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Research article

Creating bigger problems: grand challenges as boundary objects and the legitimacy of the information systems field

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Abstract
The impact of a discipline's research is constrained by its ability to articulate compelling problems. Well-crafted problems are the foundation for mobilizing the effort, resources, and attention essential to scientific progress and broader impact. We argue that Information Systems (IS) scholars, individually and collectively, must develop the practice of articulating and engaging large-scale, broad scope problems – or grand challenges. To support this position, we examine the role and value of grand challenge efforts in science and engineering based on a theory of grand challenges as socially constructed boundary objects. Conceptualizing grand challenges in these terms implies strategies and approaches for magnifying the impact of IS research by engaging these types of problems.
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Introduction

As new information and communication technologies (ICT) have become available, many previously intractable and complex challenges have been addressed. We have mapped the human genome, created computers that play championship chess and Jeopardy, and built structures able to withstand earthquakes. These same ICT-based tools are enabling scientists to confront a growing number of grand challenges, significant large-scale and complex scientific challenges that push the boundaries and capabilities of existing disciplines and communities.

Engaging grand challenges helps mobilize requisite resources, provide legitimacy, and focuses economic, social, and research activities. While the importance of problem selection in shaping the collective identity of the Information Systems (IS) research community has been noted (King and Lyytinen, 2006), the grand challenge concept is generally absent from IS scholarship. Instead, we have historically focused more narrowly on research agendas defined by the ability of individual scholars to address issues arising from surveys of corporate executives and business consultants (Brancheau *et al*., 1996; Luftman and Kempaiah, 2009). While various policymaking groups have described critical challenges related to computing (Computing Research Association, 2003), the scholars in IS have neither articulated cognizant research grand challenges nor have they yet undertaken collective projects of the size and scope of the grand challenges that are often the focus of other disciplines.

This is not to say that IS scholars do not consider complex, socially relevant problems. However, and even when collaborative work is undertaken, this is typically of limited scale involving no more than a few researchers. As a result, IS researchers are particularly prone to following fashions (not setting them) and legitimacy threats (instead of creating legitimating goals) (Baskerville and Myers, 2009).

It is our contention that IS research is undervalued, at least in part, because as a community we fail to engage the full range and scale of problems to which our work and knowledge is relevant. Rather than scoping problems for study that are familiar and tractable, we must develop the ability to seek out and engage critical problems, even when they are unfamiliar and significantly exceed the capabilities of any one individual or research team. The purpose of this essay is to prompt greater appreciation of the desirability of incorporating the tradition and practice of engaging grand challenges into IS. We begin by considering the value of grand challenge efforts in science and engineering and the applicability of these practices to IS scholarship. We argue that grand challenges can be thought of as boundary objects and conclude that IS scholars, individually and collectively, should develop the practice of articulating and engaging grand challenges. Implications for IS researchers and the legitimacy of the IS community as a whole are discussed and we present an agenda for improving our ability to craft and engage in grand challenge efforts.

Grand challenges

In contrast to science based on accretion through incremental additions of individual research studies, grand challenge efforts seek to drastically alter the boundaries of existing knowledge, established disciplines, and available capabilities: they become goals. Efforts to engage grand challenges require cooperation and interaction between groups with differing perspectives over years and decades. New norms, structures, and practices must be developed to provide the support and incentives necessary to sustain these long-term, large-scale collaborative efforts. Addressing grand challenges also requires mobilization of substantial resources and significant participation from members of many relevant academic, practitioner, and policy-oriented communities. To justify this level of investment and effort, grand challenges must be perceived as having the potential to significantly impact not only multiple academic fields but also community, national, or international concerns such as competitiveness, security, economic development, or well-being.

For example, in the mid-1980s scholars in the broad field of biology began to advance the challenge of mapping the human genome (Oakridge National Labs, 2008). The idea was first seriously discussed at a meeting of top genetics researchers convened in 1985 as a potential biology challenge that would be equivalent in size to physics and astronomy projects such as building the largest telescope or a linear accelerator. The group determined that the idea was bold and exciting, but infeasible and possibly dangerous. Some found it absurd and could not imagine why anyone would want a complete map of the human genome since most of it would be composed of junk DNA that did not do anything. However, a couple of the attendees were captivated with the idea and began building support for it. Initially, the Department of Energy (DOE) championed the project as a means of tracking mutations caused by radiation, even though it required a significant departure from the usual norms of the biology research community: it represented technology development rather than hypothesis-driven research, required large-scale coordinated efforts rather than single investigator endeavors and the estimated $3 billion cost could come at the expense of other biology research.

By 1988, sufficient support had been developed that a National Research Council panel endorsed the project and the National Institutes of Health (NIH) became the lead agency through a memo of understanding with DOE. With NIH involvement, the project began to capture the public imagination with its promise of identifying the genetic basis of diseases and developing new therapeutic treatments. Mapping the human genome would 'save children's lives' by assembling 'the book of life.' It was to rely on new sources of funding, take a phased approach involving maps of chromosomes and studies of simpler organisms, and be completed in 15 years.

In 1990, NIH and DOE created a 5-year plan to map the human genome. Five major research centers were chosen and organized to engage in big science, while coordinating with partner research centers. Bottom-up decisionmaking processes involving peer-review, advisory councils, and topic-specific workshops were put into place. Project management and quality control measures were instituted. Communication norms were developed including periodic face-to-face meetings of all 20 centers and weekly conference calls between the five largest centers to share advances in automation, experimental protocols, and computational analysis in a 'lab meeting' format. The next year, a data repository for human chromosome mapping was established and norms for release of data and materials within 6 months of their creation were established.

In that same year, Craig Venter (a key member of the NIH team) left to join a new private sector start-up called Celera Genomic that would use a different technique (whole-genome shotgun sequencing) and patent the genes it mapped, putting the company in competition with the NIH/DOE-backed efforts. Advances in technologies enabled faster progress than originally thought possible. The 1990 5-year plan was revised in 1993 and a data release policy was established in 1996 that called for availability within 24 h of discovery. A new 5-year plan was released in 1998 and the first human chromosome was completely sequenced in 1999. NIH and Celera released a rough draft of the human genome with simultaneous publication in 2001and the project was declared complete in 2003.

Altogether mapping the human genome was a 20-year international effort by researchers in six countries at 20 centers, which cost over US$4 billion. The effort developed from a peripheral project, considered by some to be absurd and impossible that quickly rose to a position of prominence as critical infrastructure. The effort was enabled by significant improvements in computational power, sophisticated software, new data handling techniques, modeling, and visualization. It required a transgressive reorganization of the norms of biological research and has had enormous and wide-ranging impact both within science and throughout society as a whole. Ultimately, the effort to map the human genome enabled the creation of entire fields such as bioinformatics, proteomics, epigenetics, and biological models of gene function. Our improved understanding of the human genome has opened up such areas of research as evolutionary biology, forensics, environment influences in gene expression, and population genetics with its promise of personalized medicine (Collins, Morgan and Patrinos, 2003).

While large-scale, interdisciplinary approaches to science have become significantly more pervasive in recent decades, there is a long history of science and scientists engaging and organizing around grand challenges (Table 1). Artificial intelligence techniques, technologies and theories developed out of a decades-long effort to replicate aspects of human cognition and behavior. Current large-scale scientific endeavors building on the human genome map include the $50 million NSF-funded iPlant grand challenge project to map the tree of life for all green plants and to relate

Table 1 Example grand challenges

Description	*Articulation*	*Outcome*
Calculate Longitude	British Parliament (1714)	Development of sextants, significant advances in clock design, and astronomical maps made during the 1700s and 1800s
Fermat's Last Theorem	Fermat (1637) French Academy of Sciences (1816 & 1850), Academy of Brussels (1883), Gottengen Academy of Sciences (1908)	Wiles general proof, 1995
Chess Playing Computer	IBM and AI researchers (1950s)	Advances in AI and Human Cognition Deep Blue defeated Kasparov in 1997
Achieve Cold Fusion	Nuclear Physicists, in the 1950s	
Global Malaria Eradication	World Health Organization (1955–1969)	DDT residual spraying, Program abandoned in 1969
Grand Unified Field Theory	Einstein, Glashow, Georgi	Theoretical: Quantum Gravity, String Theory, Loop Quantum Gravity; Empirical: Large Hadron Collider and search for Higgs Boson
Land a Man on the Moon	Kennedy (1961) and NASA	Apollo moon landings (1969–1972); affiliated technology developments in avionics, telecommunication and computers
Eliminate Small Pox	World Health Organization (1967)	Widespread vaccination; last wild case documented in 1979
Cure Cancer	Nixon, National Cancer Act (1971)	
Map the Human Genome	Biologists, DOE, NIH (1985–2006)	All human genomes mapped 2006

genotype and phenotype, the NIH-funded $115 million ENCODE functional genomics project, the 11-organization public-private structural biology consortium, and the $138 million 6-nation haplotype map of genetic variation, and DOE's genomes to life project focusing on microbes. The problem statements that focus grand challenge efforts provide a basis for legitimizing research efforts, mobilizing the necessary resources, motivating action, and enabling cooperation among otherwise disparate parties.

Although grand challenges focus efforts and mobilize resources, not all can be considered unequivocal successes. Some grand challenges have been met. The US put a man on the moon within 10 years; the human genome was mapped by 2006. In contrast, the effort to cure cancer within 10 years began in the 1970s and a cure still has not been found. In high energy physics, the Large Hadron Collider is the latest in a series of efforts to gather empirical evidence to advance the search for a Grand Unified Field Theory. In the extreme, failure to achieve a grand challenge can lead to embarrassment, raise questions about competence, threaten legitimacy, and result in reduced access to resources. Pons and Fleischmann's claim of cold fusion in 1989 called into question their reputations and resulted in a view of the entire domain of cold fusion as 'junk science.' In addition, grand challenge efforts can be tainted by their unintended negative consequences. One example is synthetic pesticides like dichlorodiphenyltrichloroethane (DDT). The substance, DDT, was a great leap forward in the control of malaria and other mosquito-borne illnesses, but devastated many bird species. Its potentially catastrophic effects were characterized as a 'silent spring' (Carson, 1962) in a book that catalyzed the environmental movement. These unintended and unforeseen consequences of technical advances can raise questions about the value of science in general and erode public support for scientific endeavors.

In spite of the disadvantages, many areas of science, engineering, and technology policy have adopted the practice of articulating and engaging grand challenges. These communities have a tradition of articulating and publicizing their grand challenges, which are then used in coordinated efforts to mobilize resources (Hoare and Milner, 2005). Grand challenges were the focus of a US policy in the early 1990s related to high performance computing (HPC), the subject of a successful NSF program in HPC, and organizing framework for an ongoing effort by the UK Computer Research Committee. Identifying and articulating grand challenges is usually a collective effort. Thought leaders within a field collaborate to identify a set of grand challenges that both build on and stretch the capabilities of the field. Committees are formed. Studies are carried out to identify and document grand challenges that are seen as relevant. Regular grand challenge progress reports are written, which are then widely publicized among research and funding communities (see, e.g., those prepared by the National Academy of Engineering (http://www.engineeringchallenges.org/), American Association for the Advancement of Science (http://cstsp.aaas.org/content.html?contentid = 1230), National Ecological Observatory Network (http://www.neoninc.org/science/strategy). Successful grand challenges form target areas for funding agencies, research programs or centers, and political bodies. They can also serve as touchpoints for outsiders seeking to engage the research community and as such feature prominently in interdisciplinary relationships, national and international discourse, and popular media coverage of the value and impact of academic research.

Central to the discussion of grand challenge is a question of who defines the problems that will be the focus of a research community's efforts. Computing needs are often part of discussion of engineering and science grand challenges (e.g., see the National Academy of Engineering's list at: (http://www.engineeringchallenges.org/), and in some cases software and hardware needs are themselves the focus of grand challenge efforts (Fuller and Millett, 2011).

In contrast much of IS research activity is organized around surveys of the interests of corporate executives and business consultants (Brancheau *et al.*, 1996; Gomolski *et al.*, 2001; Luftman and Kempaiah, 2009). IS researchers largely cede the vision work, and the power, of defining their focal problems to a narrow group of external stakeholders.

Although the general idea of grand challenge problems is part of the language and practice of modern science and engineering, the IS community has not yet adopted this terminology or tradition. The question is: Should we? Some believe it is imperative that IS scholars identify a distinctive construct or conceptual focus if it is to maintain legitimacy and institutional support (Benbasat and Zmud, 2003). Others have argued that rather than seeking agreement on method or a core concept, the IS community is best distinguished and sustained by the nature of the problems that we engage (Lyytinen and King, 2004). A related discourse on fads and fashions in IS research and practice emphasizes understanding what we study and why we study it (Baskerville and Myers, 2009; Wang, 2010). It is through the problems that the activities of researchers are motivated, situated, interpreted, and evaluated (Swales, 1990, 2004). The selected problems shape the activities, resources, impact, and legitimacy of a field as a whole. Hence, it is critical for IS scholars to proactively engage in problem 'articulation' – initiating and leading the discussion within the public sphere where problems of interest are defined and framed.

In addition to defining problems while framing individual studies, IS scholars must begin actively developing and pursuing grand challenges. While some IS researchers have engaged complex, societally relevant problems, more generally the larger community of IS scholars has not yet developed a tradition of collectively identifying, articulating, and advocating for problems of the scope and scale reflected in grand challenges. Since grand challenges are important mechanisms for acquiring resources, maintaining legitimacy, and engaging external participants, failing to articulate and pursue grand challenges will result in continued undervaluing of IS scholars' work within universities, organizations, and society as a whole.

Developing grand challenges is neither costless nor riskless. Significant time, energy, and attention are required; political and social capital must be put to use; financial and institutional resources must be allocated, and all of these done often at the expense of other activities. These costs will be incurred to develop problem statements and plans, some of which will not succeed. If the IS community is to engage in grand challenge efforts, it needs to do so thoughtfully – and collectively – to enhance the probability of success. Yet, in the absence of a tradition of grand challenges, IS scholars lack the concepts necessary to understand the nature of and develop grand challenge efforts. With the rest of this paper we describe how thinking about grand challenges as boundary objects can guide and inform efforts to identify, engage, and evaluate grand challenge efforts.

Boundary objects

By its very nature, scientific activity requires interactions among individuals from different social worlds. Researchers from different disciplines, university administrators, funding agencies, and even the individual subjects of study must all be engaged and involved. These parties inhabit different social worlds with their own perspectives, standards, norms, incentives, power structures, and infrastructures. Activities that are central to one social world may be unknown in another and development of a single shared understanding is often neither feasible nor desirable. Yet success requires that these diverse stakeholders cooperate, share resources, and coordinate activities.

Boundary objects play a pivotal role in managing the tensions that arise when diverse stakeholders are involved in cooperative scientific activities (Star and Griesemer, 1989). Effective boundary objects exist in multiple social worlds. They are structured and stable enough that they can be used within a particular social world to organize activities and support work practices, but are malleable enough that they can be used in other social worlds to organize and support a different set of activities and practices. Boundary objects provide an infrastructure for transferring outcomes of disparate activities across social worlds. Because they exist in both contexts in meaningful substantive ways, they simultaneously shape practice in both social worlds, and in doing so provide a necessary infrastructure for cooperation. Boundary objects support shared representations, cooperative development, legitimation, and transfer of knowledge, collaborative design, sharing of resources, and mobilization for action (Levinia and Vaast, 2005; Bergman *et al.*, 2007).

Boundary objects can be concrete material artifacts or abstract concepts. For example, the shared database of mapped genes is a boundary object for the parties involved in mapping the human genome. Agreed upon standards and controls over the types of items placed in the gene bank allows for common reference among parties, sharing of information, and even coordination of actions (such as data collection and analysis). Yet, samples that are retrieved from the repository, how they are used, evaluated, manipulated – indeed what they are – may be significantly different depending on the perspective, motivations, and needs of the different stakeholders. A record that, to the data collector, is a single concrete artifact with idiosyncratic characteristics and data collection 'story,' maybe, to a data analyst, one of a data set of millions, where distinctive characteristics are statistical noise. The same data from the perspective of a funder or an administrator are an instantiation of a more abstract construct such as a work product or a performance metric. Shared repositories are a type of boundary object, that by virtue of their existence in multiple social worlds, provide an infrastructure for supporting cooperation, while neither eliminating nor ignoring the fundamental differences between those worlds.

Boundary objects help resolve the tensions that exist when different stakeholders attempt to cooperate. However, the resolution enabled by boundary objects is not synonymous with consensus. By identifying 'lowest common denominators,' critical points of agreement, or shared surface referents, boundary objects provide a sufficient platform for cooperative action – but they do so without requiring the individuals involved to abandon the distinctive perspectives, positions, and practices of their 'base' social world. Boundary objects enable networks of 'alliances' – by supporting the formation and maintenance of cooperative relations across many social worlds. Boundary objects must be malleable enough to be adapted to multiple social worlds and to change over time. While boundary objects are often talked about as bridging a pair of social worlds, they are most powerful and impactful when they are enabling complex networks of actors to cooperate at a nexus of multiple social worlds.

Grand challenges as boundary objects

Conceptual boundary objects play a crucial role in the success of large-scale scientific efforts that persist over long time frames. In their study of ecologists, Star and Griesemer (1989) noted that, 'Many participants share a common goal: preserve California's nature.' Yet more than just a shared goal, the common challenge is itself a critical part of the infrastructure underlying the scientific endeavor. Grand challenge boundary objects provide a basis for cooperation by organizing and motivating critical activities within the various social worlds. Effective grand challenges are enduring conceptual boundary objects that provide a critical infrastructure for managing the tensions between the need to maintain distinct, different social worlds while at the same time cooperating.

Thinking of grand challenges as boundary objects provides insight into whether a problem is likely to succeed as a grand challenge and why grand challenge efforts have significant consequences beyond those expected from more limited scientific endeavors. As boundary objects, a grand challenge exists in multiple social worlds simultaneously. While it may originate or be initially articulated in a single community, successful grand challenges are those that are plastic enough to be adopted in other communities in ways that reflect their priorities, practices, and power structures. For example, the challenge of mapping the human genome was adaptable enough to exist in multiple social worlds. For DOE it was a matter of assessing the mutagenic effects of radiation and justified maintenance of lab resources no longer required for nuclear arms testing; for NIH it was a matter of curing genetic diseases; for biologists it was a matter of acquiring resources equivalent to those garnered by other areas of science; for Celera Genomic it was a matter of patenting genes for profit; for Congress it was about saving the lives of children. In this way, the grand challenge of mapping the human genome served to provide a critical infrastructure that allowed the diverse parties to 'agree' on what they were doing, while simultaneously maintaining their distinct perspectives on the real value, and ultimate objective of the effort.

The value of grand challenges as boundary objects is further highlighted when the difficulty of sustaining initiatives over a long time frame is considered. As the example of the human genome project illustrates, garnering the resources needed to engage a grand challenge is a non-trivial task. At different times, different parties may be more or less interested. If the grand challenge lacks the capabilities of a boundary object it is likely to become isolated within a single, or small number of communities, and become vulnerable to shifting priorities of different stakeholders. On the other hand, if a grand challenge is constructed as a boundary object, its ability to be simultaneously stable and malleable allows for integration of new stakeholders with significantly different perspectives. The human genome project succeeded, at least in part, because it could involve forward-thinking biologists, DOE, NIH, private foundations, and private corporations, as the need and interest arose. Different parties became involved at different times, for significantly different reasons. The grand challenge of 'mapping the human genome' succeeded by providing a shared 'what' and a malleable 'why' that could be adapted as the underlying developed.

Another important aspect of grand challenge boundary objects is their ability to enable cooperation in the face of unresolved, multi-party disagreement. When a research problem is seen as a basis for 'identifying the common goal' there is the risk that it will cease being a platform for cooperation and become a mechanism for control and dominance. In contrast, an effective grand challenge enables complex networks of diverse stakeholders, as opposed to acting as a passage point or limiting gateway. By allowing for lightweight resolution, where parties agree on a problem and yet still use it differently within their own social worlds, a grand challenge enables long-term collaboration among parties who otherwise have strong reasons to maintain their independence. Grand challenges support high-impact work by allowing parties to cooperate without changing who they are and what they value.

Grand challenges are boundary objects. They provide structures that can be both shared among social worlds and used within each world to motivate, frame, and communicate research activities. They support involvement of diverse parties, change over time, and allow cooperation often without true consensus. By virtue of their role as a common goal a grand challenge can lead to path-breaking science. By virtue of their role as social infrastructure, grand challenge boundary objects can contribute to sustaining and legitimizing the communities and networks involved.

Grand challenge boundary objects, IS research, and legitimacy

Those of us in IS are aware that we are currently facing a number of serious issues regarding legitimacy, interest, and support. Wide swings in the number of students enrolled in IS programs lead to alternating faculty shortages and reductions in IS faculty positions. Scarce resources and the absence of broad-based institutional legitimacy have led to IS departments in some business schools being closed or merged into others. Scholars in the IS research community are distributed among diverse academic units and professional bodies (e.g. business schools, information schools, communications departments, computer science departments), attend different conferences, and publish in separate journals. The resulting research conversation is fragmented as multiple communities rediscover insights known elsewhere, constraining our ability to make significant progress. This internal fracturing is reflected in confusion among external stakeholders regarding the identity and *raison d'etre* of the IS community and the associated research.

At the same time IS has great potential for impact and growth. ICT is widely recognized as playing a central role in organizations, markets, communities, and society. IS scholarship has a history of drawing upon and working within many literatures. The IS community continues to embrace diverse methods, approaches, and theories. Institutional and interpersonal networks of IS scholars cross institutional, disciplinary, and national boundaries and the number of information schools, faculty, and students is steadily increasing.

In spite of these many strengths and opportunities, IS as a community continues to struggle with questions of legitimacy and support. No one doubts the importance of computing and communication technologies in the global economy. Employment opportunities for ICT professionals remain high and can be found worldwide. Calls for greater investment in technical education, cyberinfrastructure for science, and ICT for healthcare suggest that there is a general recognition that ICT are critical for many areas of life and society. Yet in spite of this, policy makers, funders, and others continue to find it difficult to see why IS research matters. The nature of grand challenges as boundary objects, capable of bridging gaps between alternative perspectives, implies that learning how to more effectively construct and engage grand challenges would help IS researchers and other interested stakeholders overcome many of these issues.

One strategy for engaging grand challenges is for the IS community to more deliberately contribute to solving problems that other communities have identified as grand challenges (see Table 2 for examples). There are numerous ways in which IS research, concepts, and knowledge can contribute to these grand challenge efforts. Knowledge management, collaborative systems and virtual teams, software development, data management, standards creation, infrastructure deployment and management, IS planning and assessment, and management of users responses to technology are just a few of the fundamentally difficult issues that arise as ICT is brought to bear on these challenges – issues that are central concerns within IS. Efforts to address any of the grand challenges listed above (Table 2) will benefit significantly if IS researchers become active participants. At the same time, becoming a participant allows both individuals and the scholarly community as a whole to increase their visibility, impact, and legitimacy.

Yet actively engaging these grand challenges must be not just a matter of 'applying' IS knowledge. While there are aspects of these projects that are best addressed with standard IS management practices, their scale, scope, and complexity also result in new ICT design and management issues that require greater intellectual efforts to address. Examining IS issues in new contexts challenges us to discover our limitations and develop our knowledge, concepts, theories, and methods. Engaging grand challenge efforts that originate in other domains provides an exciting opportunity for this type of boundary-expanding work.

Engaging a grand challenge in a meaningful fashion requires more than just single individuals joining relevant cross-disciplinary teams. If individual IS researchers join grand challenge projects as technology design advisors, project managers, or administrative consultants, they run the risk of being both overwhelmed and marginalized. Pursuing grand challenges in this purely decentralized manner is also likely to result in further fragmentation of IS research, and perhaps to its eventual depopulation as IS researchers become biomedical-informaticists, education informaticists, etc. ('Will the last person out of IS please turn off the lights?'). Collectively engaging grand challenges involves accepting the idea of each grand challenge as boundary objects that are themselves worthy of sustained attention. To do this, IS scholars and students must assign meaning and value to the challenge, moving beyond its articulation by the originating field and appropriating it into IS. This approach requires careful consideration of how the activities associated with addressing the challenge mesh with the central practices, priorities, and structures of IS. This takes work, is risky, and can be uncomfortable; but it is critical if we, individually and as a community, are to derive the full benefits of engaging grand challenges.

Another strategy is for the IS research community would be to generate its own grand challenges. Although IS has focused on many problems over the past four decades, few, if any, have risen to the level of grand challenges. A cursory review of the literature shows there have been numerous calls for IS researchers to consider different issues and problems. Most commonly these lead to a small number of uncoordinated studies, if they are responded to at all. In a few cases, such as resolving the productivity paradox, theorizing the ICT artifact, and managing outsourcing, the calls have resulted in significant attention within the IS community, but failed to engage external stakeholder in any significant fashion.

Grand challenges, as opposed to incremental research efforts, are difficult to solve, require major advances in knowledge, and hence demand significant improvements in research and/or organizational capabilities. They require large collaborative efforts with participation from many communities. Grand challenge efforts often require cooperation between scholars of different disciplinary backgrounds and at different geographic locations. They must be, at least theoretically, solvable and structurable in ways that provide indicators of progress toward a solution. Succeeding at a grand challenge represents a major milestone for an academic field or for society. Grand challenge efforts require sustained effort over a long time frame with solutions expected to emerge over decades. They are perceived by a range of parties to be worth solving, with the expectation that the solution

Table 2 Grand challenge opportunities for information systems scholars

National Association of Engineers – Grand Challenges for Engineering (http://www.engineeringchallenges.org/)	Make solar energy economical Provide energy from fusion Develop carbon sequestration methods Manage the nitrogen cycle Provide access to clean water Advance health informatics Engineer better medicines Reverse-engineer the brain Prevent nuclear terror Secure cyberspace Restore and improve urban infrastructure Enhance virtual reality Advance personalized learning Engineer the tools of scientific discovery
American Association for the Advancement of Science – Global Health Grand Challenges (Varmus *et al.*, 2003)	Improve childhood vaccines Create new vaccines Control insects that transmit disease agents Improve nutrition Improve drug treatment of infectious diseases Cure latent and chronic infection Measure health status accurately and economically in developing nations
UN Development Goals for 2015 (http://www.un.org/millenniumgoals/)	Eradicate extreme poverty and hunger Achieve universal primary education for boys and girls Promote gender equality and empower women Reduce child mortality rate before age 5 by two-thirds Improve maternal health and reduce mortality rate by 75% Combat HIV/AIDS, malaria, and other diseases Ensure sustainability, increasing access to safe drinking water Develop a global partnership for development
National Science and Technology Council Subcommittee on Social, Behavioral and Economic Sciences Research Opportunities and Priorities (http://www.nsf.gov/sbe/prospectus_v10_3_17_09.pdf.)	Education Health Cooperation/Conflict Societal Resilience/Response to Threats Creativity/Innovation Energy/Environment

would have a significant impact on both academic fields and national or international concerns such as competitiveness, security, economy, or well-being.

Solving the previously articulated IS problems has not required major advances, large collaborative efforts, or sustained effort over long periods of time. Progress on these problems has not been measurable and they have rarely represented notable milestones for the field as a whole. Issuing a call for research, while useful, is not the same as developing a grand challenge that provides an organizing structure for large-scale collaborative efforts both within IS and with other areas. If IS is to achieve the legitimacy, recognition, and support that is hoped for, it must both proactively engage in problem articulation (Baskerville and Myers, 2009) and develop its collective ability to create and leverage grand challenge boundary objects.

Grand challenges crafted by and for IS scholars have the greatest potential for internal impact because they are more likely to mesh cleanly with the community's existing perspectives, priorities, and practices and are less likely to face competition from other stakeholders. For example, IS grand challenges focused on particular theoretical or empirical anomalies could help explain and predict the unintended consequences so often observed in IS research by developing fundamentally better models of emergence and collective action. Similarly, creating theories of ecologies of systems that account for cross-level and dynamic effects in complex systems of systems could have transformative impacts by providing a foundation for managing the increasing complexity inherent in tightly coupled, ICT enabled, global societies.

A second type of grand challenge that could be developed by the IS community are those associated with development of an infrastructure that enables transformation of the field itself. The original creation of ISWorld, a distributed, ICT-based knowledge archive of IS resource, with its goal of utilizing the emerging technologies of the Internet to facilitate IS scholarship was an attempt at this type of internally focused grand challenge. Alternatively, much like the map of the human genome, the IS community might undertake the challenge of designing and implementing large research-oriented data sets. For example, a large-scale effort to document the lived experience of ICT users across contexts and cultures could serve as a catalyst for fundamentally new ways of looking at how information technology affects well-being, relationships, and society. To be effective, such a database would have to be longitudinal, multi-level, and combine both quantitative and qualitative data. It would need to capture information about people, data, behavior, and technologies. Whether focused on this phenomena or some other, developing large-scale data infrastructure would have the potential to extend our abilities, trigger new kinds of questions, prompt development of new methodologies, and encourage IS researchers to learn new ways of doing research. More than just collecting large amounts of data, efforts to create transformative infrastructure would bring together diverse parties to cooperatively engage in new kinds of research and encourage external parties to see the IS in a new way.

Whether core research challenges or infrastructure challenges, it is important that grand challenges articulated within the IS community be constructed as grand challenge boundary objects. Not only must they be consistent with the perspectives, priorities, and practices of the IS community, they must be malleable enough that they can be appropriated by stakeholders who inhabit other social worlds. University administrators and funding agencies should be able to use their support of IS grand challenge efforts to demonstrate that their investment decisions further their overall mission. Individual researchers, both within the field and in other areas, should be able to use the grand challenges to motivate and frame their own work. Subjects of the research must be able to use grand challenge statements to understand the significance of their participation. IS students should be able to envision the work they might do and the impact they could have. A well-crafted grand challenge will provide both common referents and a framework that diverse stakeholders can adapt for their own purposes. Developing this aspect of a grand challenge, like any other design problem, is iterative, messy, and time consuming. Yet, it is this type of intellectual work that is central to any effort to construct high impact grand challenge efforts.

This essay should not be seen as a call for the IS community to identify a single Grand Challenge. Indeed, areas of science generally articulate and engage many potential grand challenges precisely because these efforts cannot act as effective boundary objects without some ambiguity and multiplicity. Recognition of effective grand challenges as boundary objects highlights both the disadvantages and folly of seeking consensus on a single problem or issue. On the other hand, a suite of grand challenges is far more likely to strengthen a field, increase its legitimacy, and magnify the impact of its members' work. Multiple grand challenges reduce the risks associated with failure of any particular effort, increase the likelihood of forming dense networks within a diverse collection of stakeholders, and do so without requiring individuals, either within or outside the field, to reach true consensus on all matters of methodology or motivation. Lastly, each grand challenge provides both stability and the ability to be reinterpreted and repurposed as the priorities of stakeholders change. The grand challenges described here, and others like them, present IS researchers with valuable opportunities to simultaneously address substantive issues and strengthen the field as a whole.

Crafting grand challenges – needs and next steps

The importance of grand challenges and their potential as solutions to the legitimacy and support issues facing the IS community suggest that there is a need to systematically examine how current practices and structures affect efforts to craft and engage grand challenges. While studies of citation networks, reference disciplines, fads, and fashions in IS provide insight, theorizing grand challenges as boundary objects highlights the designed nature of research problems in general and of grand challenges in particular. This raises important questions about how researchers, individually and collectively, develop problems. Studies of the rhetoric of science (see Swales (1990, 2004) for a review) can help guide this effort, having already demonstrated how the rhetorical elements of individual papers, presentations, and literature streams function to support the development of problems that can serve as the basis for mobilizing a community of inquiry.

Another implication of theorizing grand challenges as boundary objects is that IS must learn how to more effectively 'export' problems. Discussions of the impacts of IS research typically focus on theories and findings as the means of influence. IS scholars develop insightful theory and generalizable facts, which are in turn adopted by other disciplines. However, if grand challenges are effective boundary objects, then a well-constructed challenge can also be an influential and oft-cited contribution to scientific inquiry. Yet, the current structure of the IS scholarly community provides scant opportunity to publish work that seeks to substantiate particular problems. Empirical evidence can be valuable for characterizing the nature and consequences of not addressing a grand challenge. Conceptual efforts that examine the nature of the challenge can be critical for connecting it to both the priorities of the IS community and to the priorities and practices of the other likely partner areas. Providing outlets and recognition for this type of problem development work would support and enhance the process of developing effective grand challenges.

A grand challenge is a conceptual artifact that is appropriated by different parties and incorporated into their priorities, practices, and perspectives. Engaging a grand challenge involves individuals both participating in the associated trans-disciplinary community of inquiry and undertaking the work to appropriate the challenge into their home field. However, the true benefits of engaging grand challenges flow from collective action. Simply having idiosyncratic individuals participating in grand challenge efforts is insufficient. Whether a grand challenge originates from within or outside the discipline, collectively engaging it requires formation of a sustained community of inquiry within the field. The more developed a field's internal grand challenge community of inquiry, the more likely it is that the work of collectively engaging the grand challenge will be performed and the more visible that work will be to the other stakeholders. This suggests that, rather than the ability to find and disseminate solutions in response to the interests of groups such as corporate executives or business consultants, the limiting factor on the IS community's legitimacy and impact may be its collective ability (or inability) to form substantial communities of inquiry in a timely fashion. Articulating big, meaningful problems in ways that attract sustained attention from diverse groups of scholars is just as important for advancing a field as conducting studies that resolve and eliminate questions. Without grand challenges and the associated communities of inquiry, the knowledge and collective of ability of a discipline is doomed to remain latent, and largely invisible to broader intellectual and social community around it.

Developing and addressing grand challenges is complex knowledge work undertaken by large communities with participants drawn from diverse, often conflicting, social worlds. Information technology plays a variety of roles in these efforts. The cyberinfrastructure of grand challenge efforts includes, but is not limited to, computing systems; data standards, storage, and management tools; information and knowledge resources; networking; digitally enabled sensors and instruments; virtual teams and organizations. Whether as tools for addressing grand challenges or the infrastructure for enabling the associated communities of inquiry to function, IS are central to all grand challenge efforts. At a mundane level, grand challenge efforts are faced with a myriad of problems that are both familiar and fascinating for IS researchers: data, information, and knowledge management; managing investments in infrastructure; supporting collaborative work; articulating requirements, creating specifications, and dealing with the realized systems. At the same time addressing the requirements of and supporting grand challenge efforts raise new issues that if examined will expand our understanding, knowledge, and ability to derive value, broadly defined, from the deployment of IS.

Together these assertions and research questions represent a call for an empirically grounded design theory of both grand challenges and the socio-technical systems around them. What are the specific features/characteristics of effective grand challenges (and of grand challenge creation processes)? How can ICT facilitate efforts to engage a grand challenge? This question itself has the hallmarks of an effective grand challenge in that answering it can serve the ends of several different stakeholders, stretch existing boundaries, and if addressed might fundamentally transform how research is performed. To IS researchers the goal of constructing a design theory for grand challenge cyberinfrastructure can be a practical problem with immediate personal and collective consequences or an opportunity to examine how ICT and people interact in complex socio-technical systems. To researchers in areas such as engineering, biology, or chemistry, the design of infrastructure for engaging grand challenges is likely to be seen through the lens of the problems and issues they face as they engage the grand challenges that are central to their disciplines and research. For funding agencies and university administrators, such a design theory would be an important tool for ensuring that their investments in infrastructure and personnel are directed to the points of greatest impact (hence the willingness of entities such as the National Science Foundation to fund this type of research). While not assured, efforts to study, characterize, and inform the development and engagement of grand challenges have the potential to contribute far beyond the traditional boundaries of IS.

Although it is tempting to think otherwise, it is critical to remember that research problems are designed artifacts, not found objects. Individual researchers craft problems to favorably position their work in relation to other studies known to their audience. Grand challenges are the special class of problems that, by virtue of what they are and how they are appropriated, serve as a platform for cooperation among different social worlds. Whether large or small, externally or internally articulated, research problems are constructed not discovered. Relying on surveys of external stakeholders to define problems cedes the power to shape critical elements of a discipline's intellectual infrastructure to outsiders who have neither the ability nor the inclination to support a field's success and survival.

Problems, and more specifically grand challenges, matter. Successful creation of a suite of grand challenge problems can serve to place IS research in a stronger position within a network of partners, a position that can provide legitimacy, resource flows, and streams of new students, faculties, and other interested participants. Yet, creation of effective grand challenges is non-trivial. It requires not only resources and attention, but also an understanding of how grand challenges function as boundary objects that support cooperation among diverse stakeholders. Recognizing and supporting efforts by individuals to both articulate and gather evidence in support of particular grand challenge efforts is critical.

While they are not the only means for bolstering legitimacy, mobilizing support, and acquiring necessary resources, effective grand challenges are a powerful tool for achieving these goals. IS researchers, both individually and collectively, would benefit significantly from greater support of efforts to develop, articulate, and leverage grand challenges. Although our strength as a community may be problem solving, it would be to our advantage, and ultimately to that of others, if we also became expert problem creators, able to develop, appropriate, and direct attention to problems on a scale, scope, and time frame much greater than we have in the past.

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