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### Anatomy of a Good Paper Choosing Research Topics

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## Anatomy of a Good Paper: Choosing Research Topics

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### Abstract:

This report is based on the AIS research exchange held virtually on April 6, 2021, as part of a series on the anatomy of a good paper. This exchange was concerned with how researchers can identify promising research topics and avoid getting bogged down in projects that lack potential. Amber Young moderated an exchange between Robert Davison and Alan Dennis as they responded to questions from her and the audience. After the exchange, Syed Shuva interviewed Davison, Dennis, and Young. This report provides an overview of some literature on where good ideas come from and summarizes the discussion that took place during the research exchange and follow up interviews.

**Keywords:** Interesting Research, Research Ideas, Gap in the Literature, Novel Ideas, Research Contributions, Digital Humans, Future of Work, Artificial Intelligence.

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## 1 Introduction

The AIS research exchange is a series of virtual research sessions for the Information Systems community where participants from around the globe can attend presentations from top researchers before congregating in a virtual café to network and socialize. A series of this research exchange focused on the anatomy of a good paper. Any discussion about the anatomy of a good research paper must first address the heart and soul of the paper, i.e., the research topic. Choosing a good research topic is important because failure to do so may result in the loss of valuable time and energy as the researcher devotes resources to a fruitless pursuit. The research exchange on choosing a research topic was held virtually on April 06, 2021. The purpose of this research exchange was to help IS researchers understand how to avoid getting caught up in a trivial research topic, what constitutes a good research topic, and where good ideas come from. The ideas presented in this research exchange came primarily from two sources. First, this exchange was inspired by an editorial that Davison coauthored, published in the Information Systems Journal, about the importance of compelling problematization in research and the limitations of a gap-spotting approach (Chatterjee & Davison, 2021). Second, this exchange was inspired by a talk Dennis has given several times over the years about how to get published. We hope the ideas presented herein will enthruse other researchers to tackle meaningful, interesting research questions that will move the field forward in dramatic rather than incremental ways. In the following sections, we first review select literature on good research topics before presenting the questions asked during the exchange and interview along with a summary of the discussion that took place around each question. Some of the questions were prepared by Young and Shuva. Other questions came from the audience through the question-and-answer message board. We acknowledge that this report reflects the spontaneity and idiosyncrasies of the exchange and is not meant to provide a comprehensive list of good research topics. We suggest IS researchers supplement this reading with other engaging articles about how to conduct novel and useful research (e.g., Grover & Niederman, 2021).

## 2 Good Research Topics

The mystery of where good ideas come from is of interest to scholars in all scientific fields. Murray Davis' (1971) seminal management paper on what constitutes interesting research recommends offering an unexpected explanation. According to Davis, interesting research finds something assumed to be organized is disorganized, something assumed to be different is the same, something assumed to be individual is holistic, something assumed to be local is general, something assumed to be stable is dynamic, something assumed to be ineffective is effective, something assumed to be bad is good, or vice versa. But how does one stumble upon an unexpected finding? Steven Johnson's (2010) investigation of 189 "great ideas" in business and science reveals that great ideas do not tend to come to us as eureka moments or as the result of deep internal pondering. Rather, great ideas tend to develop cumulatively as the incremental contributions of prior research are pieced together like a puzzle that is indistinguishable until the right combination of pieces makes clear the bigger picture. Johnson also found that great ideas often come from social interaction or other engagement with the outside world. This finding aligns with Isaac Newton's notion of seeing further by standing on the shoulders of giants. It also aligns with Stuart Kauffman's (2000) concept of the 'adjacent possible'; innovation is not the development of something out of nothing but the novel recombination of what already exists.

In established, static scientific fields, incremental contributions are valued because there are relatively few big discoveries left to be made. Dynamism in the IS field provides opportunities to make a bigger impact. As innovation occurs, existing theories may become irrelevant in short order. There are at any given time, an abundance of new technologies, ecosystems, and industries to explore. Thus, in our field more so than in other fields there are opportunities for researchers to fundamentally shape knowledge in profound and meaningful ways.

## 3 The Research Exchange and Follow Up Interviews

The AIS research exchange opened with introductions and transitioned to a semi-structured discussion. Following the research exchange, Shuva posed interview questions to Davison, Dennis, and Young through email. Questions and responses from the exchange and interviews are synthesized and discussed below.

*A common problematization that authors use is, “no one has ever studied this before and there is a gap in the literature.” How would you respond to that kind of gap spotting?*

Gap spotting is for dentists and librarians. Dentists are concerned with gaps in your teeth and librarians are concerned with gaps in bookshelves, but researchers need not concern themselves with gaps in the literature. Some gaps are gaps for a reason and not all holes need to be filled. There are questions with obvious answers that do not require research. There are also questions that no one is concerned with. Those questions also do not require an answer. Rather than motivating research by stating that there is a gap in the literature, researchers should explain why their research matters and who should care about the answer to their research question.

*Where do you find inspiration for ideas?*

Good ideas may come from planned serendipity. You can plan to find things by chance. One way to do this is to look at research beyond the IS field. The news is a great way to identify urgent societal problems that research can address. Dennis’s stream of research on fake news was inspired by his student’s and colleagues’ reactions to news coverage of fake news (e.g., Kim & Dennis, 2019; Kim et al., 2019; Moravec et al., 2020). Reading literature from other disciplines is another great way to spark ideas about how throwing technology into the mix can change things. For instance, applying theories of human-human interaction from the psychology literature to human-technology contexts can be the basis for interesting research. Reading eclectically from diverse fields, e.g., psychology, paleontology, medicine, and pathology, can promote planned serendipity. Books written for an audience somewhere between professionals in the field and the public are particularly helpful. If you read extensively, you will come across many ideas, some of which will pop out at you. Reading fiction and literature can also be helpful. For instance, the dystopian science fiction book ‘1984’ inspired the concept of Informania in Young’s research (Kane et al., 2021). Reading is a great way to expose yourself to ideas but is certainly not the only way. Interacting with practitioners and getting out into the real world is also beneficial. For instance, Davison has come across ideas about use and misuse of technologies while doing action research in organizations (e.g., Davison & Ou, 2018; Davison & Vogel, 2000). He keeps his eyes open for ways of using technology that strike him as a bit odd.

*How do you choose research topics?*

It is best to take on research questions that are broad enough they cannot be answered with one study. The startup costs associated with moving into a new area of research are high. It is best to have synergy. For instance, Dennis has been inspired by *Thinking Fast and Slow* (Kahneman, 2011) and has applied the theories from that book to a series of experiments (eg., Dennis, Kim et al., 2020; Moravec et al., 2020). Young has applied theories of emancipation to multiple studies (e.g., Miranda et al., 2016; Young, 2018; Young et al., 2021a). If you have a fun tool in your toolkit of expertise, it is best to make good use of it. Publishing multiple papers as part of a stream of research addressing a common research topic is a way to make the most of your time and effort.

It is also important to choose topics you find interesting. Research can go beyond looking at how to make corporations richer. There are other stakeholders who matter. There are low-level employees as well as social and citizen stakeholders. The environment is also a stakeholder. Which perspective researchers take matters. The vast majority of IS research looks at the economic interests of organizations, which provides a narrow view of the world (Clarke & Davison, 2020). There is a clear opportunity for more research into how technology can be used for social good and human wellbeing.

*When you move to a new area, how do you identify the most interesting research questions in that area?*

You can start simply; identify questions that speak to the essence of the phenomenon. For example, the dominant thought is that individuals will disclose more to chatbots than to other humans because there is reduced social desirability. Yet, when Dennis studied this phenomenon, he found the exact opposite to be true (Dennis, Kim et al., 2020). Sometimes the best thing you can do to understand a new phenomenon is test dominant assumptions to either verify or debunk them.

When you identify a new area of research, do not be so naïve as to assume you are the only one who found it. It is likely many others have also found this new area and are busily looking for research questions as well. Finding a new, unusual, controversial, or iconoclastic angle can give you an edge. As researchers, we should be unafraid of being iconoclastic. This may feel uncomfortable for early career

researchers, but even at that stage of your career, it is important to get read and cited. If you want to get attention and have an impact, it is good to say something accurate but also novel and controversial.

*Are there any areas that are wide open right now? If you were going to start over as a PhD student, what would a great area to focus on?*

Dennis believes that digital humans are the future of work. Digital humans are about to change how we work, live, play, and learn. They are artificial intelligence agents that look like us, that you can talk to through videoconference. Most digital humans are designed to be workers. Someday workers will be assigned a digital human to work with. Project groups will include a digital human. Soon the interface and graphics will be much better. Remember how the internet changed everything? We are on the cusp of the same thing. These years will be called the pre-digital human years. It is an amazing change that is coming. Future historians will look back at 2020-2022 as the end of one era and the beginning of another, the same way we point to 1991-1993 as the beginning of the Internet age. Take a few minutes to watch a digital human<sup>1</sup> on a Zoom call. Soul Machines<sup>2</sup> is the leading vendor in this space and you can talk to a digital human at their site. They are not quite good enough right now, but firms are already deploying them as call center agents and on Zoom calls to support employee teams. Samsung is calling them “your digital friend<sup>3</sup>”. Soon, they will be good enough, and we will be using them as often as we use Siri, Alexa, or Google Assistant. In a recent study, Dennis and colleagues studied the challenge of crossing the ‘uncanny valley’ in virtual reality when using human-realistic avatars rendered in real-time. Their findings demonstrate the feasibility of crossing the ‘uncanny valley’ with better affinity, trustworthiness, and preference for the human-realistic avatar (Seymour et al., 2021). Within two or three years, we will begin to see digital humans with functional expertise as entry-level sales reps, accountants, doctors, engineers, exercise instructors, teachers, and so on. This raises many questions about how people will react to human-like machines. For instance, will the blurring boundary between humans and machines reduce the salience of users’ humanity during digital interactions? How will changes in the relationship between humans and machines shape relationships among humans?

Davison is interested in animal communication (see for instance Godfrey-Smith, 2017). He believes there is a lot IS researchers can learn from biology-based insights into how animals and other forms of intelligent life communicate information as part of a system. There are ways to communicate without using words or numbers. For instance, colors and the timing and patterns of color changes can communicate a great deal in the animal world. The study of how jellyfish rearrange body parts to promote self-healing has inspired efforts to develop self-healing robots (Imbler, 2015). Just as the concept of geographical fault lines has been applied to describe faultlines in geographically dispersed teams (Polzer et al., 2006) and the concept of imbricated roofing tiles has been applied to interwoven human and material agencies (Leonardi, 2011), insights about the development of intelligent life in biology can inform research on intelligent technologies. Theories of swarm intelligence explain the collective behaviors of unsupervised social insects. These theories have inspired algorithmic developments in multiple contexts including collaborative technologies (Bonabeau et al., 1999). Businesses have used swarm intelligence theories for decades to address logistics and workflow issues. Researchers should consider how theories in other fields can inform the understanding of emergent IS phenomena. For instance, how can theories of information diffusion in nature shape the understanding of digital information diffusion? How can theories of natural ecosystems inform understanding of socio-technical systems?

Young sees a huge opportunity for future-oriented theorizing and even future-oriented empirical work to be done (Young et al., 2021b). A common critique of our field is that research follows practice. If we want to get out in front of practice and lead, we need to anticipate the next generation of features and functionalities and do research that allows firms to adopt and implement new technologies with some understanding of the likely implications of those technologies. Research that investigates the potential impacts of emerging or not-quite-developed functionalities will help organizations and society plan and even alter the current trajectory to build a brighter future. Young also sees an opportunity for normative theorizing and the development of policy implications. Stahl and Markus (2021) assert that IS scholars should claim the authority to develop policies and recommendations related to IS. Researchers in IS have the computer science knowledge as well as the ethical and philosophical background to speak to how technology *should* be used. The IS field has a responsibility to consider how IT affects people, foresee

<sup>1</sup> <https://digitaldomain.com/technology/autonomous-humans/>

<sup>2</sup> <https://www.soulmachines.com/>

<sup>3</sup> <https://neon.life/>

problems, and identify solutions. She is particularly interested in how critical social theories can be used to guide normative recommendations that are forward-looking and socially just. For instance, how can emancipatory and ethical kernel theories be used in design science to promote social good? How can the net emancipatory or oppressive impact of a system be measured and monitored?

*What new technologies or technology features do you think are likely to be most transformative in the next 5 years? What kind of research do we need to help us prepare for that transformation?*

One view is that the things that will transform society 5 years from now have not yet been invented, so researchers should not speculate. Davison notes that he can create a wish list of future technologies, but it would have no basis in reality. Another view is that each generation of technology builds on other technologies and evolves as existing capabilities are combined in novel but imaginable ways. For instance, audio-visual technologies appeal to two senses. Davison suggests output devices like 3D printers will enable technologies that appeal to other senses. For instance, Young points out that 4D theaters expand the 3D movie experience by emitting scents, fog, cold or hot air, and water spray at appropriate times throughout the movie. She expects virtual reality and augmented reality technologies to have a big impact on entertainment and eventually work processes.

Young also expects intelligent agents like Cortana to transform the workplace. She expects that soon most knowledge workers will have virtual assistants to help them with things like responding to emails, scheduling meetings, and organizing files. Intelligent agents are already capable of doing a lot of useful tasks for researchers such as formatting references, developing literature review summaries, writing methods sections of quantitative empirical papers, scanning the internet, and scraping data to analyze for the researcher, etc. These technologies are especially useful for knowledge work. Dennis puts a name to these virtual assistants—digital humans. He predicts that digital humans will revolutionize how we think about technology and where we deploy it. They will upend organizational structures because most organizations are built on the assumption that people are expensive. How will we design organizations when people with entry-level skills are free? And what does this do to the education system? What does this do to the pipeline of entry-level employees from universities into firms? What are the implications for career paths and how will employees move into more senior positions? How will society be structured when AI workforces can be managed by a few specialized decision-makers and many people find their skills obsolete and their labor superfluous? Proactively addressing these and similar questions will help organizations foresee these changes and plan for digital transformation. This is important because foresight allows ethical consideration, whereas manners go out the window quickly when organizations reactively enter crisis mode. Proactive, long-term planning allows consideration of multiple stakeholders and will yield better, more ethical responses to new technologies. For instance, AI replacement of human workers does not have to result in mass unemployment and poverty. Rather, human capital can be redirected to more creative, fun, and fulfilling tasks with proper planning and societal support.

*How do you know when you have exhausted a topic?*

When you find a topic boring, it is time to move on. Your emotional reaction or gut feeling that there is nothing left to say is a good indicator that you should find a new topic. At some point you will want to stop saying the same things over and over and move on to an area where you can say more and make a bigger contribution.

*What kinds of papers get published and have an impact? How can we know when we have landed on a really good idea?*

To say at the idea stage that a paper will be read, cited, and appreciated is optimistic. At the idea stage you cannot be sure that the project will succeed. It is hard to predict how reviewers will respond to a paper. Even good ideas can be rejected from journals. A good idea is necessary but insufficient for publication and impact. Once they have landed on a good idea, researchers must be persuasive in convincing others that the idea is important. They must also possess the skill to design a method to study the idea, the ability to collect data related to the idea, and the contextual understanding to target a journal with a pool of reviewers who will be interested in the idea. Thinking about the idea without considering all these other factors is short-sighted. If it were as simple as generating ideas, a company could sell ideas with a guarantee that all the ideas would result in high-quality, high-impact papers. Unfortunately, it is not that easy.

Research is part science and part art. There are cookbooks for writing a methods section; that is the science. Crafting the presentation of the idea is the art. The art of shaping the fuzzy front-end of a paper is

the hardest part of writing a research article. The introduction of a research paper should explain the context and importance of the research question in a focused manner. Presenting the topic in an interesting way requires saying something someone has not said in exactly that way before. Often, someone will have said something similar, but not presented the idea in quite the same way. A good idea is just a little bit further ahead of where we are as a field. If you get too far ahead, readers will have trouble conceptualizing what you are talking about.

A good research question is one that does not have an obvious answer. A good research question requires data to answer the question. The best questions are those the researcher does not know the answer to. It is hard to publish a paper with predictable results in a top journal. Deciding how far beyond the predictable to go, and how much to push the boundaries of knowledge is tricky. Increasingly, reviewers are demanding a larger contribution. That means that engaging in nano-incrementalism, making tiny, tiny steps forward, may not impress reviewers. Even tiny steps forward make progress but going further into the unknown can lead to a greater contribution. Of course, taking large steps forward is risky. Another trend in reviewing is that reviewers like to see one over-arching theory. While theory is important, it can be challenging to find one over-arching theory that fits an emerging phenomenon. Beholding researchers to over-arching theories may encourage incrementalism.

*Do you often find that you are surprised by the results of your research?*

Yes, the most interesting research projects are often the ones where the research question has failed. Sometimes it seems like a study could go one of two ways, and then it goes in a completely unexpected direction. This can leave the researcher scratching their head. For instance, Dennis is working on a project with three hypotheses. The hypothesis for one relationship was supported. Another was not significant and the third has the opposite directionality of what he predicted. This is an indication that the theory around which the study was designed is flawed. Digging deeper into unexpected findings can reveal flaws in theory and help move theory forward.

Isaac Asimov once said, "The most exciting phrase to hear in science, the one that heralds new discoveries, is not 'Eureka!' but 'That's funny...'" Funny means you found something you did not expect, and you should rethink the theory. That is serendipity. Serendipity requires not only finding something you were not looking for, but also realizing you found something you were not looking for. It is about keeping your eyes open wide enough to see the whole world and not just a small part of it. It is a challenge for researchers to be open to unexpected findings and then to be prepared to run with them and try to explain them. That is where the contribution is. Just saying, "My hypotheses were not supported, too bad," does not move the field forward. In this era of readily available data, an unexpected finding can prompt another study to investigate exactly why the first study failed. A follow-up study can help you capitalize on what you learned from a failed study. Davison used an example of such a failure (Dennis et al., 1998), and a follow-up analysis 14 years later (Lee & Dennis, 2012), to illustrate how this can be put into practice.

*When a publish or perish mentality dominates, it is easy to get caught up in asking: Can I do this study? Is it feasible? But when the focus is on **can** rather than **should** we risk losing sight of the big picture and why we do research at all. With so much data readily available online, do you see a trend of researchers, particularly early career researchers like PhD students, saying, "Well, we have data, let's see if we can get a paper out of this?"*

Mining data is a good way to answer questions, but it is important to make sure the questions matter. Data mining is not a bad thing, but there are limitations regarding the types of questions one can explore using predetermined metrics. When a researcher develops a research question and then designs a study to answer that question, the researcher can pursue an interesting research question. Working backward to retrofit theory and interesting research questions to a set of findings from data that was not collected in an intentional way for the purposes of the study is challenging. Researchers may be very impressive with sophisticated analyses, but analysis for the sake of analysis is not valuable. Crunching numbers alone should not make for a publishable contribution in an IS journal. Instead, there should be a compelling reason for the analysis. One way to leverage data mining is to use a combination of approaches. For instance, data mining can be used to identify trends in the real world and a follow-up lab experiment or survey can round out our understanding. Data from the field is real data and has the advantage of reflecting actual events, but demonstrating causality is challenging. In contrast, lab experiments demonstrate causality, but do not provide authentic data. One good approach is to use one method to justify the need for research on the topic (perhaps a qualitative case study) and another method to answer the research question (perhaps a quantitative survey or a laboratory experiment).



*It seems like IS touches every discipline these days. For instance, human resource officers are using AI to sort through resumes and managers use AI-enhanced scheduling apps. What distinguishes an IS research topic from a management research topic with an IT component?*

There are different views within the IS discipline. Davison explains that an IS topic can be seen as one that has an IS contribution and is written for an IS audience. An IS audience is likely to be interested in a different kind of contribution than a management audience. Dennis and Young favor a broad view of IS. Dennis describes himself as being on the extreme left wing of our field on this issue with his belief that any paper that provides insight into the design, use, management, or policy of/for technology is part of the field. One view is that the only research that provided insights for chief information officers is IS research. However, such a view renders research about fake news on social media outside the scope of the field. Dennis brings up the counterargument, "If we don't study it, who will?" That said, Young suggests that technology should not be merely incidental in an IS paper. The study should be designed to teach us something about technology and the paper should be written in a way that highlights the implications for IS design, development, implementation, or use.

*Sometimes it is not an issue of authors not choosing interesting and important topics, but an issue of authors failing to communicate why the idea matters. It can be good to have examples and templates as we learn how to write and package our ideas. How can researchers communicate their research hook well?*

All published papers should provide a research hook. New writers like PhD students may benefit from identifying papers in their research area to emulate. They should pay particular attention to the introduction. An introduction is important because it motivates the reader to go on to subsequent pages. The introduction should inspire the reader that the rest of the paper is worth reading. An introduction does not need to be long and can be written using a cookbook approach. That is, set the stage and describe the overall phenomenon in the first paragraph. The second paragraph should describe what the researcher wants to understand and why. The third paragraph should describe what the researcher found and why it matters. The introduction does not need to have references but may have one or two references to provide context. Some papers are written with a literary style, but that is not necessary. A common mistake is to put information that belongs in the abstract or literature review in the introduction, and vice versa. Nothing that appears in the abstract should appear in the body of the paper. An abstract should be written separately to summarize the paper.

*How do we write to the audience we want to reach?*

When you write to a journal, you are writing with a specific audience in mind. While many journals may appear to be mainstream or broad in terms of what they publish, in reality each journal is likely to be known for particular genres of research. For instance, *Information Systems Journal* has historically published a lot of interpretive research that draws on qualitative data. In the last few years that has changed with many quantitative data studies published, but few papers that apply an econometric approach or that involve analyses of electronic marketing are submitted. Special issues in a journal encourage submissions on specific topic areas and are a good sign that the journal editors and readers are currently interested in those themes.

Davison wrote about the nature of the audience in this editorial (Davison, 2019), suggesting three possible audiences: the readers, the reviewers, and the self. Perhaps it is idealistic, but many authors probably start off by writing for a particular audience of readers. However, as the reviews and demands for change come into the picture, the audience is cynically transformed into the reviewers themselves. As Allen Lee put it (cited in Davison, 2019), "I believe that many or most seasoned researchers write with reviewers in mind, if not for the first submission, then certainly by the third revision. In fact, the exasperation is so great by the third or fourth revision that the general audience has completely disappeared from the author's mind and the author is left just wanting to address what the reviewers and editor want." This is very sad: in our view, the author should always be in a position to address the audience of readers and it is the responsibility of the Senior Editor handling the submission to facilitate this.

*As new, more sophisticated technologies emerge, how should we study them? Should we replicate existing studies using the new IT artifact or should we assume previous research on prior generations of IT is generalizable?*

Theoretically, there may be less gained by retesting old theories with new data and new technologies. For instance, TAM will be just the same. Thus, Davison sees little theoretical value in replication ad nauseam

though some journals do publish replications. Davison sees more value in new theoretical ideas. In contrast, Young believes replication studies are an important way to verify and refine existing knowledge. Without replications, one cannot be sure that controversial studies are not a fluke. Replication research enhances the credibility of existing research when findings align and reveal anomalies, and opportunities for theory development when findings diverge. Ideally, though, researchers should focus on IS features, capabilities, or affordances to promote generalizability and ensure research is still applicable as technology evolves.

Technology changes. Some changes are minor while others change our theoretical conception of the technology or how it should be used. Some changes are fundamentally disruptive. Chatbots have been widely adopted over the past few years. To some extent, they are quite like search boxes that provide access to self-service customer support information, but they radically change how we think about self-service support and they have been fabulously successful in the market. Dennis points out that we cannot just assume that what we know from prior self-service support research applies to chatbot. Likewise, digital humans are poised to make chatbots obsolete; researchers cannot just take chatbot research and assume it applies to digital humans providing self-service support because once again, this technology will totally disrupt what we think of as self-service support.

## 4 Final Words of Advice

We hope this paper has sparked some good ideas. We leave you with this final advice from Steven Johnson's (2010, p. 261) book *Where Good Ideas Come From*:

*Go for a walk; cultivate hunches; write everything down, but keep your folders messy; embrace serendipity; make generative mistakes; take on multiple hobbies; frequent coffeehouses and other liquid networks; follow the links; let others build on your ideas; borrow, recycle, reinvent.*

Good topics are best identified through engagement with the real world and IS practice, so get out there and explore the world. Pursue the big, unresolved questions that matter. Find your passion and create synergies in your work.

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