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Increasing Research Relevance in DSS: Looking Forward by Reflecting on 40 years of Progress

Karen Vizecky
Dakota State University

Omar F. El-Gayar
Dakota State University

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Increasing Research Relevance in DSS: Looking Forward by Reflecting on 40 Years of Progress

Karen Vizecky
Fortune 500 Retailer
Dakota State University
kjvizecky@pluto.dsu.edu

Omar El-Gayar
College of Business and Information Systems
Dakota State University
Omar.El-Gayar@dsu.edu

Abstract

Decision Support Systems (DSS) is a field that has made rapid progress in establishing itself and maturing into seven key sub-fields. However, there are continued concerns about the relevancy and growing disconnect between academic contributions and professional applications of DSS. This paper serves to review the existing literature, and propose a Research Relevance Framework meant to serve as a guideline to ensure future DSS research has a consistent guideline upon which relevance can be assessed.

1. Introduction

The field of Decision Support Systems has evolved, matured and re-invented itself since its inception in the early 1970's. Reflective of its roots in supporting complex problem solving and decision making, the field has adapted to current t
rends in technology, interfaces and contemporary business problems all while establishing relevance in the Information Systems domain. This has not been an easy task given the significant advances that have taken place in the past four decades. However, the key concept supporting the field remains the same today as it did in the 1970's; providing improved decision support to decision makers through leveraging the data that exists internally or externally through fast and flexible systems.

It is in this evolving information environment that this paper stands to reflect upon the birth and development of DSS while postulating the future of the field. This paper will highlight the continued struggle that the discipline has experienced in regards to retaining relevancy of research to practice. In so doing the paper is structured as follows: first, a review of the conception of DSS will be presented. The history of the field will be followed by a discussion of the highly relevant and cited papers in relation to their influence on the development of DSS. The challenges, opportunities and predictions of these highly pertinent papers will then be appraised against market trends in describing a proposed research framework focusing on academic and professional relevancy. Finally, the author will suggest a research agenda that illustrates the use of the proposed relevancy framework in relation to a current contemporary issue. Finally, the paper will conclude with a summary of the field and the future facing DSS in its' continued quest to remain relevant in a rapidly evolving environment.

2. The history of decision support systems

The field of DSS was born in 1971 when Gorry and Morton [2] combined existing theories proposed by Anthony [3] relating to categories of management theory, and the work of Simon [4] in regards to decision types into a new framework. This seminal work focused on improving the existing field of Management Information Systems to address semi-structured and unstructured decision making. The new framework Gorry and Morton proposed called for dramatic changes in thinking relating to systems design, organizational structure and model differences in challenging the design based on the construct of the decision which they are supporting [2].

The work of Gorry and Morton went on to be the foundation for continued research and publication in the field of DSS including the work by Keen and Morton which focused specifically on the semi-structured aspects of managerial decision making [5]. This book remains as one of the four recognized as foundational to the development of DSS in the 1970's and early 1980's. Throughout this time period, work focused on development of systems where human decision makers interacted with information technology (IT) in an attempt to make the decision maker more effective.

As a result of the foundational work in the 1970's, the field of DSS began to diverge and specialize in the decades that followed. Sub-fields have been introduced to the DSS landscape, and they have been classified into seven distinct capabilities. These include 1) Personal Decision Support Systems

(PDSS), 2) Group Decision Support Systems (GDSS), 3) Expert Information Systems (EIS) now commonly referred to as Business Intelligence (BI), 4) Data Warehousing, 5) Intelligent Decision Support Systems including Artificial Intelligence (AI) and Data Mining (DM), 6) Knowledge Management Decision Support Systems and 7) Negotiation Support Systems. The evolution of the field of DSS and the development into sub-fields has been best captured by Arnott and Pervan [1] as illustrated in Figure 1.

be specifically mentioned in order to maintain the overall purpose of this paper.

Throughout the 1970's and early 1980's the field of DSS focused on evolving from the original foundations of the failed field of Management Information Systems to successful single manager focused Personal Decision Support Systems [1]. By the mid-1980's researchers began to see the opportunity that DSS had in influencing corporate activities rather than very specifically designed individualized systems [11]. It was at this juncture that
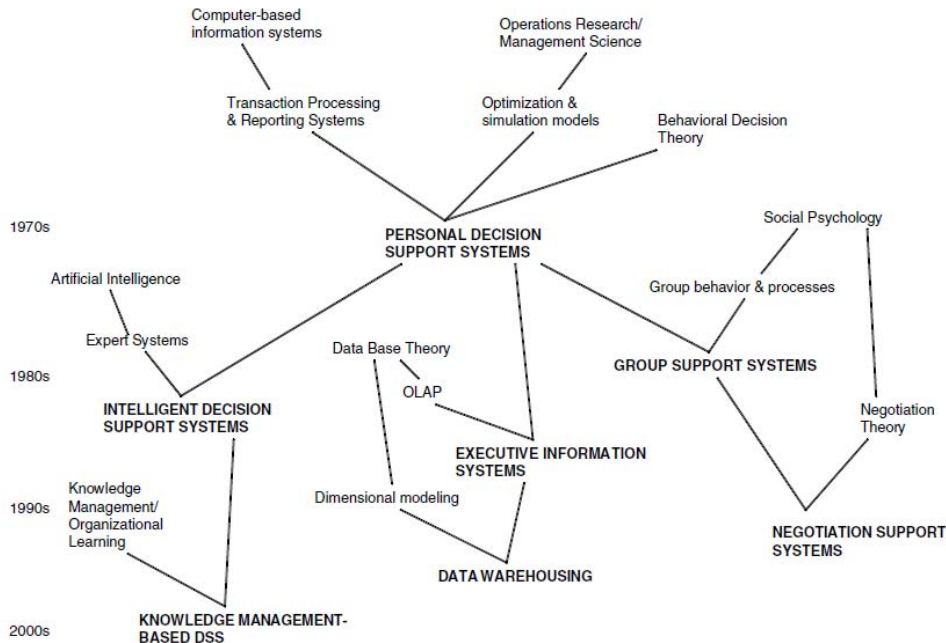

Figure 1. Arnott & Pervan's Evolution of the DSS field [5]

Each of the sub-fields was driven by the professional and academic leaders who saw a unique and distinct niche for which DSS would be beneficial. However, DSS remains a field that is at odds in terms of development. While professional relevance has been questioned in Information Systems as a whole [6], it is particularly concerning in a field such as DSS which is so closely tied to enabling the "business".

3. Literature review

In order to best understand the academic contributions in DSS, it was essential to review the literature surrounding the field. As such, the literature review undertaken spanned the four decades since the inception of the field in the 1970's through to most recent publications in 2010. Given the strength of existing literature reviews [1; 7; 8; 9; 10], only those papers that contribute significant original insight will

researchers became keenly aware of the distinct requirements for relevance relating to DSS. In lamenting the passive stance that DSS development had taken to date Jelassi states "*DSS could also undertake a far more active stance by identifying gaps in existing operations and suggesting ways to strengthen the standing of the firm*" [11].

The challenge Jelassi laid down for the field of DSS did not go unnoticed, and publications surrounding the implementation and optimal use of DSS began to appear. In perhaps the most practitioner friendly of those publications, Bidgoli wrote a common sense guide to the packages available and the market and their optimal use [12]. It is not surprising that at this same time, significant development and creativity took root through the introduction of new sub-fields of DSS. The advent of Group Decision Support Systems (GDSS), also referred to as Group Support Systems (GSS) can be attributed to this period of growth and discovery in the 1980's.

By the 1990's literature reviews were saturated with new research introducing the impact of the new DSS technologies [13; 14]. These papers were significant in their continued challenge to increase relevance and impact of DSS research through multi-methodological foundations [14] and theoretical basis [13]. As was described by Benbasat and Nault in their 1990 work, *"It is ironic that even though the fundamental studies in this field have placed emphasis on understanding the influence of computerized support on the decision making process, few MSS studies that examine this relationship have emerged"* [13]. Articles throughout the mid 1990's continued to focus on the institutional importance of DSS and the need for the field to continue to innovate and expand the scope of DSS research [15; 16]. Frameworks and literature reviews highlighted the continued research opportunity as was described by Eierman et al. *"...past research has examined less than half of the possible relationships among constructs, creating opportunities for new insights"[15].*

Meanwhile research contributions and influential contributors were evaluated by Eom as a way in which to reflect upon researchers who met high research standards as illustrated by the volume of peer citations [10; 17; 18]. In so doing, Eom provided benchmarks and examples of researchers who were performing highly cited research. While this methodology did not take into account professional relevance, it did serve as a reference point upon which novice researchers could benchmark themselves. Eom's work also served to establish 3 universities as top institutions for DSS research including MIT, University of Texas-Austin and the University of Minnesota.

Again, as the calls for relevance and highly citable research increased, the field of DSS fractured further into more specific sub-fields. Perhaps this fracture was in response to the call for relevance and the recognition that relevance can often be achieved through specialization, or perhaps it was simply the maturity curve of the DSS field. Either way it resulted in the birth of many sub-fields including Intelligent Decision Support Systems, Executive Information Systems, Business Intelligence, Data Warehouses and Knowledge Management based DSS. It is therefore not surprising that in the early part of this decade, retrospective literature reviews of the DSS field became widely published, even resulting in a special issue in the Journal of Information Technology in 2005 [1; 7; 8].

Almost three decades after the original criticism of relevance in the DSS surfaced [11], each of the literature reviews in the 2000's highlighted the continued and growing gap between academic contributions and professional practice. Unfortunately,

in his literature review spanning 1093 articles over almost two decades Arnott listed relevance as the number one concern in his summary of the eight key issues for DSS [19]. Arnott's 2008 paper followed two highly explicit criticisms of relevance in 2005 which concluded that DSS faced *"...a crisis of professional relevance" [1]* stating further *"...low practical relevance of DSS research is in part of symptom of research inertia...the earliest sub-fields, now 30-40 years old, still dominate quality research publication"[1].* This criticism followed findings that illustrated that *"...personal DSS and GSS dominate the DSS literature with data warehousing the least published, even though the latter is the most prevalent in practice" [8].*

Accordingly, we recognize that while the field of DSS has evolved dramatically in four decades, the contributions of the field are still overshadowed by the underwhelming ability to achieve professional relevance. Therefore, the remainder of this paper will focus on how we can apply the findings of this literature review in developing a framework for ensuring relevance in ongoing and future DSS research.

4. Proposed research framework

Decision Support Systems is a field that is built upon professional relevance, as indeed without managers in professional venues to support, the very foundation of DSS is abolished [2]. As we just discussed in depth the findings of the literature review strongly highlight the need for the field to ensure relevance in the research agenda [1; 8; 14; 16; 19]. Criticisms such as those leveled by Arnott *"DSS research is simply focusing on the wrong application areas" [1]* are specific to the DSS domain, however the author recognizes that a broader debate of relevance exists in the Information Systems community [20; 21]. Given the breadth of the ongoing debate in IS, and the continued discussion of research rigor versus the shortcomings in relevance the research framework being proposed focuses on establishing relevance criterion specific to the field of DSS given the distinct business facing nature of the DSS context. This framework therefore seeks to serve as an evaluation mechanism for future DSS research agendas. We suggest researchers should continue to consult the work of Henver et al. [22] in ensuring their DSS research is rigorous without compromising the relevance of the contribution.

Relevance can be defined in many ways and therefore the criterion to evaluate relevance can be complex. In order to provide a simplified framework the author has adopted the definition of relevance laid out by Hjorland and Christensen [23]. Therefore

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relevance will be defined as *"Something (A) is relevant to a task (T) if it increases the likelihood of accomplishing the goal (G), which is implied by T."[23]*. Therefore, we propose that while the task is certainly variable in DSS research there are specific criterion for reaching the goal of relevant academic research. However, as Hjorland and Christensen postulated, components of that goal may not be implied by the task. As such, this framework should be used as a starting point for relevance evaluation, and those framework components that are not implied by the task should be ignored.

We propose that the goal (G) of DSS research is made up of five components as illustrated in Figure 2. These five components were derived from the shortcomings of DSS research illustrated in the literature review. Therefore, while the evaluation criterion for relevance can vary based on individual perceptions and experiences, the components selected above are based on best practices, and shortcomings identified by experts in the Information Systems and DSS fields.

Figure 2. Components of the Goal (G) of DSS Research

The need for a theoretical framework, and grounding in existing literature was seen as a continued challenge in DSS publications. As Arnott and Pervan described the need to expand theoretical confines of DSS research beyond specific behavior decision theory is critical to leading practice:

DSS researchers need to embrace contemporary research in psychology, management and related fields to provide a stronger theoretical basis for projects. DSS seems to have an over reliance on the style behavioral decision theory developed by Herbert Simon. We believe that a broader theoretical foundation may also make DSS research more relevant as the use of a narrow base of reference theory may have acted to overly constrain what projects have been thought to be feasible and important. A broader foundation may take DSS research

into a role of shaping practice rather than ignoring it.[1]

While grounding in existing theory, and expanding that theory base beyond traditional DSS sources may serve to improve relevance, the feasibility and applicability of the research must also be considered. In order for research to serve a purpose beyond pure academic enlightenment, it must be feasible to implement and applicable to settings beyond which it was created or proposed. There are a variety of factors that influence feasibility; most important is that the process or artifact created is simple and straightforward enough to be implemented by someone other than the original creator. A strong example of having successfully contributed a research product that was both feasible and applicable was the work of Datta et al. in the proposal of the OLAP cube structure and algebraic foundation [24]. As the excerpt below illustrates the authors not only identified the issue and proposed foundational solutions, they also provided an example of how the model could be applied. This simplification of the research message to allow it to be interpreted in a variety of environments is key to the relevance of this strategically important and relevant contribution to OLAP development.

In this paper, we have addressed an important issue within the realm of decision support databases: the lack of a precise, commonly agreed upon conceptual model for OLAP…To address this problem, we have made two significant contributions. First, we have presented a detailed data model for the data cube. Secondly, we have presented a detailed operations model for the data cube in the form of a powerful yet simple algebra that operates on the data cube. Our proposed model and algebra meets one of the key requirements of OLAP by allowing uniform treatment of dimensions and measures. We have also demonstrated the capabilities of the proposed algebra by providing examples of typical OLAP queries expressed in our algebra.[24]

The third component is critical to all research, and that is the timeliness of the research. While this statement can often be misconstrued as a "need for speed", that is not necessarily the case. Rather timeliness is a more complex concept that focus instead of the three aspects of receptivity, uniqueness of contribution and latency to the marketplace. While academic research in most disciplines leads that of professional practice, this has not been the case with DSS [7; 8]. In order to regain the position of leading

the profession through academic research, researchers must gain credibility with timing of that research being a significant influencing factor. If a researcher is proposing that the existing data warehouse is an antiquated technology that should be replaced with a conceptual schema that they just developed that will cost the company $3M to implement, the receptivity will likely be low. However, if a researcher is proposing a contribution that optimizes performance of antiquated technology utilizing new indexing or taxonomic techniques that lower the cost to implement and reduce business interruption the receptivity is likely to be much higher. The same is true in regards to uniqueness of contribution and speed to market. Research only meets the goal and relevance if it is unique either in proposing a new concept, or in proving that an existing concept will work in a new way. Confirming a concept, while useful does not equate to timeliness. Speed to market is essential to achieving the research goal. Take for example, Apple and the iPhone as an example of technology transforming the marketplace. A great idea can only stay internal to the researchers developing it for a limited period of time, the key being that the researchers must be able to transform that idea into a product, or research contribution prior to competitors. Researchers in the DSS field must always keep in mind the competitiveness of the DSS marketplace and the deep pockets of the vendors which supply the supporting technologies. A research contribution is only relevant if the research can be completed and delivered in timelines similar to that of the private market place.

The fourth component is in relation to the resources and skill sets required to implement the research. Relevant research as a general rule of thumb can be used in an implementation of a project or technology. However, in order to be implemented, the proposed outcome of the research must be able to be implemented within the confines of resources, be they financial or human in nature. Therefore, a researcher should bear in mind when conducting DSS research, if the technologies, solutions, concepts or frameworks they are proposing can be reasonably achieved. There are few organizations that exist in the world with unlimited time, money and human capital. Grounding research to the professional audience and considering the equivalent of the contributions "target market" will ensure that the research product is appropriate for the task, and also fiscally practical to implement. It is suggested that this viewpoint will spur greater creativity in the researchers' solution space as well by

leveraging the DSS foundational iterative design and incremental improvement approach[2].

Finally, the most important aspect of achieving the goal of academic research is in providing competitive advantage. There are few professional organizations that are going to utilize academic contributions that advertise that they will make the organization less competitive. While this seems to be a fairly common sense statement, it is laced with truism that is all too often ignored by the research community. Take for example the evolution of Medical Diagnostic Decision Support Systems. While these systems have existed for nearly twenty-five years, adoption rates have been low and the products have been laced with a myriad of problems [25]. We now sit in 2010 with continued low adoption rates, although academic research has been significant as to technology improvements. Well there are many reasons 'why' this has occurred, one might go so far as to assume that the research contributions have not yet provided the compelling competitive advantage that drives the engine that is consumers. Without product demand for an Electronic Patient Record, a Clinical Decision Support System, or a Personal Health Record the research contributions have been largely academic. Therefore a researcher must consider prior to conducting their research what goals exist for the DSS research topic at hand that would result in competitive advantage for a professional organization.

The research framework proposed is summarized in Figure 3. This framework recognizes that a researcher must have a clearly defined task, which they can evaluate against the 5 components of goals as described in detail above. Following this simple Research Relevance Framework has the potential to assist researchers to evaluate their research proposal to ensure its contributions will be significant beyond an academic audience and rather can also drive DSS professional practice.

5. Application of the research relevance framework

The Research Relevance Framework outlined above provides the opportunity to discuss contemporary concepts that are highly relevant to the professional community. Given the authors unique position of straddling the professional and academic communities, the discussion that follows offers the perspective of both a researcher and practitioner.

As a foundation for the contemporary concepts in DSS, the author proposes an extension of the work of Arnott and Pervan [1] to include three sub-fields of high relevance to the professional community for 2010 and beyond. The inclusion of one of the sub-fields requires the addition of a field that existed earlier in the DSS framework that was not originally included. The extensions to the proposed framework are included in green in Figure 4. These sub-fields were selected

beyond DSS into the overall Information Systems domain.

In order to illustrate the evaluation method of the Research Relevance Framework, we will describe in depth only one of the proposed sub-fields. Given the authors extensive professional experience in DSS in the retail, financial services and healthcare sectors the field of Organizational Knowledge Decision Support Systems has been selected for in depth
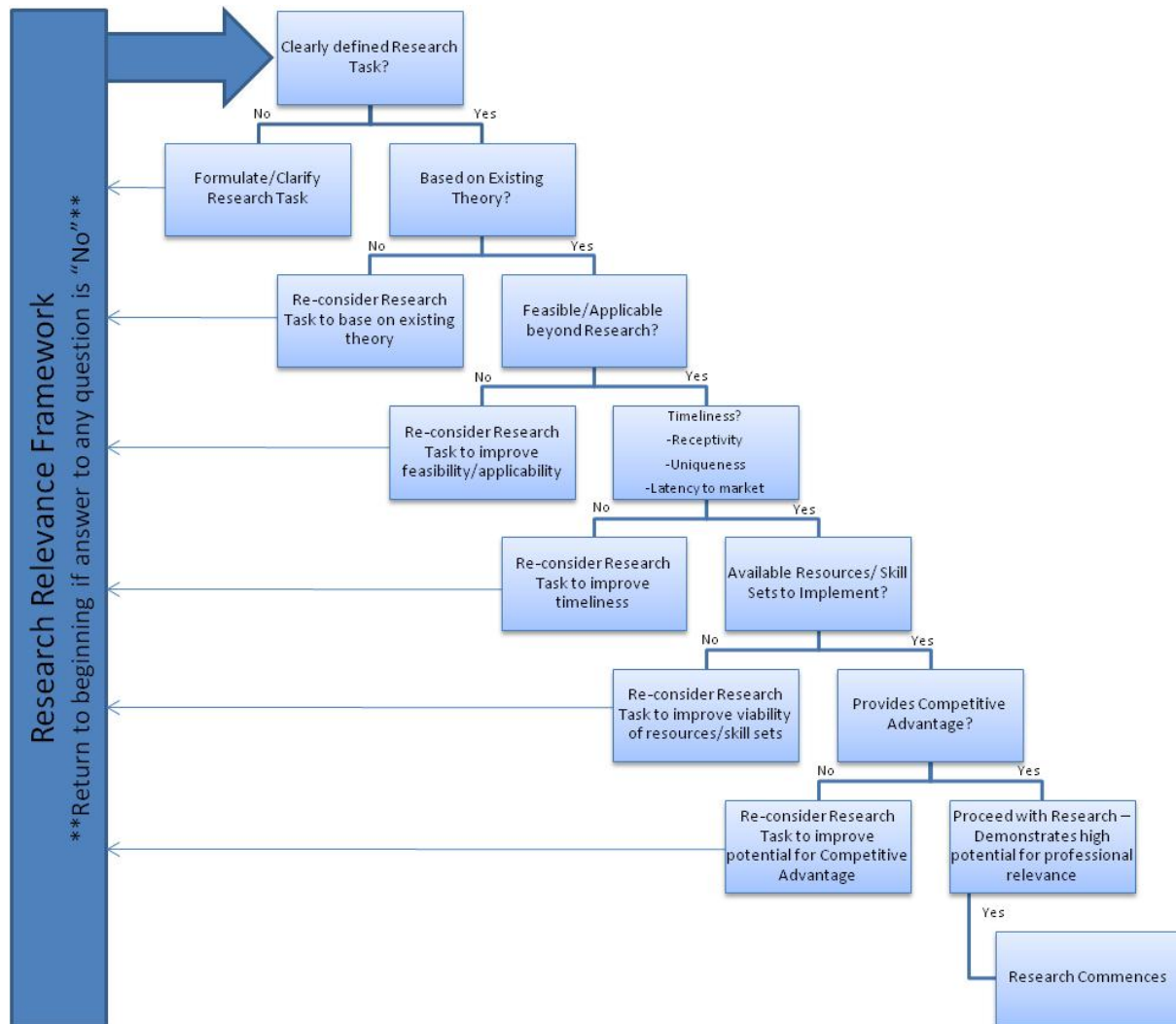

Figure 3. Research Relevance Framework

based on the Research Relevance Framework and include Clinical Decision Support Systems, Organizational Knowledge Decision Support Systems, and Spatial Decision Support Systems. While the author recognizes that there are additional supporting technologies that will undoubtedly shape the field of DSS including social networking, mobile technologies and advances in visualization these technologies are umbrella technologies whose impact will extend

evaluation due to its potential breadth of application.

5.1. Organizational knowledge decision support systems

Organizational Knowledge Decision Support Systems is an emerging concept surrounding the need for a marrying of knowledge management of organizational knowledge and the data available in data

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warehouses. This sub-field is a contemporary nod to the success of knowledge management research in the arena of DSS and the continued opportunity to expand that success to assist in new and emerging fields [26]. In combining the success of knowledge management with the relative underwhelming research into data warehouses, this sub-field has the opportunity to marry organizational knowledge with the organizational data. Data warehousing is reaching a critical mass in the professional arena, but utilization is still broadly centralized in Centers of Excellence [27] or Competency Centers [28; 29], organizational designs popularized in the late 1990's and early 2000's. Realization of the strength and capabilities of data warehouses really lies in distributing the technology to the masses, but doing so appropriately requires knowledge transfer about the data that exists and its appropriate use and interpretation. The opportunity for research and contribution in this arena are paramount to DSS relevance.

In order to illustrate the benefits of the

explicit knowledge transfer within an organization relating to data sources, specifically data warehouses. Given this definite task, the below description focuses on answering each of the questions of the Research Relevance Framework.

First, and foremost, there exists a clearly defined research task that is specific as to the types of knowledge to be transferred, the inter-organizational sender/receiver nature of knowledge transfer, and the subject matter expertise to be transferred is defined as relating to organizational data warehouses. As such, a defined research task exists, one which can be soundly based in existing knowledge transfer theory. The feasibility of the research will be immediately tested via the case study methodology. This methodology also lends itself to proving the timeliness of the research task in that an organization is participating, demonstrating receptivity and uniqueness. This professional organizational alignment also ensures that there are available resources to implement the research contribution and provides a continued and immediate relevancy feedback loop. This mechanism will serve
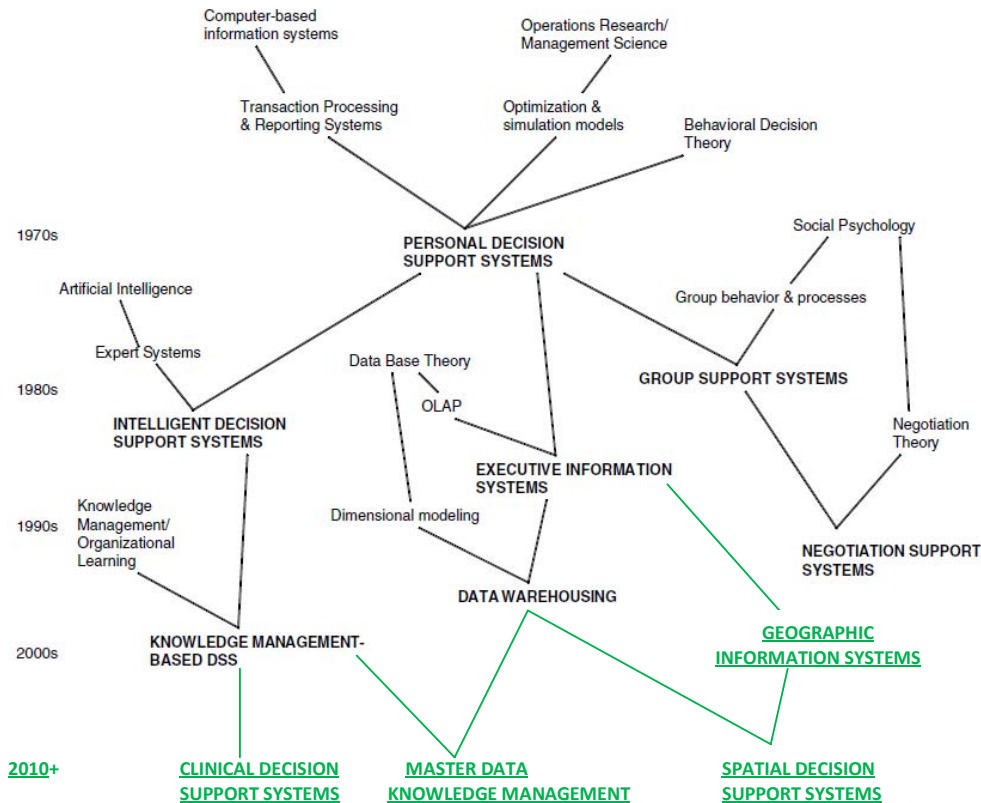

Figure 4. Proposed Extension of Arnott & Pervan's Evolution of the DSS Field

Research Relevance Framework the proposed sub-field of Organizational Knowledge DSS will be evaluated. To effectively do so, we will describe a research task currently being undertaken. The task can be described as understanding the critical success factors of tacit and

to ensure that any proposed artifacts are indeed practical to implement from a financial and human capital perspective. Finally, the research task presents significant opportunity for competitive advantage. Enabling an organization to best leverage its data,

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especially that stored in large or disparate data warehouses, is indeed the foundational definition and purpose for DSS.

In evaluating the research task identified against the Research Relevance Framework it can be determined that the proposed research has a high likelihood of relevance. As such, the proposed framework would suggest that the research into the task commence.

6. Conclusion

Decision Support Systems have achieved a great deal in four decades of existence. From the early foundations in Management Information Systems to the diverse landscape of sub-fields that have since been derived, DSS is truly a cornerstone of IS. However, in order to remain critical to the future of the IS discipline, researchers must make the establishment of relevant research their number one priority. DSS exists upon the very foundation of enabling the professional community through fast and flexible decision making solutions. To ignore these roots would be indeed a recipe for a continued disconnect between academia and professional disciplines, and would be the demise of DSS.

The authors' proposed Research Relevance Framework is a tool which recognizes that a research task must be evaluated against a set of comprehensive criterion to ensure relevancy beyond that of an individual researcher's perspective. As such, the proposed framework focuses heavily upon the lessons learned from the literature reviews conducted by highly respected researchers and contributors to the field. A renewed purpose towards relevant research and the achievement of a healthy balance of task and goal will ensure that academic and professional communities can once again regain credibility in DSS.

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