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Great theses and dissertation start with an intriguing idea^{☆☆,☆}

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ABSTRACT

Strategic efforts expended towards identifying and evaluating ideas for a graduate student's thesis or dissertation can help prepare that student for a successful career as either a researcher or practitioner. Generating intriguing and novel ideas can help advance the field of leisure studies, yet the process of identifying great research ideas and questions is largely uncritiqued. This paper presents alternatives to the traditional “extend the research literature” approach, as a means of identifying research questions focusing more strategically on the diversity of information and process of observation and thinking. Graduate students upon beginning their studies can be mentored by faculty to explore ‘what's next’ by capitalizing on their ‘fresh eyes’ to generate novel ideas worth exploring. Some of these methods are akin to how persons and organizations develop ideas for new products and services. Consequently, these methods not only help students going into research-based careers but also those intending to work in professions outside of academia.

1. Introduction

Scholars have critiqued how the knowledge base in the fields of parks, recreation, and tourism is created. Some have explored the paradigmatic and pragmatic orientations of researchers and the editors and editorial boards who act ‘gatekeepers’ to the types of research that are disseminated (e.g., Aitchison, 2001; Mair & Reid, 2007; Ren, Pritchard, & Morgan, 2010). Others have argued for increased attention to the paradigms researchers operate from (positivism vs. interpretivism), as well as the types of methods used to answer questions being asked (Mair & Reid, 2007; Samdahl, 1999). However, much less attention has been given to the processes of identifying salient research topics. That is, how do emerging scholars identify unique, meaningful, and intriguing questions that will impact knowledge production within the leisure field? As Alvesson and Sandberg (2013) have stated, “a novel research question may be what distinguishes exceptional from mediocre research and the production of trivial results” (p. 1). If established researchers do not consistently grapple with this challenge they end up modeling traditional approaches to the scientific process to their students. This contributes to a weak and dated research literature, with cascading effects on our professional literature and practice. Consequently, the purpose of this paper is to provide guidance to graduate student mentors, and new scholars themselves, on how to identify great ideas. The paper describes the dangers of simply trying to “extend the literature”. Following this, we will propose methods for generating great ideas for leisure research that addresses the environment and processes we study.

^{☆☆} Author order is randomly assigned.

[☆] This project was unfunded.

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1.1. How do we mentor students to encourage the production of great research ideas?

Science is expensive, time intensive, and at times tedious. Why not strive to make it deeply meaningful? A major challenge for students beginning their formal graduate studies is sorting through general research ideas to identify a specific topic and research question(s) for their thesis or dissertation. The value of a graduate education, and its impact on our profession, is partially a function of the quality of the research question chosen. The optimum outcome is that the search yields an influential and interesting idea. There are many unexplored issues, problems, and opportunities in our field that have not yet been addressed, but for a novice researcher, recognizing them is a challenging but crucial task.

The difficulties faced in choosing questions are exacerbated within the digital age by information anxiety and overload due to the proliferation of information available to us and its diffusion beyond academic settings (Bawden & Robinson, 2009; Hessels & van Lente, 2008). Ideas produced outside the confines of the ivory towers must also be considered within our own approach to science and the production of knowledge as scientists. Producing great ideas requires not only gathering information from the environment around us, but also a process of integrative thinking. Some of our greatest scholars, such as Einstein, have noted that ideas are produced from combinations of old elements, or what were originally disparate thoughts. (also see Abbott, 2004).

Falling into status-quo practices of developing research ideas (e.g., 'read the literature') is too easy, resulting in projects that rarely adopt counterintuitive perspectives that support impactful research and have the potential to challenge conventional wisdom (Abbott, 2004; Astley, 1985; Davis, 1971; Ioannidis, 2005). As educators and researchers who are training new leisure scholars, we must explore mentoring strategies to help students identify great thesis and dissertation topics and questions.

Science, regardless of philosophical school, is a systematic investigation of phenomena through observation and interpretation of data. Scientific progress occurs through discovery, analysis, explanation, integration, and development (Schafer, 2012). Learning the research process – and how to do research – is integrated into most graduate education programs through required research design and methods courses. Yet, many students still develop an inaccurate sense of the purposes and potential of social sciences. This alternative conceptualization of the purposes of research is linked to distorted motivations from the need to win external funding, sensational reporting of science in the media, and how activities that appear to be science" are theatrically produced for political purposes (Alvesson & Sandberg, 2013; Gibbons et al., 1994).

Politics, economics, and personal dogma may result in losing sight of the traditional purposes of social science. Additionally, researchers often gravitate to popular or status enhancing topics which are already over-populated with researchers. Thus, teaching students to negotiate these factors is challenging and with more guidance given as to what constitutes an important research topic, the greater chance they have to stay focused on the purpose of science, and identify new and novel research topics.

1.2. Paradigm shift: Extending the literature is not enough

Emerging ideas in the social sciences suggest that a different approach to identifying topics and questions is needed rather than 'extending the literature', 'filling the research gap', or justifying a study by noting that 'no other studies have examined the issue' (Alvesson & Sandberg, 2013). While Alvesson and Sandberg suggest an approach of *problematization from existing academic literature* to develop interesting questions, this paper goes further. Specifically, we contend that while reading the existing literature in our field is a necessary part of a graduate education, it is an act of looking backwards. Great questions depend on first having small ideas; these small ideas come together to form complex ideas that create new and potentially intriguing questions. Yet, if all the ideas are pulled from what is already done, then we essentially ignore the diversity of knowledge production that defines the 21st century and reinforce assumptions that need to be questioned.

The importance of this is most evident when taking a historical approach to thinking about the cycle of science. Science goes through periods of stability then a collapse of a dominant perspective occurs through the emergence of new ideas. For example, at the time of this writing, the social sciences are in the middle of just such a paradigm shift. Mainstream psychology, communications, economics, and marketing research are rapidly moving away from viewing humans as agentic rational actors capable of providing accurate retrospective and prospective answers to researchers' questions on surveys and through interviews/focus groups (Graves, 2013; Ioannidis, 2005; Tellock, 2005). Even with a major development within psychology research throughout the latter part of the 20th century, based on the work of Daniel Kahneman and Amos Tversky (Lewis, 2016), documenting cognitive biases in human judgment (Hastie & Dawes, 2009), these new ideas have remained eerily isolated. For example, in economics, this notion was only widely acknowledged after the consequences of the great recession of 2008/09 (see Ariely, 2009).

Simply extending the existing literature is a constrictive exercise, and thus, we need more insightful approaches to thinking about great research ideas and questions. Great ideas for research topics are more likely to come from multiple information sources and processes. We need to guide new scholars to balance a skeptical respect (Graves, 2011) for the existing body of literature, with the ability to critique with 'fresh eyes' the questions we should be asking when approaching new research projects.

1.3. Judging the quality of an idea: The Murray Davis Test

While science is fundamentally responsible for identifying truths, novel and counterintuitive scientific truths seem to have the greatest influence within society. The seminal work by Davis in 1971 surveyed sociological theories to identify those that historically have had the greatest impact. How interesting the theory was, rather than its accuracy was predictive of the quality of "greatness." First, Davis (1971) makes it clear that research and theory that only affirms what is already known will be viewed as trivial as the findings fail to challenge the audience's assumptions. As an example, the Theory of Planned Behavior, has become a mainstay of

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