peers vs. faculty and peers, and found zero results. We counted suggestions, counted number of times certain words showed up, counted nasty comments, examined the creativity of the suggestions by a panel of judges. Nothing. No differences. We gave up.*

Table 3 contains indicators related to the data analysis/conclusions stage for whether or not a project should be continued.

**Table 3: Continuance Heuristics for Data Analysis/Conclusions**

No data are available as expected  
No significant results  
No significant results that can be explained theoretically  
Results are weak and the preconceived theory is not supported.  
Empirical results—meaningful? Significant?

Precautionary steps to minimize the probability and impact of problems in this stage happening to you include:

- Consider additional explanations and hypotheses.
- Have a strong narrative whether the findings support or refute your expectations.
- Be willing to collect additional data.

### Integrating Research and Results Relative to Previous Work

The activities related to positioning research relative to previous work can occur at the beginning and near the end of the research lifecycle. Understanding the literature related to the phenomenon of interest probably occurs near the beginning. Returning to the literature near the end of the research may be needed in order to further explain unexpected significant results (or direction of those results) or salient concepts that might arise during the research process. Either way, a “strong and new intellectual contribution” relative to existing knowledge must be clear.

*If I do not see a powerful story that can contribute to knowledge, it is difficult to get behind the project. It then becomes more “game playing,” rather than a driven initiative that I want to see succeed. In such cases, where the contribution is marginal at best, my co-authors need to be highly motivated to move it forward. I will contribute and push the project more to see them succeed (particularly if they are doctoral students) than my innate desire.*

During the course of working on research projects, core components that research builds on may fall out of favor or be questioned. These exogenous events may directly impact the likelihood of yielding benefit. One researcher shared:

*I was working on a project testing Nolan’s “Stages of DP Growth” when it fell into disfavor. I decided to abandon my efforts.*

### Research Validation

Research artifacts in the form of articles or manuscripts are typically validated by reviewers and editors prior to publication. Reviewers can play a significant role in the continued formation of the paper, and often reviewers are able to identify deficiencies in the manuscript. While many issues can be addressed, at times reviewers identify research flaws that require a substantial amount of effort to correct or that invalidate the research. Certainly finding flaws late in the research cycle can be costly and if reported by multiple reviewers and multiple journals may indicate fatal flaws in the research.

*If the comments keep coming back with the same problems even when you try to fix them, then you are better off moving on. And you start to realize the reviewers are probably right and there is an unfixable error. Especially when you have sent the paper to an A and then several B journals.*

Another researcher shared a similar experience and conclusion:

*Reviewer comments in a paper from multiple journals even after attempting to fix the problems kept on being identical. Thus, [there was] no other conclusion than the research was flawed. Put its publication on indefinite hold.*

Many research papers go through the review cycle multiple times and the feedback from reviewers are not always indicative that the project should be abandoned as explained in the following:

*I’ve been fortunate in that I’ve seldom had to pull the plug. And I’ve almost always had the research published somewhere, but not always in the journal that was initially targeted.*

*Surprisingly, I can’t think of one case where I actually pulled the plug. Rather I let the project run its course and try to do what I have promised to deliver. Obviously, the less promising the publication outlet, the less I commit to deliver. But I let the others on the project completely disengage before I determine a project dead.*





However, the demands of reviewers might require a substantial amount of rework or extra research and time constraints might play a role in deciding whether or not to continue the research.

*I had conceptualized a stream of research on "Productivity in Knowledge Work." Some students and I wrote a working paper and submitted it for publication at a first rate journal. The paper was rejected. I decided to not pursue the project and revise the paper because the concepts needed some research in order to support them. I decided I could not devote enough time to build the body of knowledge to populate the article with good examples.*

Table 4 contains indicators related to the research validation phase for whether or not a project should be continued.

**Table 4: Continuance Heuristics for Research Validation Phase**

Paper gets rejected twice due to the same serious issue that is not easily correctable. The paper has been rejected so many times that it is clear that it is too problematic to publish in a good journal. The underlying theory/framework is invalidated There is no prospect for publication.
---

Precautionary steps to minimize the probability and impact of problems in this stage happening to you include:

- Prior to submission, share/present manuscript ideas to more experienced researchers for frank evaluation of manuscript potential
- Become familiar with theoretical developments and current issues/perspectives related to theory and research and analytical approaches related to your manuscript
- Be willing to spend appropriate effort correcting issues identified by reviewers

### Co-Author Communication/Contributions

Most research in the Information Systems discipline is conducted by multiple researchers. As suggested previously, researchers may engage in a project at different stages of the research lifecycle. Throughout the lifecycle, researchers/co-authors, coordinate and share information which can be critical to a project's success. We suggest that co-author communications and contributions are the most frequently cited category of why researchers pull the plug on research projects. One participant explicitly called out the fact that "in most cases when [he or she hadn't] completed a project it [was] due to a co-author not doing his or her part."

An underlying theme of several of the comments received in this category the idea that co-authors would not "carry their weight." If this can be detected early on, time can be reallocated appropriately. Heuristics or rules of engagement can help guide the effort of whom to work with. One simple and straightforward heuristic that one of the authors developed and employed is that he will do a research project with anyone "once," but will not conduct additional research with co-authors that do not pull their share. Setting expectations between co-authors and taking time to vet potential collaborators may help prevent conflicts.

*I try to vet co-researchers before doing anything, but sometimes the group outvotes the individual and allows a nonparticipant in the group. Then, I have to decide how important the topic/pub(s) are to me. Often I'm the main intellectual contributor and that's ok, but I need to know the work will be shared. Sadly, from history, you don't know until you start the work. I now do not work with "big names" or anyone I've been burned by in the past. I tend to get to know people better first, then see if research is possible. It doesn't completely protect me but it's better than nothing.*

Circumstances play a role in co-author communication and contribution. Multiple scholars cited that projects were abandoned when "students lost interest," "moved away," "dropped out," "lacked effort," or "stopped working" with the researcher or when "key collaborators abandoned the project." The data suggest that when expectations about co-authors are repeatedly violated, there is a higher likelihood that the project will be abandoned.

*Co-authors were nonparticipants, did sub-par writing, did not make meetings, did not comment on anything when requested, were not engaged in most meetings they did make, and then only to get their own way, but without follow-up. In short, everyone wanted the research, they also all wanted me to do all of the work.*

Compatibility of personality and styles can also impact a research project or a planned research stream. There may be a tradeoff between productivity and happiness as the following suggests:



*Compatibility as people—post-tenure I'll only work with people that I enjoy. If I wouldn't have someone over for dinner, I would not collaborate with them. Sometimes, you only come to understand this over time. As a result, you only write one paper with them ... then cut your losses on the planned research stream. This philosophy has not helped my productivity. It has helped my happiness.*

While compatibility among people may play a role in selecting who to work with on research project, it may also lead you to abandon projects. One researcher suggested that sometimes projects may need to be abandoned if it affects or ruins friendships.

Co-author expectations shift based on perceived contribution throughout the lifecycle. When commitments are not met and the time commitment increases, co-author expectations may become misaligned. A potential challenge is that co-authors view the same incidents through different lenses. However, we suggest that the general mismatch of expectations by co-authors can lead to cancellation of projects.

*I pulled the plug on a project when a co-author could not accommodate my schedule. The project had become a time sink ... with little prospect of yielding a positive long term outcome ... when I asked the first-author to help work on the implications ... the author indicated that they'd done their part and felt like they no longer needed to look at the project.*

Most of the comments in this section fit into one of three categories: communication, contribution, and commitment. Most of the comments in the communication and contribution subcategory relate to missed expectations either during the process (communication) or related to the product (contribution).

Table 5 contains indicators related to co-author communications/contribution for whether or not a project should be continued.

<b>Table 5: Continuance Heuristics for Co-Author Communication/Contribution</b>	
Commitment	Declining interest in the project Significant knowledge barriers that requires a stretch of time Lack of commitment Loss of momentum due to busy schedule at home institutions
Communication	Irreconcilable differences between co-authors (e.g., students refusing advice) Committing and not delivering Failure to meet deadlines Frequent and unreasonable demands Low frequency of updates on project status
Contribution	Low quality of deliverables Primary author of project does not push it forward Inability to work on the conceptual part of the project (numbers are easy) Lack of contribution by co-author (e.g., commensurate with a peer reviewer, audience comments at a conference) Lack of effort

Precautionary steps to minimize the probability and impact of problems related to co-author communication/contribution include:

- Negotiate and specify commitments regarding:
  - Who communicates with the journal and makes final decisions regarding where and when to submit
  - Expectations related to different components of the paper, such as lit review, data collection, etc.
  - Order of authorship listing
  - Minimum contribution needed to remain an author of the paper should one of the authors need to pull out
  - General research procedures (e.g., having a “hot copy” that moves from author to author adding content versus each author preparing segments that someone integrates)
- Evaluate project progress periodically, particularly at key points (e.g., agreement with business host, completion of data collection, completion of analysis, return of submission feedback whether the project is on track); renegotiate as needed.

## IV. DISCUSSION AND IMPLICATIONS

For many researchers the evaluation process of pulling the plug on a research project is triggered by a negative event occurring at a specific point in the research lifecycle. These events, which may be commissive or ommissive, trigger the evaluation process by exceeding a reasonable threshold specified by the researcher. These thresholds provide markers of our expectation that can help determine if resources should be refocused. Some researchers continually assess the possible outcomes of a research project and use that data during the evaluation process.

*I look at changes from the original intent and anticipated research effort. I don't like to "quit" on any project, but I have a "threshold" (an internal sense) that if a project simply becomes to "difficult" to execute relative to the anticipated potential benefits, then I discontinue the project. I continually assess "best-case," "worst-case" and "most-likely" scenarios as I work through a research project. If the most-likely case becomes the worst-case, then the project is stopped or "re-focused."*

Perhaps from the results and discussion thus far, some readers might assume that pulling the project is an explicit decision to abandon the project. However, pulling the plug often will occur through a natural evolution of the allocation of time based on the likelihood of payoff. Often there is not an overt decision to pull the plug, rather, projects with the highest potential get priority over those with less potential.

*There are enough journal outlets that anything that I was serious enough to start can find a home. So, I don't pull the plug, other projects crowd out the least promising ones and I just dedicate less and less time to them. Yes, I have numerous projects that are sitting unfinished that I probably will never get back to because other more promising ones have captured my attention, however, I have successfully published papers that I have left dormant for several years. Given acceptance rates at top journals, you have to maintain a deep portfolio of projects, and expect rejections, rather than kill projects. I take a portfolio approach and just let the more promising ones capture my attention. Deliver on what you promise colleagues and just keep moving the portfolio along.*

Further, some readers might infer that pulling the plug is a negative thing. We suggest that identifying challenges earlier in the research cycle will save time and resources to pursue higher impact/more promising research. One researcher's perspective is that too many projects move too quickly through the early stages of research; that is, they don't spend enough time on the design and feasibility of the research.

*Based on lots of papers I have reviewed, far too many projects move forward that never should have. There does not, typically, seem to be enough time spent in design and feasibility assessment, but rather we, to use an analogy, start coding "willy nilly."*

We suggest, as others do, that many research projects would benefit by applying a little bit more structure throughout the process. Perhaps more research projects should incorporate some of the basic elements and principles common in the domain of project management.

*A research project, such as a dissertation research project, should have a project plan that provides the motivation, assumptions, short review of literature, and description of the major activities, review points, estimated times and review points, and expected outcomes. If a researcher cannot or does not have such a project plan, the probability of success is low. I have seen brilliant students who have great ideas but do not or cannot turn them into a research plan with deliverables, review points, and a timeline.*

Success in research can come at different levels, but as the following suggests, is more likely when collaborators on a research project work hard and are dedicated to the work.

*Most of the projects where everyone is working hard eventually turns into a success (at some level). It is really the dedication and hard work that must be evident to indicate whether a project will eventually be completed successfully or whether it will be a waste of time. There can always be some false starts that get better directed as new information is learned. Without effort, however, the project is doomed.*

We conclude with one scholar's direct advice related to research projects and suggest that the decision to pull the plug occur earlier rather than later in the research lifecycle.

*Don't waste your efforts in trying to publish marginal or bad research. There is already too much marginal/bad research being published.*

## V. CONCLUSION

We suggest that a more conscientious effort should be made to evaluate the progress of research throughout the research lifecycle. While we are not suggesting that research projects should be abandoned whenever obstacles or

difficulties are encountered, we do believe that there are times when terminating a project is for the best. We acknowledge, from our own experience, how difficult it can be to determine if one is experiencing irrational commitment or just needs one more round of editing to get “over the hump.” Again, from personal experience, much can be learned from a project that is never completed both from a process and content perspective that can and should be applied in future studies (as well as in amusing anecdotes for classroom presentation). By sharing experiences of when scholars in the IS field have pulled the plug on projects we hope other researchers will find this helpful in performing their own evaluation when faced with similar decisions in their own research streams. We also suggest that throughout the project researchers use the heuristics/key indicators provided to reallocate time and resources sooner than later.

In conclusion, it is also important to keep in mind that an extraordinary number of projects are in fact completed. We are very pleased that this study is one of them. We note also that not every participating researcher has “pulled the plug” on a project as evidenced by the following comment:

*Apologies, but I haven't had this experience. Usually what happens is that I have permitted adjustments to the research process.*

Whether or not you personally have pulled the plug on a research project—yet, we hope that this broader array of decision rules, examples, anecdotes, and other ideas will be helpful in guiding project management decision-making throughout the research lifecycle.

## ACKNOWLEDGMENTS

We would like to thank Gordon B. Davis, Dennis Galletta, Varun Grover, Blake Ives, Hugh Watson, and our many other colleagues who contributed specific scenarios and heuristics related to this project.

## REFERENCES

- Agerfalk, P.J. and B. Fitzgerald (2008) “Outsourcing to an Unknown Workforce: Exploring Opensourcing as a Global Sourcing Strategy”, *MIS Quarterly* (32)2, pp. 385–409.
- Ba, S. and P.A. Pavlou (2002) “Evidence of the Effect of Trust Building Technology in Electronic Markets: Price Premiums and Buyer Behavior”, *MIS Quarterly* (26)3, pp. 243–268.
- Bapna, R., et al. (2004) “User Heterogeneity and Its Impact on Electronic Auction Market Design: An Empirical Exploration”, *MIS Quarterly* (28)1, pp. 21–43.
- Chatterjee, D., R. Grewal, and V. Sambamurthy (2002) “Shaping Up for E-Commerce: Institutional Enablers of the Organizational Assimilation of Web Technologies”, *MIS Quarterly* (26)2, pp. 65–89.
- Crowston, K., et al. (2005) “Effective Work Practices for FLOSS Development: A Model and Propositions” in *HICSS'05, Proceedings of the 38th Annual Hawaii International Conference on System Sciences*, p. 197a.
- Davis, G.B., et al. (2010) “Ideas for the Future of the IS Field”, *ACM Transactions on Management Information Systems (TMIS)* (1)1, p. 2.
- DeLone, W.H. and E.R. McLean (2003) “The DeLone and McLean Model of Information Systems Success: A Ten-Year Update”, *Journal of Management Information Systems* (19)4, pp. 9–30.
- Fitzgerald, B. (2006) “The Transformation of Open Source Software”, *MIS Quarterly* (30)3, pp. 587–598.
- Gefen, D., E. Karahanna, and D.W. Straub (2003) “Trust and TAM in Online Shopping: An Integrated Model”, *MIS Quarterly* (27)1, pp. 51–90.
- Gentile, D.A., et al. (2011) “Pathological Video Game Use Among Youths: A Two-Year Longitudinal Study”, *Pediatrics* (127)2, pp. 319–329.
- Ha, J.H., et al. (2007) “Depression and Internet Addiction in Adolescents”, *Psychopathology* (40)6, pp. 424–430.
- Hitt, L.M. and E. Brynjolfsson (1996) “Three Different Measures of Productivity, Business Profitability, and Consumer Surplus”, *MIS Quarterly* (20)2, pp. 121–142.
- Im, K.S., K.E. Dow, and V. Grover (2001) “Research Report: A Reexamination of IT Investment and the Market Value of the Firm—An Event Study Methodology”, *Information Systems Research* (12)1, pp. 103–117.
- Keil, M. (1995) “Pulling the Plug: Software Project Management and the Problem of Project Escalation”, *MIS Quarterly* (19)4, pp. 421–447.



Pavlou, P.A. and M. Fygenson (2006) "Understanding and Predicting Electronic Commerce Adoption: An Extension of the Theory of Planned Behavior", *MIS Quarterly* (30)1, pp. 115–143.

Tan, B., et al. (2009) "Leveraging Digital Business Ecosystems for Enterprise Agility: The Tri-Logic Development Strategy of Alibaba.com", *ICIS 2009 Proceedings*, p. 171.

Venkatesh, V., et al. (2003) "User Acceptance of Information Technology: Toward a Unified View", *MIS Quarterly* (27)3, pp. 425–478.

Wasko, M.M. and S. Faraj (2005) "Why Should I Share? Examining Social Capital and Knowledge Contribution in Electronic Networks of Practice", *MIS Quarterly* (29)1, pp. 35–57.

Watson, R.T., M.C. Boudreau, and A.J.W. Chen (2010) "Information Systems and Environmentally Sustainable Development: Energy Informatics and New Directions for the IS Community", *MIS Quarterly* (34)1, pp. 23–38.

Zhu, K. and K.L. Kraemer (2003) "E-Commerce Metrics for Net-Enhanced Organizations: Assessing the Value of E-Commerce to Firm Performance in the Manufacturing Sector", *Information Systems Research* (13)3, pp. 275–295.

## APPENDIX A

Researchers employed a two-phase, qualitative approach based on the principles of grounded theory in order to draw out similarities between the open ended comments. First, both researchers free- or open-coded the responses. Initially one researcher coded the statements, splitting apart compound statements into multiple atomic units before providing labels for the statements. The second researcher then free-coded these statements at the same level. The second researcher did not find any additional statements that needed further decomposition. In the second phase, researchers grouped their individual codes into higher-level categories which are listed in Table A-1. Although researchers came up with different labels for the categories, the categories were semantically similar.

Researcher 1 Label	Researcher 2 Label	Description
Flawed Research	Early Steps Flawed	The research is flawed; often during the early steps.
Lack of research support	External Market	Support from external sources for the research; often during data collection
Author Time/Interest	Future Time	The author time/interest diminishes; the future commitment of time exceeds the perceived benefits/impact of the research.
Co-author Contribution	Participation	Participation in and contribution to the research project
Results	Results	Results are not significant or compelling.

Table A-2 lists the number of ideas categorized by each of the researchers. Researcher 1's categories are listed in columns and Researcher 2's categories are listed in rows. Researchers had a high level of agreement between raters as evidenced by the inter-rater reliability coefficients listed in Table A-3.

		Researcher 1						
		Time/Interest	Co-Author	Flawed	Research Support	Results	Total	
Researcher 2	Time/Interest	4	0	0	0	0	4	[8%]
	Co-Author	1	25	1	1	0	28	[56%]
	Flawed	0	0	5	0	0	5	[10%]
	Research	0	0	1	4	0	5	[10%]
	Results	0	1	1	0	6	8	[16%]
	Total	5	26	8	5	6	50	[100%]
		[10%]	[52%]	[16%]	[10%]	[12%]	[100%]	

Method	Value	Std Error	95% C.I.
AC1	0.856554	0.055374	0.745 to 0.968
KAPPA	0.816962	0.069096	0.678 to 0.956
PI	0.816458	0.069584	0.677 to 0.956
BP	0.85	0.057446	0.735 to 0.965



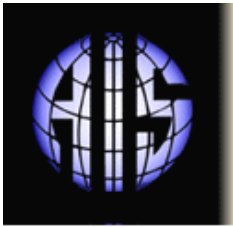
## ABOUT THE AUTHORS

**Thomas O. Meservy** is an Assistant Professor of Management Information Systems at the University of Memphis. He graduated with his Ph.D. in Management from the University of Arizona in 2007. Tom graduated from Brigham Young University with a BS in Management and a Masters of Information Systems Management. His research interests include software development tools and methodologies, collaboration, and automated understanding of human nonverbal behavior. His work has been published in various journals such as *Data Base*, *IEEE Computer*, *IEEE Intelligent Systems*, *IEEE Transactions on Intelligent Transportation Systems*, *Group Decision and Negotiation*, and numerous proceedings of major IS conferences such as *ICIS*, *HICSS*, and *AMCIS*. Much of his research has been funded. He also holds a number of professional technical certifications.

**Fred Niederman** serves as the Shaughnessy Endowed Professor of MIS at Saint Louis University. His Ph.D. and MBA are from the University of Minnesota in 1990. His research interests include global information management, MIS personnel, and using MIS to support teams and groups. Recently he has been investigating the integration of MIS functions after corporate mergers and acquisitions. He is a proponent of grounded theory and theory building as a way to enrich the MIS discipline and build intellectual content customized specifically to our field of practice. He serves on editorial boards for *Journal of the Association for Information Systems*, *Communications of the Association for Information Systems*, *Human Resource Management*, *Journal of International Management*, and the *Journal of Global Information Management*. He recently served as co-program chair for the 2010 ICIS conference in St. Louis, Missouri and is an active member in the MIS "senior scholars."



Copyright © 2011 by the Association for Information Systems. Permission to make digital or hard copies of all or part of this work for personal or classroom use is granted without fee provided that copies are not made or distributed for profit or commercial advantage and that copies bear this notice and full citation on the first page. Copyright for components of this work owned by others than the Association for Information Systems must be honored. Abstracting with credit is permitted. To copy otherwise, to republish, to post on servers, or to redistribute to lists requires prior specific permission and/or fee. Request permission to publish from: AIS Administrative Office, P.O. Box 2712 Atlanta, GA, 30301-2712, Attn: Reprints; or via e-mail from [ais@aisnet.org](mailto:ais@aisnet.org).



# Communications of the Association for Information Systems

ISSN: 1529-3181

**EDITOR-IN-CHIEF**  
Ilze Zigurs  
University of Nebraska at Omaha

## AIS PUBLICATIONS COMMITTEE

Kalle Lyytinen Vice President Publications Case Western Reserve University	Ilze Zigurs Editor, CAIS University of Nebraska at Omaha	Shirley Gregor Editor, JAIS The Australian National University
Robert Zmud AIS Region 1 Representative University of Oklahoma	Phillip Ein-Dor AIS Region 2 Representative Tel-Aviv University	Bernard Tan AIS Region 3 Representative National University of Singapore

## CAIS ADVISORY BOARD

Gordon Davis University of Minnesota	Ken Kraemer University of California at Irvine	M. Lynne Markus Bentley University	Richard Mason Southern Methodist University
Jay Nunamaker University of Arizona	Henk Sol University of Groningen	Ralph Sprague University of Hawaii	Hugh J. Watson University of Georgia

## CAIS SENIOR EDITORS

Steve Alter University of San Francisco	Jane Fedorowicz Bentley University	Jerry Luftman Stevens Institute of Technology
--	---------------------------------------	--

## CAIS EDITORIAL BOARD

Monica Adya Marquette University	Michel Avital University of Amsterdam	Dinesh Batra Florida International University	Indranil Bose University of Hong Kong
Thomas Case Georgia Southern University	Evan Duggan University of the West Indies	Andrew Gemino Simon Fraser University	Matt Germonprez University of Wisconsin-Eau Claire
Mary Granger George Washington University	Åke Gronlund University of Umea	Douglas Havelka Miami University	K.D. Joshi Washington State University
Michel Kalika University of Paris Dauphine	Karlheinz Kautz Copenhagen Business School	Julie Kendall Rutgers University	Nelson King American University of Beirut
Hope Koch Baylor University	Nancy Lankton Marshall University	Claudia Loebbecke University of Cologne	Paul Benjamin Lowry City University of Hong Kong
Don McCubbrey University of Denver	Fred Niederman St. Louis University	Shan Ling Pan National University of Singapore	Katia Passerini New Jersey Institute of Technology
Jan Recker Queensland University of Technology	Jackie Rees Purdue University	Raj Sharman State University of New York at Buffalo	Mikko Siponen University of Oulu
Thompson Teo National University of Singapore	Chelley Vician University of St. Thomas	Padmal Vitharana Syracuse University	Rolf Wigand University of Arkansas, Little Rock
Fons Wijnhoven University of Twente	Vance Wilson Worcester Polytechnic Institute	Yajiong Xue East Carolina University	

## DEPARTMENTS

Information Systems and Healthcare Editor: Vance Wilson	Information Technology and Systems Editors: Dinesh Batra and Andrew Gemino	Papers in French Editor: Michel Kalika
--	---	---

## ADMINISTRATIVE PERSONNEL

James P. Tinsley AIS Executive Director	Vipin Arora CAIS Managing Editor University of Nebraska at Omaha	Sheri Hronek CAIS Publications Editor Hronek Associates, Inc.	Copyediting by S4Carlisle Publishing Services
--	--	---	--

