REVISITING THE PROBLEM OF THE PROBLEM – AN ONTOLOGY AND FRAMEWORK FOR PROBLEM ASSESSMENT IN IS RESEARCH

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REVISITING THE PROBLEM OF THE PROBLEM –
AN ONTOLOGY AND FRAMEWORK FOR PROBLEM
ASSESSMENT IN IS RESEARCH

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Abstract
A comprehensive understanding of how to achieve relevance and practical impact with our work remains elusive within the information systems (IS) community. While we know that finding or constructing important research problems sets the bar for the potential impact that research can have, we know little about how to support research problem assessment and selection in practice. This paper address this gap by presenting the problem assessment framework (PAF) and outlining its application for the assessment, selection, and justification of important research problems. The PAF builds on the problem assessment ontology, which explicates the domain of problem assessment based on a synthesis of extant research. We have instantiated the PAF in the problem assessment canvas to make it more accessible. Altogether, we contribute three novel artifacts that support researchers looking to work on the most important research problems as the basis for more relevant and impactful IS research.

Keywords: Relevance, Rigor, Problem Assessment, Problem Selection, Ontology, Design Science Research.

1 Introduction

The quality, relevance and practical impact of information systems (IS) research has been a prime concern for the IS research community since at least Peter Keen’s (1991) seminal call to focus on the topic. Keen (1991) proposed that relevance and a focus on practical impact must drive IS research because purposive and impactful research is a desire that is shared by the majority of researchers that identify with the IS research community. Thus, “until relevance is established, rigor is irrelevant” (Keen, 1991, p. 27). Since Keen’s (1991) work, many debates have been undertaken to advance and reflect upon the topic of relevance and practical impact in IS research (Applegate, 1999; Agarwal and Lucas Jr, 2005; Desouza et al., 2006; Wiener et al., 2018). Yet, as recent contributions in major IS journals demonstrate (e.g., Nunamaker, Briggs, Derrick and Schwabe, 2015; Nunamaker, Twyman, Giboney and Briggs, 2017; Davison and Bjørn-Andersen, 2019; Gable, 2020; Mohajeri, Mesgari and Lee, 2020; Pan and Pee, 2020), a comprehensive understanding of how to achieve relevance and practical impact remains elusive within the IS community.

Against this backdrop, this paper builds on and contributes to the ongoing debate about relevance and practical impact in IS research, by zooming in on the role of research problems. It is generally agreed that finding or constructing an important research problem (whether this happens in a planned or unplanned manner) is a necessary precursor before any relevant and impactful research can take place (Getzels, 1975; Weber, 2003; Van de Ven, 2007; Rai, 2017). Nevertheless, extant research has high-

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lighted that there seems to be much confusion and little shared understanding on how to rigorously assess or effectively justify the value and importance of research problems (e.g., Hassan, 2014; Rai, 2017; Tremblay, VanderMeer and Beck, 2018; Weber, 2003; Wiener et al., 2018; Winter and Butler, 2011; Moeini, Rahrovani and Chan, 2019; Gable, 2020).

For instance, Weber (2003) already reflected that finding deep and important research problems often remains more of dark art rather than a systematic and rigorous endeavor. As recent investigations and discussions highlighted, not much seems to have changed in this regard (e.g., Tremblay et al., 2018; Wiener et al., 2018). Others, such as Winter and Butler (2011), highlight how important research problems can act as coordination mechanisms for impactful research programs but suggest that typical IS research problems do not fully live up to this potential. Nunamaker et al. (2017) detail pragmatic guidance on how this situation could be improved by focusing on systematic programs of high-impact research but remain largely silent on what characterizes high-impact research problems in the first place.

Most recently, Moeini et al. (2019) synthesized extant research into a framework to assess potential for practical relevance. Their work highlights the importance of research topic selection but also suggests a need for a better evidencing of problem importance and relevance in practice. Gable (2020) builds on these insights and forcefully advocates the value of being strategic in IS research. His proposal emphasizes the need for an explicit discourse on the strategic direction of the IS field and, thereby, acknowledges the importance of systematic problem assessment and selection.

Altogether, the given examples attest to the ongoing relevance of the problem of the problem: knowing that the selection of problems is important but not knowing enough about how to support the identification and selection of important problems. Thus, we heed prior calls to revisit the problem of the problem by investigating the following research question:

- How can we assess and select research problems as well as justify their importance and relevance in a more rigorous way?

We answer the research question by presenting the problem assessment framework (PAF) and demonstrate its applicability for the assessment, selection, and justification of important research problems. The PAF is built on the problem assessment ontology (PAO), which explicates the domain of problem assessment based on a synthesis of extant research. The PAF consists of four modules (sic., perspective, definition, barriers, importance) and nine related components that help to systematically characterize, assess, and compare research problems. We have also instantiated the PAF in a proof-of-concept visual inquiry tool (Avdiji, Elikan, Missonier and Pigneur, 2020), the so called problem assessment canvas (PAC) to make the PAF more accessible and improve its ease of use. Altogether, we contribute three novel artifacts that support researchers looking to work on the most important research problems as the basis for more relevant and impactful IS research.

The rest of the paper is structured as follows. First, we outline related work that has informed our work. Second, we explain our research approach. Third, we present the problem assessment framework. Fourth, we demonstrate the use of the framework and articulate guidelines for the selection, assessment, and justification of important research problems that can help to improve the relevance and impact of IS research. Fifth, we discuss the implications and limitations of our work. Sixth, we conclude the paper with a short summary and outline of future research directions.

### 2 Related Work

In the following sections, we describe related work that has informed our enterprise. In general, we build on insights and perspectives from three distinct research areas, namely, IS research, effective altruism, and social-ecological systems research.
2.1 Information Systems Research

As mentioned in the introduction, IS research has a long history of engaging with the question of relevance and impact of IS research (e.g., Keen, 1991; Applegate, 1999; Agarwal and Lucas Jr, 2005; Desouza et al., 2006; Wiener et al., 2018). However, even though it is widely acknowledged that finding or constructing an important research problem sets the bar for the potential impact that research can have (Getzels, 1975; Weber, 2003; Van de Ven, 2007; Rai, 2017), comparatively little IS research has examined the topic of research problem assessment and selection. In a laudable effort, Rai (2017) makes nine general suggestions to help with articulating “research questions that matter”. While we generally agree with the suggestions offered, we observed that they do not lend themselves to the assessment of the relative importance of research problems and, thus, have limited value for the comparative appraisal of research problems that is needed when deciding between different research options. Specifically, they do not help to answer the question of how important or relevant additional work on a research problem is expected to be. Thus, the work illustrates the need for more systematic engagement with the topic of research problem selection.

Extending this line of inquiry, Gable (2020) advocated the value of being strategic in IS research and having an explicit discourse on the strategic direction of the IS field. Thus, he goes beyond a focus on research problem selection as a challenge for individual IS research (as implicit in Rai, 2017) and follows Moeini et al. (2019) in proposing a multilevel perspective that also includes the perspectives of collectives such as systematic research programs and research communities. In particular, it is highlighted that systematic discourse and cumulative progress is needed to engage with research problems that are larger in scale and, thus, provide more opportunity for impact (Winter and Butler, 2011). However, as of yet it can be observed that the IS research community has not had a systematic focus on addressing the most important and relevant research problems (Moeini et al., 2019) but has rather been dependent on the preferences and expertise of individual researchers or research teams to initiate such efforts (Nunamaker et al., 2017).

Based on our engagement with the IS community, we propose that an important determinant for this situation is the fact that most discussion on the importance and relevance of research questions has been based upon only implicit assumptions about what those terms actually mean. This leaves much wiggle room for vague justifications of potential importance or relevance that are hard to evaluate because it is unclear what is actually claimed (e.g., Moeini et al., 2019). Moreover, because there is no explicit and shared understanding of importance and relevance in the IS research community, there are also no clear benchmarks that IS researchers or research teams could use to decide whether a specific problem is worthwhile to investigate. As a famous quote (often attributed to Lord Kelvin) says: “what is not measured cannot be improved”.

A related effort in this space is the recent work by Maedche et al. (2019) who have put forth a framework to conceptualize the problem space in design science research (DSR). While their framework seems useful to help formalize and ground discussions about problematic situations, it remains agnostic about the importance or relevance of problems. Thus, it cannot help to address the challenges outlined above.

2.2 Effective Altruism

The effective altruism (EA) community (MacAskill, 2015, 2019b) is an applied research community at the intersection of philosophy and economics that engages with the question of how to do as much good as possible given the resources that are available. Moreover, it aims to actively translate its research insights into practice to improve the world. Thus, the vision of EA is in important ways similar to the vision of the IS community that Peter Keen (1991) and others (e.g., Hevner, March, Park and Ram, 2004; Walsham, 2012) have outlined.

One important cornerstone of EA is the development and use of rigorous methods and tools that help to effectively prioritize the use of resources for the greatest altruistic benefit (MacAskill, 2015,
2019b). As part of this effort, EA is emphasizing the use of cost-effectiveness (CE) analyses (Garber and Phelps, 1997) to inform the allocation of available resources (MacAskill, 2015). Specifically, while CE analyses can be misleading if not interpreted carefully (Simcikas, 2019), they can help to assess the gains in a dependent variable (e.g., healthy lifeyears or any other quantity) that are expected to be gained by investing additional resources into a specific course of action (Garber and Phelps, 1997). Thus, they lend themselves to the assessment of a set of alternative options against a common benchmark (i.e., the same dependent variable). This allows for a systematic and cumulative approach to research as demonstrated by the field of development economics, which has been pioneering the use of CE analyses to generate important insights about the relative effectiveness of particular courses of actions (Olken, 2020).

Going beyond the traditional use of CE analyses in the retrospective evaluation of particular courses of actions, the EA community has started to develop ways of applying CE analyses prospectively to estimate the expected value of engaging with problems in the first place (e.g., Cotton-Barratt, 2014; Wiblin, 2017; Wiebe, 2019). For instance, Wiblin (2017) demonstrates how to use estimations of the importance, tractability, and neglectedness of problems to compare them in terms of potential impact of additional resource investments. Wiebe (2019) proposes an alternative model which only requires the estimation of importance and tractability by integrating considerations of neglectedness into them.

The main intent behind the use of such methods is to leverage the full potential of the EA community by facilitating the proactive coordination of its members around the worlds most pressing problems. CE analysis is often quite useful for this case because, in a world of entangled complex systems (Liu et al., 2007; Benbya, Nan, Tanriverdi and Yoo, 2020), huge (i.e., often multiple orders of magnitude) differences in CE between the best and the worst opportunities for action are likely to arise, as regularly observed in multiple domains of practice (e.g., Hattie, 2012; Ord, 2013). Thus, just having a general idea of the CE of a problem can already help to prioritize the best opportunities for action. In sum, we were inspired by the success of the EA community in applying prospective CE analyses and adapted as well as improved existing approaches for application in the IS research community.

2.3 Social-Ecological Systems Research

Social-ecological systems (SES) research is an interdisciplinary field that is concerned with the analyzing, comparing, and diagnosing of complex SES (e.g., Ostrom, 2009; Binder, Hinkel, Bots and Pahl-Wostl, 2013). SES are generally understood to be complex systems that are composed of multiple natural and artificial subsystems at multiple levels analogous to living organisms that are composed of cells, tissues, organs, etc. (Ostrom, 2009). Thus, a major challenge for SES research is the integration of work from a variety of perspectives and disciplines into a cohesive and cumulative body of knowledge.

A major progression in the field of SES research has been the development of the SES framework (Ostrom, 2009, 2010; Cox, 2014; McGinnis and Ostrom, 2014), which provides a conceptual lens through which SES can be comparatively studied. In particular, the SES framework organizes the analysis of SES around the core concept of action situation (Ostrom, 2010; McGinnis and Ostrom, 2014). An action situation describes focal situations or interactions that are repeatedly occurring and deemed interesting enough for study. Ostrom (2010) demonstrates how action situations can be formalized using the logic of game theory by specifying seven rule sets to describe the game dynamics that are inherent in all interactions. This formalization has proven invaluable for the highly cumulative and successful research program on sustainable governance arrangements for common pool resources (Ostrom, 2010). Although we do not explicitly use the action situation lens in our work as of yet, we were inspired by it and have developed our framework around the core idea of problems being linked to focal situations.
3 Research Approach

This project follows other methodological contributions in the IS field (Gregor and Hevner, 2013) in applying a design science research (DSR) approach to framework development. The online appendix to this paper (Herwix and Haj-Bolouri, 2021) provides a comprehensive description of the research process as well as a design justification. Thus, for the description in this section we rely on Hevner et al.'s (2004, p. 83) seven guidelines for DSR to outline our research approach.

First, our research produced several useful artifacts in the form of the PAO (a model), the PAF (a model), and the PAC (an instantiation).

Second, the problem we addressed is relevant to the IS research community because it currently does not have a systematic approach to assess research problems in terms of importance and relevance, which is an important gap as cumulative research around the most pressing issues is to arise. Specifically, it stands to reason that without a systematic discourse about the research problems we should be tackling, research efforts will not be systematically aimed at the most important and relevant topics, thus, diminishing the potential impact IS research could have.1

Third, the PAF has been formatively evaluated with expert feedback by senior IS scholars as well as practitioners experienced in problem assessment and is currently undergoing additional testing in an undergraduate university course. It is important to note that the PAF, PAO, and PAC should be considered as living artifacts that are meant to be used, extended, and adapted by the community to solve emergent needs. Thus, the traditional guideline of conducting rigorous summative evaluations (Venable, Pries-Heje and Baskerville, 2014) are less applicable to our case. Rather, it seems useful to consider the take up of the artifacts by the IS research community as the best evaluation of their utility.

Fourth, we contribute three artifacts to the IS research community that, at the same time, expatiate existing knowledge on problem assessment to IS research as well as improve upon this existing knowledge by making it more accessible to non-experts in problem assessment (Gregor and Hevner, 2013). In terms of Gregor and Hevner’s (2013) contribution types, they can be classified as level 1 (instantiation) and level 2 (nascent design theory) contributions.

Fifth, to ensure the rigor of our research approach, the development of the artifacts was informed by the design theory for visual inquiry tools (Avdiji et al., 2020), which provided a set of design principles that acted as a point of reference for our design activities. Although this design theory has been developed to help with the development of visual inquiry tools for strategic management problems in business, we propose that it is still applicable for the case of problem assessment in research as there is considerable overlap between strategic management problems in business and the problem of problem assessment and selection in research. For instance, both problems are complex in nature, concern the effective allocation of resources and often affect a team of people. Table 1 gives an overview of how we addressed the main design principles that Avdiji et al. (2020, p. 716) have articulated.

Sixth, our research approach followed an iterative search process. While developing the artifacts, we constantly moved back and forth between the literature and our personal experience from the field until we converged on a coherent design. For instance, the results presented in this paper are informed by several presentations of earlier versions of this work at research seminars, workshops, and a conference.

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1 We do not wish to imply that all IS research should necessarily follow a systematic or mechanistic approach to optimize problem selection but highlight that without a systematic discourse about what the most important and relevant topics are, we are obviously leaving potential impact on the table as it is highly unlikely that everyone would intuitively work out where to invest their resources to optimize for impact.
Seventh, we are using a variety of channels to communicate our research, for instance, by using it in teaching, presenting it at academic workshops and conferences as well as writing papers for academic and practitioners audiences. The PAC is licensend under a creative commons license and will be made available on the web to encourage use and adaptation. Professional development workshops to train IS researchers in the use of the artifacts are also being considered.

<table>
<thead>
<tr>
<th>Design Principles</th>
<th>Implementation</th>
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<tbody>
<tr>
<td><strong>DP1 Conceptual model:</strong> To structure the [...] problem, frame it with a conceptual model describing the relevant building blocks (components) of the problem. The conceptual framework should be modeled according to academic justificatory knowledge and be kept parsimonious.</td>
<td>We developed the PAO to frame the problem situation with a conceptual model. The PAO is informed by the practices of the applied research field called Effective Altruism (MacAskill, 2015, 2019b) and the Social-Ecological Systems (SES) framework (Binder et al., 2013; McGinnis and Ostrom, 2014).</td>
</tr>
<tr>
<td><strong>DP2 Shared visualization:</strong> To facilitate communication between users, represent the conceptual model as a shared visualization by structuring the components logically into a visual problem space.</td>
<td>The PAC provides a visualization of the PAF that can facilitate the communication between users. For instance, it provides nine empty problem spaces for each of the nine properties in the PAO. The properties are logically grouped and related in four modules that are visually highlighted. Textual prompts help to orient and direct users while filling out the canvas.</td>
</tr>
<tr>
<td><strong>DP3 Directions for use:</strong> Define and specify techniques that allow for joint inquiry.</td>
<td>This paper illustrates several use cases for the framework and how to apply it. Regarding the PAC, short prompts for each property provide instructions for how to think about the problem space.</td>
</tr>
</tbody>
</table>

Table 1. Overview of how this work implemented the main design principles articulated by Avdiji et al. (2020, p. 716).

### 4 The Problem Assessment Framework

The main goal of the problem assessment framework (PAF) is to provide a more rigorous foundation for the systematic assessment of research problems that is conducive to more relevant and impactful research. The PAF is grounded in the problem assessment ontology (PAO) which synthesizes extant research into a coherent perspective. Figure 1 shows how the PAO breaks up the domain of problem assessment into four entities (white boxes), three major relationships (labeled arrows), and nine properties (white ellipses). A description and justification for each element can be found in the online appendix to this paper (Herwix and Haj-Bolouri, 2021).

Reading from left to right, the PAO proposes that *actor* entities can be affected by a *problem* entity, which describes in some way unsatisfactory *situations* and frames potential *solution* entities. The PAF then uses four *modules* (shaded boxes) to organize the domain of problem assessment and unpack the mentioned entities and high-level relationships in more detail:

- A module that explicates the *perspective* that is taken when assessing the problem.
- A module that encourages consistent *definitions* of problems.
- A module that considers *barriers*, which could limit the impact of solutions.
- A module that assess the overall *importance* of a problem from a given perspective.
Figure 1. The problem assessment ontology describes the most relevant features that a systematic problem assessment should consider. The problem assessment framework groups those elements into four distinct but related modules.

4.1 Module 1: Perspective on the Problem

The first module describes that problems are always viewed from a specific perspective because problems are relational constructs that are defined in relation to who they affect (Maedche et al., 2019). A problem which will never affect anyone is simply not a problem. Moreover, different actors have different roles and priorities in the world and are, thus, likely to evaluate problems differently (Maedche et al., 2019). Thus, an important challenge in this regard is to find a way to characterize actors that is mindful of different roles and priorities but still allows for the recognition of regularities across contexts and time.

We propose that institutional logics (Seidel and Berente, 2013) provide a suitable abstraction for characterizing the perspectives of actors. In particular, institutional logics describe the ensemble of core goals, values, and prescriptions that are associated with general roles in organizations and society (Berente and Yoo, 2012). Thus, they represent an intersubjective or societal perspective on appropriate goals, values, and behaviors for an actor in a given role (Berente and Yoo, 2012). Importantly, actors are often exposed to a multiplicity of – sometimes conflicting – institutional logics that they are expected to draw upon and reconcile as part of their practice (Berente and Yoo, 2012). In sum, using institutional logics can help us to aggregate micro-level behavior into higher-level constructs that are applicable across a wider range of contexts (Seidel and Berente, 2013). Moreover, taking the perspectives of a variety of institutional logics can help to uncover conflicting institutional logics as sources of problems or barriers to solutions.

For instance, when assessing IS research problems, academics and practitioners are generally acknowledged as relevant stakeholder groups (Keen, 1991). Both can be described with different institutional logics. First, we propose that the institutional logic of an academic highlights knowledge as a core value that should be optimized for (Klingbeil, Semrau, Ebers and Wilhelm, 2019). From this perspective problems become more relevant the more they pertain to the academic discourse and state of
the art. Second, we propose that the institutional logic of a practitioner highlights satisfaction with performance in their respective domain of expertise as a core value that should be optimized for (Simon, 1955; Berente and Yoo, 2012). From this perspective problems become more relevant the more they affect the state of the art of the profession of the practitioner. These institutional logics can be complementary if, for instance, a practitioner values academic knowledge as a contributor to her excellence. They can even be held simultaneously if an academic understands herself to be a practitioner or a practitioner takes on the role of an academic (e.g., as part of a PhD program). However, they can also be in conflict if practitioners do not perceive the academic knowledge pursued by academics to be valuable to their practice (Rosemann and Vessey, 2008).

Furthermore, going beyond the traditional confines of IS research being focused on academics and practitioners, it is increasingly recognized that additional perspectives such as those of affected individuals or societal institutions ought to be considered (Davison and Bjorn-Andersen, 2019). Institutional logics are a flexible framework that can be easily extended to accommodate multiple perspectives even across multiple levels of abstraction (Seidel and Berente, 2013). Thus, as described in Table 2, in addition to academic and practitioner, we suggest six additional institutional logics that we believe provide a comprehensive yet parsimonious overview of perspectives which could be considered for a holistic problem assessment. This list is derived from iterative discussion and reflection between the authors and should not be viewed as complete. It merely aims to provide a thought provoking starting point for the deliberate engagement with the question of what actually makes a problem matter.

<table>
<thead>
<tr>
<th>Institutional Logic</th>
<th>Core Value</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>Academic</td>
<td>Knowledge</td>
<td>A generic academic is generally interested in advancing knowledge.</td>
</tr>
<tr>
<td>Practitioner</td>
<td>Satisfaction</td>
<td>A generic practitioner is generally interested in achieving satisfaction with her performance.</td>
</tr>
<tr>
<td>Individual</td>
<td>Wellbeing</td>
<td>A generic individual is generally interested in personal wellbeing.</td>
</tr>
<tr>
<td>Organization</td>
<td>Value Realization</td>
<td>A generic organization is generally interested in realizing value for its members.</td>
</tr>
<tr>
<td>Business</td>
<td>Success</td>
<td>A generic business is generally interested in success within a competitive environment.</td>
</tr>
<tr>
<td>Government</td>
<td>Societal Welfare</td>
<td>A generic government is generally interested in managing and increasing societal welfare.</td>
</tr>
<tr>
<td>Civil Society</td>
<td>Justice</td>
<td>A generic civil society organization is generally interested in increasing justice in relation to a particular cause.</td>
</tr>
<tr>
<td>Future People</td>
<td>Sustainability</td>
<td>Future people are generally interested in the long-term sustainability of the human project.</td>
</tr>
</tbody>
</table>

Table 2. Proposed set of eight institutional logics for a holistic problem assessment.

### 4.2 Module 2: Definition of the Problem

The second module captures the definition of a problem through three complementary components, namely, context, situation, and boundary. Altogether they allow for the succinct definition of a problem and provide an anchor for the cumulative and comparative assessment of problems.

First, context clarifies important aspects of the environment of the problem (e.g., Davison and Martinsons, 2016). In particular, Table 3 details four general scopes of environment that we suggest provide a holistic but concise first order classification for problem contexts. As presented in more detail elsewhere (Herwix and Haji-Bolouri, 2020), this classification organizes problems in terms of the scope of the environment that they relate to, ranging from the universal to the local environment. In
general, the higher up a problem is in terms of scope, the more foundational the effect of solving it.\(^2\) Thus, this classification allows for a quick but systematic positioning of problems in the “grand scheme of things”.

<table>
<thead>
<tr>
<th>Scope</th>
<th>Focus</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>Universal</td>
<td>Value systems</td>
<td>The universal scope encompasses problems that engage with foundational</td>
</tr>
<tr>
<td></td>
<td></td>
<td>questions around the nature of the “goodness”, “badness” or “value” of</td>
</tr>
<tr>
<td></td>
<td></td>
<td>actions and their outcomes.</td>
</tr>
<tr>
<td>Global</td>
<td>Global priorities</td>
<td>The global scope encompasses problems that transcend particular domain</td>
</tr>
<tr>
<td></td>
<td></td>
<td>scopes and help to identify, understand, and improve global priorities.</td>
</tr>
<tr>
<td>Domain</td>
<td>Problem classes</td>
<td>The domain scope encompasses problems that persist across a range of local</td>
</tr>
<tr>
<td></td>
<td></td>
<td>contexts.</td>
</tr>
<tr>
<td>Local</td>
<td>Instantiations</td>
<td>The local scope encompasses problems that are idiosyncratic to the local</td>
</tr>
<tr>
<td></td>
<td></td>
<td>environment.</td>
</tr>
</tbody>
</table>

Table 3. Four general scopes of environment to classify problem contexts.

Second, *situation* describes the problem situation and should detail the reason why it ought to be tackled from the perspective selected in module 1. Being clear about what the problematic situation is and why it is important from the perspective of a given institutional logic is a precondition for an intersubjective assessment. Specifically, it should be possible to work on problem assessments collaboratively (Avdiji et al., 2020) as well as evaluate them through peer-review (Hecht et al., 2018). Theoretical lenses such as Maedche et al.’s (2019) conceptualization of the problem space or the analytic structure of the action situation from the SES framework (Ostrom, 2010) could be used to support a systematic description.

Third, *boundary* intends to further refine the problem definition through the explicit documentation of what is *not* considered to be part of the problem. The specification of clear boundaries has been recognized as a necessary component of rigorous academic work in IS research (Weber, 2012).

### 4.3 Module 3: Barriers to Solutions for the Problem

The third module prepares the overall assessment of the importance of working on a problem with the identification of potential *barriers* to solutions for the problem in the form of *limiting factors* and *risks*. Such barriers are important determinants of the overall tractability of working on a problem and can, thus, inform a subsequent estimation of tractability.

First, *limiting factors* identify those aspects of potential solutions that are least likely to scale to the full extent of a problem. This helps to better understand the bottlenecks that solutions are most likely to run into and prioritize our efforts (Savoie, 2019). Some general limiting factors that can be considered as starting points for this analysis are (Savoie, 2019): talent pool, political support, funding, room for more funding (i.e., how much funding can be effectively absorbed by organizations working on the problem), and logistical capacity (i.e., how many resources can be effectively used). For instance, on the one hand, if the problem assessed is “climate change” through green IS then academic incentives seems to be a major limiting factor that blocks solutions to the problem much earlier than either talent, room for more funding, and logistical capacity would require (Gholami et al., 2016). On the other hand, if the problem assessed is “risks from emerging technologies” then the talent pool seems to be a

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\(^2\) For instance, solving a problem in the local scope generally only affects the people who are directly involved in the situation, solving a problem in the domain scope can generalize to people in similar situations, solving a problem in the global scope is likely to affect at least large parts of the world, and solving a problem in the universal scope might even radically alter our very understanding of what is valuable.
major limiting factor because time is needed to develop experts capable of effectively working on such topics (80,000 Hours, 2020).

Second, risks describe the potential negative effects that could be triggered by working on solving a problem. It is prudent to explicitly think about risk before steps are taken to work on a problem (Jirotka et al., 2017). For instance, in the context of “risks from emerging technologies” IS research has started to highlight the potential dark sides and unintended consequences of novel IT use (Tarafdar, Gupta and Turel, 2013). Becoming aware of such risks and potential opposition to further research and IT development because of them is very helpful when thinking about the tractability of problems and is deemed an important aspect of responsible research efforts (Jirotka et al., 2017).

4.4 Module 4: Importance of the Problem

The fourth module assess the overall importance of a problem from a given perspective with the help of three components, namely, scale, tractability, and trajectory. This assessment of importance is informed by the logic of CE analysis (Garber and Phelps, 1997) and the basic economic premise that one should prefer investing into solving those problems that promise the best return on investment (ROI). Specifically, we suggest to look at the scale, tractability, and trajectory of problems to make a rough assessment of the ROI that could be expected from working on a problem (Wiblin, 2017; Halstead, 2019; Wiebe, 2019). Given problem scale measured in the form of “utility gained / % of problem solved” and problem tractability in the form of “% of problem solved / extra resources”, the CE of the investment of extra resources can be calculated with the formula “scale * tractability”, which simplifies to “utility gained / extra resources” (Wiebe, 2019).

Moreover, assessing the trajectory of expected changes in scale and tractability (i.e., roughly how much are scale and tractability going to increase or decrease in the future?) can help to interpret the estimate of CE in the light of expectations about how the importance of a problem will develop over time. This is important because the marginal CE of extra resources depends on the amount of resources which are and will be invested into solving a problem in addition to the scale of the potential benefits to be gained (Caspar, 2017). This means that CE analyses can only provide a snapshot of CE at a specific point in time. Thus, in times of quickly changing environments CE estimations might need to be updated in relatively short intervals. We propose that the simple approach of estimating the problem scale, tractability, and trajectory can act as a quick and useful proxy for assessing the importance of problems from a given perspective that is conducive to rapid iteration and feedback.

5 Application of the Framework

In the following sections, we describe the application of the PAF in the context of three different IS research scenarios: problem assessment, problem selection, and problem justification.

5.1 Scenario 1: Problem Assessment

Figure 2 details how a basic application of the PAF works. In general, one would start with a problem that is deemed interesting enough to require a more detailed assessment and work through all of the modules of the PAF in turn. The first module is used to clarify the perspective that is taken for the assessment of the problem. The second module is used to capture a consistent definition of the problem. The third module is used to identify barriers, which could affect the tractability of working on the problem. The fourth module is used to assess the importance of the problem from the chosen perspective. Altogether, these modules provide a systematic way to create a comprehensive problem assessment for a specific perspective. To create a holistic assessment from multiple perspectives, multiple applications of the framework can be carried out for the same problem.
Going beyond this underlying logic, we also instantiated the PAF in the form of the PAC (see the online appendix; Herwix and Haj-Bolouri, 2021) to make it more intuitive and useful in collaborative research team settings. In particular, the PAC aggregates all of the relevant information for a systematic problem assessment on one page or canvas, which makes it easier to work together in collaborative problem assessment sessions. Moreover, the PAC provides instructions for how to carry out the assessment and links to a supplementary online tool to help with the calculation of the expected CE of working on a problem based on estimates of problem scale and tractability. Together these tools lend themselves to a systematic and comprehensive, yet relatively quick assessment of problems. For instance, a tentative problem assessment from one perspective facilitated by someone who is familiar with the PAF should not take longer than about an hour. However, more thorough assessments could require the conduct of additional data gathering (e.g., literature reviews) or even additional empirical measurements. Thus, the PAC can be seen as a starting point for systematic and iterative investigations of the problem space.

5.2 Scenario 2: Problem Selection

As we have already highlighted in the introduction, the selection of important research problem is generally a quite opaque and not well understood part of the IS research process (Weber, 2003; Winter and Butler, 2011; Rai, 2017). Against this backdrop, the PAF affords a more rigorous approach to problem selection that is built around the cumulative use of systematic problem assessments for the benchmarking of problems in terms of importance. In general, two different modes of using the PAF can be distinguished. On the one hand, it is possible to start with the identification of broad global priorities and then, in a second iteration, look for important IS research problems within these broad problem areas. We term this mode of using the PAF priorities-led problem selection. It could be particularly interesting for researchers that are willing to explore new areas and push the IS field in new directions. On the other hand, it is possible to start by looking at important research problems within a specific IS subcommunity and then, in a second iteration, look for important global priorities as application areas that they can contribute to. We term this mode of using the PAF domain-led problem selection. It could be particularly interesting for researchers that are looking to exploit existing IS research expertise for maximum impact.

Both modes of using the PAF require an iterative application of the framework and the buildup of a problem assessment portfolio (cf. Gable, 2020). While it is informative to assess a problem on its own, looking at and comparing a variety of different options is necessary to identify the most important problems. Specifically, given potentially vast differences in expected CE of working on problems (e.g., Ord, 2013; Wiblin, 2017) investing at least some resources into building up an understanding of the most important ones seems like a worthwhile investment. In practice this could be realized through the publication of problem assessments or comparisons of problem assessments that are informed by the PAF. This would also mean that not all IS researchers would need to apply the PAF themselves, rather thoughtleaders within the field could use the PAF to inform their suggestions for new research directions that could then be taken up by other researchers.
5.3 Scenario 3: Problem Justification

In addition to facilitating a more systematic and cumulative problem selection process, the PAF could also be used to ground the justification of research problems. As Spindeldrecher et al. (2020) illustrate, research problem justification in IS is generally concentrated around business logic and neglects other perspectives which could also reveal important problems. While we do not see the PAF as a normative framework in the sense that it suggests to put certain perspectives over others, it does help with appraising different perspectives in a systematic way. Specifically, it provides a common theoretical foundation for the assessment of problems across perspectives that can encourage discourse ethics focused on the justification of IS research priorities (Mingers and Walsham, 2010). For instance, IS researchers could use the PAF and the suggested perspectives within it as a starting point for the justification of their work from multiple perspectives: How does a research problem relate not only to academics, practitioners and businesses but also to governments, civil society organizations or, maybe most importantly, future people? How are potential value conflicts handled? The PAF can help to systematically develop answers to such questions and, thus, help to clearly justify the importance and relevance of a research problem.

Beyond this, the PAF also encourages an empirical look at the importance and relevance of research problems. As Moeini et al. (2019) highlights empirical evidence is thought to be conducive to a practice-oriented framing of research problems but not used to the extent that would be desireable. The PAF provides a clear rationale and strategy for using empirical data to justify problem importance and relevance that could help to improve the status quo.

6 Discussion and Limitations

We have presented the PAF and the underlying PAO as a more rigorous foundation for the systematic assessment of research problems that is conducive to more relevant and impactful research. In particular, we outlined three scenarios in IS research where we expect the PAF to make significant contributions: problem assessment, problem selection, and problem justification.

In terms of problem assessment, extant research has highlighted that it is generally treated as an unstructured process that is mostly guided by an intuitive understanding and perception of phenomena and problems (Weber, 2003). Although broad recommendations for how to increase the importance and relevance of problems exist (Davis, 1971; e.g., Alvesson, 2011; Winter and Butler, 2011; Rai, 2017), we are not aware of any framework that provides a theoretical foundation for the systematic assessment of research problems from multiple perspectives. This is an important gap because it means that the IS research community is unlikely to engage in a cumulative discourse about IS research priorities that could systematically identify and promote the most important research problems. The PAF provides the theoretical foundation necessary to close this gap. The feasibility of PAF supported problem assessment can be verified through the application of the PAC.

In terms of problem selection, we have argued that this can be supported systematically through the iterative and cumulative application of the PAF. Currently, problem selection is mostly facilitated by disconnected research agendas that do not attempt to enable (or even intentionally obscure) the comparative assessment of research problems in terms of importance or relevance. The paper by Moeini et al. (2019) provides an interesting illustration of this dilemma. They develop a framework of potential practical relevance of IS research and aim to demonstrate its usefulness by outlining future research directions. However, all topics that they suggest for future study are simply listed without reference to justification of their importance – a practice that they criticized just a few pages before. This illustration is not meant to paint the work of Moeini et al. (2019) in a bad light but simply to highlight the entramched nature of how little we, as a research community, currently support problem selection. Although this state of affairs is very understandable from a pragmatic or evolutionary perspective, we question its efficacy for leveraging the full potential of IS research. If the very best research problems in terms of relevance and importance are likely to be few and far better than the average problem (e.g.,
Ord, 2013; Wiblin, 2017), it seems important and worthwhile to invest at least some resources into the systematic comparison of problems and identification of priorities (cf. Gable, 2020). We propose that the PAF provides the theoretical foundation necessary to facilitate this kind of discourse in a rigorous and systematic way.

In terms of problem justification, we have argued that the PAF helps to clearly and rigorously justify the importance and relevance of research problems from different perspectives. Currently, it often seems like the justification of IS research is an afterthought that is only done in reference to market logic and business values (Spindeldreher et al., 2020). Again, we question the efficacy of this state of affairs for leveraging the full potential of IS research. While existing businesses are certainly important stakeholders in our world, recent developments in moral theory suggest that from an impartial ethical perspective the needs of future generations and, thus, the sustainability of the human project might be even much more important (e.g., Beckstead, 2013; Bostrom, 2013; MacAskill, 2019a). Thus, it seems fruitful to encourage the exploration of new scripts that go beyond the standard focus on near-term business value and embrace other perspectives such as those of future people (Gholami et al., 2016; Seidel et al., 2017). The PAF provides a theoretical foundation to support and guide this process toward the most important research problems. Encouragingly, given a trend toward earlier peer-review on research projects through registered reports (Weinhardt, van der Aalst and Hinz, 2019; Doyle and Luczak-Roesch, 2020) and recommendations to emphasize problem justification (Moeini et al., 2019) the attention paid to the topic could grow in the future.

In terms of the limitations of our work, we note that the PAF is not a panacea for solving the issues and realizing the opportunities outlined in this paper. While we would argue that a theoretical framework for assessing problems such as the PAF is a necessary ingredient to start with the systematic identification of the most relevant and important research problems, it is by no means sufficient by itself. For the promise of more relevant research on important problems to be realized the IS research community, or rather a substantial portion of individual IS researchers, needs to start engaging in systematic and rigorous discourse about research priorities. Although, we take first steps on this road, we have only invested a small amount of resources into making it easy and desirable for people to walk with us. The instantiation of the PAF in the form of the PAC is only a first attempt and proof-of-concept in the much larger project of providing adequate support that empowers IS researchers to make the most of their efforts. In particular, future research should more deeply investigate the utility of the PAF by building on it or comparing it to alternatives.

7 Conclusion

In this paper, we have presented the PAF and the underlying PAO as a more rigorous foundation for the systematic assessment of research problems that is conducive to more relevant and impactful research. We have discussed its use in the IS research scenarios of problem assessment, problem selection, and problem justification and made the feasibility of the PAF easily verifiable by making the PAC freely available and encouraging its use. Altogether, we make an important contribution by presenting a set of artifacts that together provide a foundation for the development of more relevant IS research through a focus on more important problems. As Winter and Butler (2011) have highlighted, the IS research community has tremendous methods, capabilities and insights to offer, let us apply them wisely.

Future research is encouraged to further improve on our work. For instance, currently a structured description of the problem situation is not suggested by the PAF despite potential options for doing so because additional tooling would likely need to be developed to make the result accessible to researchers. Moreover, currently the PAO only highlights the major relationships that are necessary for the purposes of this paper. Future research is encouraged to further explore the domain of problem assessment and to extend and refine the considerations offered in this paper.
References

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