

EXAMINING ASSUMPTIONS: PROVOCATIONS ON THE NATURE, IMPACT, AND IMPLICATIONS OF IS THEORY

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The Information Systems research community has a complex relationship with theory and theorizing. As a community of scholars, our assumptions about theory and theorizing affect every aspect of our intellectual lives. Ideas about what theory is, who theorizes, where theory comes from, when we theorize, how theory is developed and changes, and why theory is (or isn't) important shapes the projects we do, the partnerships we have, the resources available to us, and phenomena that we find to be significant, interesting, and novel.

Dspite its prominent place in our thinking, it is often difficult to critically examine the assumptions we make about theory and theorizing. Our assumptions affect our priorities, decision, and actions. How we think about theory and its role in our individual and collective intellectual lives is a product of complex paths and multiple influences. Although we may try to stay mindful of how our assumptions shape our perspectives and ways of thinking, from time to time we need to look beyond them.

The short papers presented here highlight and challenge some core assumptions. Each provocatively engages with one or more assumptions about theory, theorizing, and/or their impli-

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cations for IS research. In doing so, these authors engage and contribute to the long tradition of reflecting on theory and theorizing. They challenge us to return to these dialogues anew as the worlds we study and the ones we inhabit change. Readers will also find healthy differences in opinion across the provocations. We expect the diversity in these provocations will inspire thought and reflection as much as the strong views within each one.

Responding to opportunities and challenges arising from the advent of big data, machine learning, and AI, Monica Tremblay Rajiv Kohli, and Nicole Forsgren, in their paper "Theories in Flux: Reimagining Theory Building in the Age of Machine Learning," and Dirk Hovorka and Sandra Peter, in "Speculatively Engaging Future(s): Four Theses," urge us to reconsider what we seek to accomplish with theory and theorizing. Tremblay, Kohli, and Forsgren argue that practitioners are able to make use of machine learning to advance their knowledge because they are willing to accept theories that are "good enough" for their particular context and needs. This presents an opening for IS researchers to learn from practice and benefit from machine learning, but only to the extent we are willing and able to accommodate these theories in flux. Similarly, Hovorka and Peter develop the idea that

constructively engaging unprecedented phenomena requires that we shift our theoretical gaze from understanding the past to speculatively understanding our futures.

Illustrating other ways that assumptions about theory shape our work, some authors suggest that next-generation theory and theorizing, the success of our approaches, and ultimately the institutional survival of the IS discipline, necessarily entails critical examination of what we theorize and what we do not. In "Scale Matters: Doing Practice-Based Studies of Contemporary Digital Phenomena," Michael Barrett and Wanda Orlikowski seek to shift the focus of practice researchers, and the IS community in general, from the relatively defensive project of theorizing at scale to the generative project of theorizing about scale. Likewise, Eric Brynjolfsson, Alex Wang, and Michael Zhang, in "The Economics of IT and Digitization: Eight Questions for Research," develop the idea that by altering what we study, how we study it, and to whom it matters, IS economics scholars can move us from drawing on reference disciplines to being a reference discipline for others.

Even more fundamental, we are challenged to consider whether the IS community's emphasis on theoretical knowledge is, or is not, conducive to achieving impact and influence. In their paper, "Focusing on Programmatic High Impact Information Systems Research, Not Theory, to Address Grand Challenges in the Real World," Sudha Ram and Paulo Goes assert that now, more than ever, the IS research community must engage societal grand challenges through high-impact cumulative research programs. For this to be successful, they argue that we must shift from thinking of theory development as the primary focus of research to being a potentially useful, but peripheral, goal that is secondary to problem-solving. In

contrast, Bernd Stahl and Lynn Markus, in "Let's Claim the Authority to Speak out on the Ethics of Smart Information Systems," assert that not only is theory useful for constructively engaging the challenges of ethical and responsible use of AI, but that having a developed, validated, deployable theoretical foundation is essential if the IS research community is to have a "seat-at-the-table" in current and future discussions of AI use and societal implications. Without this, our ability to contribute to the resolution of these critical issues will be severely limited.

Finally, others ask where theory "comes from" and what that means for how we conduct ourselves as writers and scholars. In "All Information Systems Theory is Grounded Theory," Natalia Levina shows how the paths to theory development often differ from the process that is described in published work. By acknowledging the nature of the theory development process as a dynamic interplay between ideas and phenomena, Levina argues that we would be better prepared to theorize in a way that is both more systematic and easier to learn from. Complementing this, John King, in "Who Needs Theory?," relates data and theory to the more basic concepts of observation and insight, reminding us that when we mobilize observation to provide refined insight, we successfully balance the instrumental and practical with the critical need to motivate inquiry, and thus generate knowledge that is well crafted, accessible, and high-impact.

Whether you agree with them or not, these provocations will challenge you to reflect on the ways we theorize and their implications for our ability to recognize, engage with, and address the seen and as yet unseen challenges and opportunities in the coming years.



THEORIES IN FLUX: REIMAGINING THEORY BUILDING IN THE AGE OF MACHINE LEARNING¹

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Researchers employ methodologies that rely on contemporary technologies to study phenomenon. Recent advances in artificial intelligence (AI), particularly machine learning (ML), have intensified the speed, and our abilities, to create and deploy new knowledge for constructing theories (Abbasi et al. 2016). The availability of big data and ML tools is no longer the sole domain of academics. Business processes generate large amounts of data and now practitioners also deploy ML-enabled methodologies to create new knowledge through working theories.

This presents an opportunity for Information Systems (IS) academics to collaborate with practitioners by addressing their business problems while also creating new theories that have the potential to serve as building blocks toward a generalizable theoretical contribution. As IS is an applied discipline, IS academics must uphold methodological rigor when new technologies, such as ML, offer new methods of knowledge creation.

Both practitioners and academics share the view of theory as abstracted knowledge about the world, and both seek rigor in theory building methods. Academics consider methods to be

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rigorous when such methods result in parsimonious, generalizable and repeatable² theories. Practitioners apply rigorous methods, such as experimentation and A/B testing, to create theories from situated abstractions that can be acted on and can be used to defend their actions. Practitioners value theoretical precision because the consequences of their decisions are costly, for example, in how to price products or when to launch advertising campaigns.³ They weigh the degree of acceptable uncertainty with the costs of delay. A delay of even one month to act on findings could add up to millions of dollars in lost revenue. Therefore, practitioners are willing to accept greater uncertainty in a theory that is *good enough*, then iterate and improve.

We propose that ML offers an opportunity to reimagine the theory building process. By rapidly generating numerous situated abstractions that can be discarded or refined in pursuit of a generalizable theory, researchers can iterate quickly. As such, we can expect a new form of theory that remains in

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²United States of America National Institutes of Health (NIH) define scientific rigor as "the strict application of the scientific method to ensure robust and unbiased experimental design, methodology, analysis, interpretation and reporting of results ... [so that] others may reproduce and extend the findings" (https://grants.nih.gov/policy/reproducibility/guidance. htm). It reinforces the notion that rigorous research is parsimonious, generalizable, and repeatable.

³We thank the senior editors for this insight.

flux and then evolves. By collaborating with practitioners, academics can apply theories to fill the knowledge gaps when practitioners are unable to rationalize relationships among the variables analyzed. Similarly, practitioners can test and (dis)-confirm academic theories when a technological change results in changes in human behavior, such as how privacy calculus in online shopping explains drivers' willingness to share data in Internet of Things (IoT)-based connected cars. We argue that managerial sense-making, combined with ML, will accelerate our ability to generate new theories that are relevant, rigorous and *good enough* to be useful. We refer to these as theories in flux (TIF).

What Is a Theory in Flux?

We define TIFs as evidence-based inferences that emerge from analyzing large amounts of data or big data, often gathered from business processes and in partnership with practitioners. ML and big data present an opportunity to generate numerous TIFs in several ways. For example, an academic can be an active participant who collaborates with the practitioner organization as an action researcher to develop and refine the theory. Alternatively, the academic may simply obtain data or provide findings and remain as objective and independent as possible. A TIF generally takes shape when a pattern of a phenomenon emerges from the analysis of data. The role of the academic researcher is to further refine TIFs into generalizable theories through engaged scholarship and subsequent scholarly validation. There is a place for, and indeed a need for, targeted and contextual micro-theories that comprise TIF. Under contemporary constraints of academic rigor, TIFs perish along with possible future new theories, resulting in a loss to academics and practitioners.

Practical relevance is the key to a TIF. Therefore, targeted relevance appeals to practitioners and emerges in the form of bounded generalizability, fast feedback, and iterative refinement. These characteristics are inherent in TIFs, and academics should reflect upon their merits to reimagine theory building. In this way, TIFs will augment theory building methods deployed by academics and better explain a phenomenon by providing new paths to discovery that are consistent with academic standards of rigor, relevance, and generalizability.

Machine Learning and TIF

With ML, contemporary organizations can process large amounts of data to create new knowledge about customers' behavior, product quality, and the effectiveness of delivery processes. Practitioners apply ML to big data to model, for example, consumer behavior, to rapidly build TIFs, test them in a redesigned process, then adopt or refine in favor of emergent TIFs that better explain the phenomenon of interest, which in turn improves decision quality. Even when a TIF is discarded, the emergent learning informs future TIFs, just as an unsupported hypothesis informs future hypotheses to advance scientific discovery.

Following a series of interviews with executives of leading digital organizations, we observed the divergence between how contemporary organizations use big data and ML to generate practical insights in the form of TIF, and how IS academics build theory. A vigorous panel discussion at the International Conference on Information Systems and recent journal editorials (for example, Rai 2016) indicate that many academics in the IS discipline hold similar views.

By reimagining theory building, academics can recouple theory building and theory testing that were traditionally viewed as two separate activities. For example, an ML-driven IT artifact enables reciprocal learning between the artifact and the researcher to create new knowledge and build theories. Researchers can interact with ML and quickly explore large data sets and examine a number of relationships to create multiple TIFs. Subsequently, they can discard or refine TIFs, and provide the most promising theory-supported guidance to decision makers, who can then test the efficacy of TIFs in practice. Furthermore, after decision makers take action, the outcomes can be fed back into the model and analyzed for emergent learning.

Some may argue that the iterative methodology is not new and ML is simply a tool that cannot, and should not, replace human imagination in theory building. We agree. However, we propose that the speed with which ML can discover initial patterns, test, and develop theory can pave the way to create relevant and rigorous theories in the future. Furthermore, with reciprocal learning, ML can help uncover obscure features and relationships in a problem setting, expand researchers' imagination and motivate further theory development. Our failure to take advantage of emergent developments, such as ML, risks missing opportunities to make important discoveries (Maass et al. 2018). Therefore, reimagining how to leverage the emerging nexus of big data and ML to build TIF will expand goals to construct actionable and useful theories.

Reimagining Methodological Rigor in TIF

Generalizability: In traditional theory building, generalizability is a core objective. Without it, a theory may apply to only one or a few instances, requiring scarcely available

resources to create new knowledge. Generalizability requires theorists to be precise in defining and using constructs and variables. In practice, the application of theory across contexts rarely meets the original definition or assumptions, which then requires adjustments to tailor the theoretical specifications to fit the new context. In the ML and big data environment, generalizability is not a concern, or in the words of a director of a digital organization that utilizes ML capabilities, "generalizability is not a virtue." Organizations that deploy ML find that the costs to customize past theoretical insights to a new context often outweigh the costs of seeking insights tailored to the new context. By reimagining the scope of generalizability, ML can quickly provide a large number of context-specific situated insights for further validation. Multipurpose generalizable theories, practitioners argue, are like a Swiss army knife that offers general guidance in typical contexts, while insights from a TIF are like a scalpel to perform precision surgery, customized to a context. The availability of large datasets and abundant processing power has made it possible to generate quick, precise, custom TIFs for a targeted context.

Replicability: Replicability implies that other researchers should arrive at similar findings when testing theories. Replication builds confidence in the integrity of logical relationships among variables and ensures that findings indeed describe a phenomenon and are not an artifact of the method or the context. For TIFs, replicability of findings across time or contexts isn't a requirement because what matters is fast, useful analysis, good enough for making decisions. Indeed, artifacts of the method or context may be a virtue because they can illuminate how different methods provide more precise guidance under certain conditions. However, when the phenomenon is in flux, replicability is neither expected nor desired. For example, property rental company Airbnb must deal with fluctuations in rental markets and with changes in consumer preferences depending upon location, events, and time of the year. In this case, findings of consumer preferences are neither replicable nor desirable because each dataset will produce different and customized theories, or TIFs, for each consumer population. Should a common construct for consumer preferences emerge, TIFs have the potential to coalesce into a generalizable theory while still creating new knowledge relevant for a specific consumer segment.

Parsimony: Academics aim for theories to be parsimonious, that is, demonstrate high explanatory power using the fewest variables or constructs. This criterion overcame resource scarcity because data collection was costly, intrusive, or otherwise difficult, and processing costs were high. For TIFs, the abundance of data, compression of time to capture large amounts of data, and few processing constraints make parsi-

mony no longer necessary.⁴ Indeed, including more variables in the analysis leads to deeper insights, and more TIFs, because ML can uncover latent relationships that may *prima facie* appear to be unrelated.

Despite the emerging methods to build TIFs, researchers will continue to play a crucial role in interpreting the analysis (e.g., explainable AI) and to ensure that the findings are not spurious or inadvertently biased.⁵ We contrast the traditional and TIF evaluation criteria in Table 1.

In our review of recently published papers, we found that the theory building process is expanding to a rapid, pragmatic theory development that, like TIF, can be quickly and iteratively refined through the use of ML. It is clear that authors, reviewers, and editors of these papers were flexible about generalizability, replicability, and parsimony, so that such papers (four papers discussed here) were published in the leading IS journals. Zhou et al. (2018) and Adamopoulos et al. (2018) used ML (textual analysis) and econometric techniques to propose TIFs. The TIFs are drawn from a specific time frame in an online platform and may not be generalizable to other time frames or platforms, yet the authors uncovered interesting insights for their specific context. Lin et al. (2017) and Dong et al. (2018) used the design science paradigm to extract useful features through ML algorithms. These papers are not framed as theory-building papers per se; however, the authors uncovered new relationships that are consistent with our definition of TIF. Such papers demonstrate that the IS community is likely to benefit from reimagining generalizability, replicability, and parsimony for theory building.

Opportunities and Risks of Theories in Flux

Science embraces discovery. ML advances the discovery process by providing academics with an opportunity to observe phenomena in near real-time and to build TIFs that open new paths to discovery and provide a promising pipeline for the development of future grand theories. We see an example in biology where the electron cryo-microscopy, an emergent tool, provided biologists the opportunity to observe cell activity in real-time and to uncover new paths to discover how cancer progresses.

⁴In ML, parsimony is applied by selecting the simplest model from all possible models that provide similar performance. In this paper, we will use the definition used by IS theorists and outlined in the paper.

⁵For example, Amazon conducted an internal AI study to help speed identification of people to select for interviews. What they found instead—upon human inspection—was that it was biasing against women.

Table 1. Summary of Methodological Evaluation Criteria						
Criteria	Traditional Criteria	TIF Criteria				
Generalizability	Generalizability is a core objective	Focused, context-specific solution is valued over generalizability.				
Replicability	Replicability is required	Replicability is not pursued. The goal is context-specific new knowledge.				
Parsimony	Parsimonious solutions are preferred, both for simplicity in analysis and for explanation	ML computational power enables analysis of many features or variables from large datasets and facilitates latent patterns to emerge. Transparency will sometimes be desired.				

Timely development of theories encourages their use by practitioners. However, academics must be aware of risks when engaging with practitioners. Practitioners may be unwilling to share proprietary data, their expertise, or access to processes because they fear exposing the organization's intellectual property. Even when practitioners engage with the academic community, they may do so for reasons that serve their business interests, not for scholarly pursuits such as theory building and publications. Academics can mitigate the risks by signing nondisclosure agreements (NDA), agreeing how to assign ownership of intellectual property, discussing how to publish academic findings while protecting propriety information, and by involving professionals from their institutional knowledge transfer office to ensure that the relationship with practitioners is transparent and enduring.⁶ We believe that, with such safeguards, the rewards of collaboration are worthy of the potential risks.

A Call to Action

IS academics are uniquely positioned to use ML and TIFs for theory building: we are an applied discipline, with technical proficiency, and the methodological expertise in areas such as grounded theory and design science. The history of the IS discipline is that of solving business problems with IT, and advancing the discovery process as new technologies emerge. Now ML-enabled TIFs have the potential to further that role. And yet, IS academics have been slow to embrace ML in theory building, while other management disciplines such as marketing and finance are already utilizing ML to build

theories. Below, we propose three actions that the IS community should take to endorse TIF as a viable method for theory building.

First, to avail opportunities to conceive new theories that impact practice, academics must be able to publish TIF-based research. Journal and conference editors and reviewers must be open to ML-enabled TIFs to promote emergent learning, whether through micro-theories, algorithms, models, or lessons learned. We must be open to TIF's context specificity, reciprocity, and bounded generalizability as acceptable standards of rigor. When a scholarly community determines, say through the review process, that a method demonstrates a logical link between the research question and the answer, the method is accepted as evidence of rigor by the community.

Second, academics and practitioners in the IS community can be pragmatic in adopting evidence-based TIFs, just as medicine and healthcare disciplines adopt proven practices even as controlled trials further refine the findings. This demonstrates the value of sharing emergent knowledge quickly and in ways that are accessible and useful to both academics and practitioners. The IS discipline has thrived when it has adapted methods to leverage emergent tools of data collection and hypothesis testing. A TIF enables IS researchers to combine theory development with contextually informed rapid data collection and analysis to appropriate the benefits of ML.

Third, IS academics can serve as pioneers for other business fields by identifying new and better ways to develop, refine, and manage TIFs. This could include tackling the nontrivial problem of opening the black-box embedded in ML theory development. With proper safeguards to mitigate risks, academics can make ML-based TIFs transparent, explainable, and reversible such that others can rapidly build and test theories that the practitioners can apply with confidence. Generally, IS researchers can take the lead in deploying TIF-related research to accelerate, iterate, and build the cycle of scientific discovery, and to benefit science, in general.

⁶For examples, see https://tlo.mit.edu/ for the Massachusetts Institute of Technology's Technology Licensing Office and https://www.ukri.org/ for UK Research and Innovation described as "works in partnership with universities, research organisations, businesses, charities, and government to create the best possible environment for research and innovation to flourish."

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Speculatively Engaging Future(s): Four Theses¹

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"Bold ideas, unjustified anticipations, and speculative thought, are our only means for interpreting nature." Popper 1959, p. 280

Our theoretical understanding of the world and our expectations for the future are challenged by unprecedented sociotechnical phenomena. In multiple instances, including novel economic logics of surveillance, the control of collective life by digital systems, and the valgorithmic distribution of "truth," new intertwined cultural and technological configurations have come about at unanticipated scale, scope, or speed. We are sleepwalking into future(s) no one planned or can account for as our individual and collective experiences of life are themselves changing and enacted into enduring configurations distinct from what has been previously experienced. The unprecedented are not merely surprises or events that happen unexpectedly or take unanticipated shapes (Cunha et al. 2006). Surprises occur within an accepted understanding of the world and are resolved within that reference frame. The unprecedented subverts our theorizations and calls us to engage our future(s).

The unprecedented is the often-unseen restabilization of technology, society, individuals, work, economics, and law—our technoculture—into novel and durable configurations. Our dominant research apparatus makes events, such as 9/11 or the COVID-19 pandemic highly visible and focuses research attention on "what will happen now?" But these events, which we view in hindsight as catalysts of change, are surface symptoms of more substantive and pervasive reimagining of the world. To make the unprecedented visible, we need modes of theorizing that prepare us to recognize and understand unexpected, creeping relationalities, logics, beliefs, values, and activities through which the world is navigated and realized.

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Speculative engagement with future-oriented theorization can problematize the assumed challenges of contemporary technologies (e.g., AI, social media, persuasive computing), alongside legacy infrastructures and the formation of beliefs and values to engage in determining future research and participate in the societal discourse of our future(s) living with technologies.

In this essay we foray into *speculatively engaging future(s)* as a mode of theorization in the present. Speculation becomes central to engaging with the unprecedented and is required to extend research into questions more epistemically distant than presently visible. We challenge the demand for empirical data because it grounds our understanding of destabilizing/restabilizing future(s) in the past. As "human and nonhuman actors [are] brought into alliance by the material, social, and semiotic technologies through which what will count as nature and as matters of fact get constituted for—and by—millions of people" (Haraway 2018, pp. 50-51) it is vital that IS researchers engage the world with working models and social imaginaries of livable future(s). It is through the processes of research and practical application that new relationships and novel assemblages are formed.

Mary Shelley's *Frankenstein* provides both monsters and Promethean aspirations as an allegory for the unprecedented. The allegory of Frankenstein does not concern out-of-control technology but rather foregrounds how modes of knowledge production can unsettle the fundamental categories and means of understanding the world and focus attention on the responsibilities of the creator to attend to the social implications of their creations. To provoke this unsettling, we offer four theses to IS researchers to *speculatively engage* the future(s) we seek to explain, understand, and create.

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Thesis I: The Unprecedented Restructure the World

Frankenstein's subtitle, "A Modern Prometheus," acknowledges the Age of Discovery as a period where expanding geographic knowledge, novel forms of philosophy, and advancing technoscientific developments subverted prior apparatus of comprehension. The new "sciences" fueled Promethean ambitions, created logics, and drew attention to beliefs, relationships, and commitments previously unseen. As Victor Frankenstein proclaims,

They penetrate into the recesses of nature and show how she works in her hiding-places....They have acquired new and almost unlimited powers; they can command the thunders of heaven, mimic the earthquake, and even mock the invisible world with its own shadows (Shelley 2018, p. 57).

The sciences and technologies of Victorian time, although nascent, were easily discernible and remarked. It was readily apparent that electricity, the steam engine and railroad system, and newly created forms of work would change the future, but it was unclear how. In speculatively deploying a living being not made by God, something unnatural, something *monstrous*, Mary Shelley reveals the perils and hubris of an unprecedented shift in the apparatus of knowledge creation, a shift that would remake the rhythms of life, professional identities, the production of knowledge, and the landscape itself.

In contrast, our present culture is techno-centric, and science and technology inter-penetrate to a point where it is difficult to participate without digital technologies and systems. The sheer ubiquity of technology renders unprecedented reconfigurations as "necessarily unrecognizable...we automatically interpret [them] through the lenses of familiar categories, thereby rendering invisible precisely that which is unprecedented" (Zuboff 2019, p. 12). The incremental accretion of technologies and processes, such as surveillance, technological politics, or algorithmic categorization, is viewed as the slow pace of constant progress occurring within our current understanding of theory and research. We struggle to articulate and comprehend the reconfiguration of socio-technical relationships, logics, and values underlying the destabilization of institutional authority, the embrace of misinformation and conspiracy in social media, large-scale social-credit systems, and the economics of surveillance, using the same theoretical apparatus as have been useful in the past. The dominant research apparatus assumes the future will be a mostly familiar extrapolation or extension of the past (Hovorka and Peter 2018).

Future technoculture will increasingly be performed through both manifestation of technology (e.g., data science, algorithmic decision systems, digital humans, distributed ledger technologies, and the Internet of Things) and enactments of cultural beliefs, imaginations and values such as mis/disinformation, privacy, and social activism. Yet we lack language to describe these unprecedented configurations or to articulate why they matter. Our current research orientation to the past obscures our ability to recognize and research implications for the future.

Thesis II: Research as a Commitment to Future(s)

We must concern ourselves with what kind of worlds we expect with our theories (Schultze 2017) recalling that the future is "a profoundly vital component of the present (however defined) or, more fundamentally, a principle of present action" (Slaughter 1998, p. 372).

Victor Frankenstein's failure to consider the broader implications of his narrow pursuit for technical proficiency resonates as IS researchers theorize technology systems without a firm commitment to the holistic world-making in which they participate. Victor was obsessed with technical proficiency which he viewed as

so astonishing a power....I hesitated a long time concerning the manner in which I should employ it....whether I should attempt the creation of a being like myself....but my imagination was too much exalted by my first success to permit me to doubt of my ability to give life to an animal as complex and wonderful as man (Shelley 2018, p. 170).

It was not until his own creation caused death and suffering that he became aware that he was implicated in research that might make the future lived conditions of humankind "precarious and full of terror" (Shelley 2018, p, 170).

Our theories and research designs always commit to an image of the future. Multiple modes of theorization implicitly conceptualize *the future* as a teleologically driven extension/extrapolation of a familiar present to a mostly-as-familiar future state (Hollinger 2014). The field's empirical orientation drives theory building as a process of reconstructing the past from the viewpoint of the present. Identifying salient structures, causal pathways, relationships, and variables designate the future as a manageable and predictable outcome of present activities. In future-studies research (e.g., predic-

tion, forecasts, scenarios; for a review see Hovorka and Peter 2018), and design-oriented modes (e.g., ADR, DSR), this knowledge is instrumentally and optimistically applied to create future(s).

But our ability to predict or to design and create desired futures is inconsistent at best. Looking 50 years into the future, leading science and technology experts of 1968 accurately envisaged the miniaturization of computers, long-distance face-to-face communications, social networks, and computer storage (Foreign Policy Association 1968; Karpf 2018; Lepore 2018). But outside the domain of technology "most of those machines have had consequences wildly different from those anticipated in 1968" (Lepore 2018).

The consequences arise as institutional norms, politics, conceptions of truth, and our collective life are enacted digitally, and subject us "to decisions made for us by entities beyond our control and understanding" (Susskind 2018, p. 361). The dominant focus on technological functions or economic/efficiency benefits abstracts people into a quantified generality or renders them absent altogether (Law and Mol 2001) and technocultural enactments challenge the reliability of theorization practices because political categories (e.g., human, machine, power, consent) have been upended.

An explicit commitment to future(s) in our research liberates our thinking regarding what is around the corner of assumed trajectories and returns the richness of the embodied world to our perception. A commitment requires imagining possible lived future(s), grasping how beliefs and values shape technological and cultural development and who benefits from which futures (Chiasson et al. 2018). It does not demand that we determine right or correct predictions or make predictions at all. Our dominant research apparatus enables theorisation about techno-cultural configurations in the past. A commitment to future(s) makes visible possible ethical, political, environmental, and social landscapes in which humans and nonhuman actors will exist to theorize for their becoming.

Thesis III: Speculative Engagement Navigates Epistemic Distance

Mary Shelley's figuration of "the monster" is an example of speculatively engaging, as it makes visible unconsidered implications of world-making. As a young man, Victor Frankenstein's pursuit of magical theology and alchemy was upended by a lightning-struck tree "entirely reduced to thin ribbands of wood...so utterly destroyed" (Shelley 2018, p. 18). The obliteration of that tree by "electricity" set him on the "scientifical" path that decided his destiny. The nascent scientific apparatus of the time was oriented to epistemically

close phenomena: the visible, the present, and the measurable, which provide the foundations for theorization to this day. But life and death and the processes in-between, were epistemically distant and were beyond the ability of research apparatus to make comprehensible. In speculating Victor's animation of a living being, Shelley navigated the epistemic distance between the immediately visible and the boundaries of knowledge.

Specifically, Shelley unsettled existing categories and concepts and challenged what life is, how it is created and who can manipulate it. This speculation highlights the role of novel scientific practices and technological achievements in creating perceptions of the world that did not previously exist. In a general sense, speculation is the process of making hypothetical statements about the world for which evidence is not (yet) available (Achinstein 2018; Swedberg 2018). In the writings of Popper, Kuhn, and Feyerabend, speculation, propositions, and conjectures play a central role in the theories of science where they articulate potentialities that are "the tales that might be told about particular actualities," from a given perspective (Whitehead and Sherburne 1957, p. 256). Speculation that remains rooted in current thinking and practice can extend theory within existing bodies of knowledge (Weick 1989). But this form of speculation cannot traverse the epistemically difficult terrain encountered when conceptualizing future(s).

Instead, speculatively engaging with the future on its own terms, rather than as an extension of the past, provides a mindset of "keeping the doors and windows open...a refusal to sit within...grooves of thought...or stick within the parameters given to us by our own areas of specialization" (Halewood 2017, p. 58). We speculate regarding what engagements might be fruitful and reach beyond data into digital future(s). New apparatus—categories, relationships, and techniques—are required to disclose the unseen (re)stabilization of cultural enactment and to spotlight what is at stake. Speculative engagement can both overcome a lack of empirical observations and caution against the certainty with which our apparatus portrays the world.

For example, our existence as cyborgs (Cecez-Kecmanovic et al. 2014; Schultze and Mason 2012; Wilson 2009), the potentiality of human—AI hybrids (Rai et al. 2019) and human—machine relations (Rhee 2018), each would benefit from speculative engagements that make visible and foreground how entangling of humans with autonomous artificial agents is stabilized into meaningful configurations. Techniques such as social imaginaries (Jasanoff and Kim 2015), artifacts from the future (Peter et al. 2020), and science fiction (Parrinder 2000) can provide speculative worlds that provide sites of inquiry into relationality and difference (Wilson 2009), how

things came to be, and how things could be otherwise. How we theorize and what is of concern across this epistemic distance is the focus of our speculation. If we recognize future(s) as inhabited, care for the fabric of life becomes a principle for present day action, and our research is implicated in the lives of the people inhabiting those future(s).

Thesis IV: Our Responsibility to Inquire with Alternative Possibilities

We draw a final lesson from Frankenstein as Victor laments:

Had I a right, for my own benefit, to inflict this curse upon everlasting generations?... I shuddered to think that future ages might curse me as their pest, whose selfishness had not hesitated to buy its own peace at the price perhaps of the existence of the whole human race (Shelley 2018, p. 156).

Speculative research apparatus allows researchers to inhabit knowledge-making as embedded in the remaking of world(s) inhabited and navigated by human and more-than-human (Puig de La Bellacasa 2017) descendants of the present. Our concept of the *unprecedented* suggests that novel assemblages of "of computational power and processes into nearly every sector of global society and even the fibres of our being" (Guston et al. 2017, p. xvii) are always out of sight. Speculative engagements allow us to research from within (future) communities for which we care (Puig de La Bellacasa 2017).

Our dissent from the optimistic visions of futures which dominate IS research is needed because assembling technoculture does not result in a sudden awakening to a brave new world, in part, because "the future gets away with a lot, making itself at home in our lives before we've had a chance to say no thank you" (Zuboff 1990, p. 20). Our present theorizations implicate us in the consequences of our research. For example, algorithmic control over delivery of social goods, the distribution of information, and the constitution of "humans" in work and in recognition practices (Rhee 2018; Susskind 2018) are each based in changing technocultural relationships. What is at stake are the conditions of *living with technologies* through gradual accretion of technocultural configurations into unforeseen stabilities or in the wake of dramatic events (e.g., 9/11, COVID-19).

Other research communities have risen to such responsibility. For example, the Biological and Toxic Weapons Convention and the Chemical Weapons Convention (Walker 2015) engaged with potential manifestations of dual-use technologies despite uncertainty regarding all potential lethal combinations and contexts. New conventions, policies and

evaluations—for things that did not yet exist—were developed and have played a fundamental role in preparing for real-world concerns. The Asilomar Conference on Recombinant DNA offered experimental guidelines, containment practices, and prohibitions (Evans and Frow 2015). "Killer drone" research (Suchman 2018) has reframed foundational issues such as the constitution of *a weapon*, the concepts of *safety* and *accuracy*, and caused concerns regarding configurations of autonomous *distinction* (e.g., of combatants and civilians) to be (re)conceptualized and evaluated.

IS has an obligation to question the development and deployment of technologies "born perhaps slightly before their time; when it is not known if the environment is quite ready for them" (Mosley 2012, p. 71). The cultural consequences of technology cannot be foretold but researchers bear responsibility to inform, protect and prepare society to coexist with the creations of our research. The world-making effects of our research apparatus implicate researchers in conditioning future lives. Our future(s) deserve reflection on what we do *before* we loose monsters into an unprepared world.

Conclusions: What Happened to Our Future(s)?

If we accept responsibility for the scale and scope of technocultural change, it becomes imperative to ask of our theorizations: What happened to our future(s)? We seek to comprehend, and more importantly, to participate in "the world as becoming," yet our current apparatus renders future(s) unspoken and invisible or reduces them to extensions of the past, and leaves us unable to see, let alone respond to the unprecedented. Our apparatus (e.g., vocabularies, concepts, instruments, and practices) channels perceptions of the world in the broader society and combine with technologies and cultures into a durable present. Our creations precede us, such that "the future will consist not only of new stuff; it will also consist of what we have built and what we are building now" (Aanestad 2011, p. 28). Just as future environmental risks are grounded in the physical sciences but embodied as convergent beliefs and values which problematize global governance (Jasanoff 1999), our current research is fashioning our social values, beliefs and normative expectations for future(s). The realization that future(s) are always in our theorizing, implied but lurking just out of sight to catch us unawares, provokes us to advance speculative engagement to make multiple future(s) visible, contestable and negotiable.

Focusing research attention on technology as a manageable and predictable driver of the future shapes what we claim to know, what we believe is worth knowing and what we do not know. Our reliance on empirical data conceals embedded beliefs about knowledge and forecloses alternative approaches to the relationships of research to past/present/future(s).

The illusion of predictability and the management of uncertainty obscures the very questions which would enable perception of the unprecedented. In contrast, speculative engagement shifts our focus to research as an imaginative and disclosive practice. Speculative research has been critical in physics, mathematics and other fields to disclose and extend the boundaries of knowledge. It is equally important in IS to develop speculative judgement by removing the straitjacket of empiricist and methodological evaluation. Digital geographies (Hovorka and Peter 2021), artifacts from the future (Peter et al. 2020), thought experiments (Kuhn 1977), and other speculative engagements foreground researchers' assumptions regarding inhabited future(s). We can adopt other or even absent perspectives revealing what we fail to notice in our everyday encounters in technoculture. Speculative apparatus will open our research to creative and present engagements with Big Tech, society, and government and will invite journals and conferences to rethink knowledge production and evaluation.

Our four breakable theses begin to prepare researchers to address significant challenges of our time which lie beyond the IS fields' established parameters. Speculative research engagements allow us to dwell in imagined stabilities, rendering visible that which is unprecedented. By foregrounding the unprecedented, navigating epistemic distance, and committing to inhabited future(s), researchers can engage diverse audiences in questions of how things and people come to matter and how that knowledge can inform theorization as the future. IS research, like Shelly's Victor Frankenstein, has Promethean aspirations presenting both needed benefits and real and even existential threats to humans and more-thanhumans alike. The Academy should again find its role in the vanguard by critically engaging the unprecedented, grasping what is at stake, and providing a foundation for speculative engagements.

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SCALE MATTERS: Doing Practice-Based Studies of Contemporary Digital Phenomena¹

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Introduction

As a practice scholar of contemporary digital phenomena, I feel increasingly challenged to better account for scale. My conceptual toolbox, which has served me well in conducting "classic" workplace studies of technology, has not helped me attend to scale in my practice-based studies of digital phenomena. Should I relinquish this responsibility to those researchers whose tools and methods seem to address scale more naturally or obviously? Or are there ways to develop practice approaches to scale in studies of contemporary digital phenomena?

In recent years, we have heard these questions raised by experienced practice researchers and pondered them ourselves. Why and how does scale matter in practice? And relatedly, how should we as practice scholars conceptualize and examine scale in our research?

Contemporary phenomena are digital phenomena, replete with big data, machine learning algorithms, multiple sensors, and distributed cloud platforms. As such, they are complex, dynamic, open-ended, and scalar. It is this last characteristic that is particularly pressing for IS scholarship today, given that our phenomena are increasingly large and widespread, and our conceptual engagement with matters of scale relatively limited to date.

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Established ways of handling scale have typically involved adopting a macro focus: studying regional, national, or global phenomena that cut across spatial boundaries through techniques such as surveys, simulations, and trace analyses. These approaches build models and theories to explain phenomena at scale by aggregating and abstracting from specific conditions and experiences on the ground.

While valuable, this is not sufficient. It is not sufficient because examining phenomena *at scale* does not help us understand phenomena *with scale*. The former is an analytical focus, the latter an empirical one. This distinction is related to that made in sociology (Brubaker and Cooper 2000) and geography (MacKinnon 2010; Moore 2008), where researchers differentiate the use of notions as "categories of analysis" (e.g., experience-distant categories developed and used by scientists) from their use as "categories of practice" (e.g., categories of everyday experiences developed and used by ordinary actors).

Most treatments of scale in IS scholarship have been analytical, examining how contemporary digital phenomena *have scale* (focusing on size) and how they operate *at different scales* (focusing on hierarchical levels). These approaches do not capture the lived experience of scale, a condition integral to contemporary digital phenomena (Polykarpou et al. 2020). If we are to effectively study and theorize scale in contemporary digital phenomena, we must expand our approaches to scale, from treating scale only as "naturalized, taken-forgranted categories of analysis" (Kaiser and Nikiforova 2008, p. 538) to also considering scale empirically, as enacted in practice. Such a move recognizes that scale, while it is "real, it is *also* made" (Law and Urry 2004, p. 395).

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This is where IS practice scholars can make a significant contribution: by explicitly and intentionally taking up the empirical matter of scale so as to examine how it is produced and stabilized in everyday digital practices. This requires challenging the concerns raised about the suitability and value of a practice approach to address scale. Such concerns contend that the emerging digital landscape does not lend itself to being investigated with the typical practice approach of immersive, qualitative field studies of single workplaces. As a result, so the argument goes, a practice approach is simply inadequate or even inappropriate for studying contemporary digital phenomena that span multiple, distributed contexts and increasingly involve large platforms and big data. On the contrary, as we argue below, a practice approach has a crucial role to play in offering distinctive insights into how the scale of contemporary digital phenomena is enacted in ongoing practice.

A Practice Approach to Scale

A practice approach posits everyday recurrent actions as constitutive of social life (Giddens 1984; Schatzki et al. 2001). We highlight key premises (Feldman and Orlikowski 2011) that are particularly salient in studying contemporary digital phenomena.

- Practices enact phenomena as contingent, ongoing achievements. A practice approach posits that the social world is not fixed or homogeneous, but a dynamic and heterogeneous reality that is continually being produced, reproduced, and transformed through everyday practices. Phenomena, such as institutions, structures, supply chains, and infrastructures, are not givens but ongoing achievements that are recurrently and contingently enacted in practice.
- Practices are sociomaterial. A practice approach recognizes that practices are not just discursive or cultural, they also manifest in the world as specific materializations. These materializations of practice (e.g., through bodies, devices, data, media, robots, software, etc.) shape the possibilities of what and how phenomena are enacted (Orlikowski and Scott 2014; Scott and Orlikowski 2014).
- Practices enact phenomena in/over time and place. Phenomena are enacted in practices that are always situated in specific times and places. As these practices recur repeatedly in many locations, phenomena are enacted over time and across place. A practice approach rejects dualisms such as local—global, viewing these as co-constitutive, with the global being constituted through ongoing and distributed local practices (Barrett et al. 2005).

Practices are consequential. A practice approach views
all enactments as producing both intended and unintended consequences. By examining actions on the
ground and their resulting enactments, a practice approach calls attention to the emergence of gaps, tensions,
contradictions, and disruptions that overflow and contest
habitual and normalized performances, generating problematic as well as constructive outcomes.

Building on these premises, a practice approach posits scale as enacted in ongoing practices. Such a perspective has been emerging in the human geography literature, where scholars have proposed that scale is actively produced, contested, and reconstructed through scalar practices aimed at achieving particular goals (Fraser 2010; Moore 2008; Papanastasiou 2017). Such enactments of scale may be understood as processes of scalar structuring (Brenner 2001) that recursively (re)produce and transform scale through the choices, negotiations, inscriptions, experiments, and struggles of everyday practice (Moore 2008).

Drawing on Butler's (1993) performative account, Kaiser and Nikiforova (2008) argue that the recurrence of practices over time constructs, stabilizes, and shifts the scale effects of phenomena. While their account focuses on discursive practices, we consider the reiterative practices that produce the effects of scale to be sociomaterial. For us, scalar practices are always sociomaterial, which in contemporary conditions means that scalar practices entail pervasive digital technologies. And it is their specific materialization through digital technologies that necessarily entangle contemporary phenomena with scale.

What do we mean by this? Consider work as an example. The ways in which work is done now—whether in the office or factory, on the farm, or online—is inextricably tied up with many digital configurations entailing various platforms, multiple servers, numerous networks, countless algorithms, and big data (Orlikowski and Scott 2016). Every online search, mobile text, social media post, video meeting, financial transaction, medical diagnosis, product design, and fabrication process implicates actions on the ground with diverse, distributed, and interconnected digital configurations. It is through such recurrent, everyday digital practices that the scale effects of phenomena are constituted over time. This entanglement of digital work with scale—which is mostly invisible, inscrutable, and inaccessible—has far-reaching consequences for what is generated in the near and long term.

From this viewpoint, all contemporary work has scale effects through its ongoing enactments with digital configurations. We thus need practice-based studies that examine how work is entangled with scale through the digital materializations that condition the possibilities of getting that work done.

A Practice Approach to Scale in the COVID-19 Pandemic

To highlight the possibilities of a practice-based treatment of scale, we consider the COVID-19 pandemic, generally understood as a large-scale contemporary phenomenon. We examine how the scale of the COVID-19 pandemic is being enacted in multiple, diverse, and ongoing sociomaterial practices that are entangled with distributed and shifting digital configurations. Specifically, we offer four practice-based theses on scale.

Scale Is Enacted in Ongoing Practice

As the COVID-19 virus emerged and spread around the world, understanding of this infectious disease rapidly shifted from viewing it as a localized epidemic to making sense of it as a world-wide pandemic.² As of October 2020, there have been almost 40 million cases and over one million deaths in 215 countries and territories.³ Knowledge of this pandemic has been and continues to be generated by multiple data collection and simulation activities managed by governments, health authorities, and researchers around the world. Such knowledge has been heavily relied on to monitor the trajectory of the disease and to guide decisions about when and how to intervene so as to reduce viral spread.

While strategies focused on "flattening the curve" varied across the world, what was significant and common to the approaches was the emphasis on using insights from big data and simulation tools to manage responses to the crisis. Over time, it became clear that the recommendations generated by computational models needed situating within specific contexts and activities of social distancing, lockdown, self-isolating, quarantining, and curfew compliance in different locations. This led to a variety of epidemiological approaches and developments of digital infrastructures to support the work of monitoring, testing, treating, tracing, and predicting the spread and containment of the disease in different communities.

The large scale of the COVID-19 pandemic is not a fixed or given state "out there," but an ongoing sociomaterial enactment produced through recurrent, everyday practices that assess, generate, track, challenge, and transform knowledge and management of the virus, and which in turn, dynamically

affect the scale and trajectory of the disease over time and across the world.

Scale Is Performed Through Multiple Practices in/over Time and Place

Management of the crisis varied over time and location as diverse policies responded to projected shapes of curves, which influenced progress on the ground. A range of field experiments in various regions were attempted. For example, Taiwan acted swiftly having learned from previous experience with the SARS virus crisis in the early 2000s, while Israel similarly transitioned rapidly, reflecting their always-ready state of switching from normal to crisis mode. While these and other countries (e.g., China) were quick to use modeling and contact tracing technologies with lockdown practices, other countries (e.g., South Korea) implemented early testing on a massive scale and used contact tracing technologies aggressively to monitor spread over time. A few countries chose to carry on "as normal" (e.g., Sweden), attempting to produce a controlled spread of COVID-19 viral infection among the population so as to build "herd immunity."

Initial responses yielded varying results, with some countries seeming to have "crushed the curve," and others experiencing further spread and escalation. Fears of additional spikes have emerged in some of the countries that had reduced infections (e.g., South Korea and Australia), as new cases emerged following the easing of restrictions. In one response, the government of the United Kingdom instituted "lightning lockdowns" that shift responsibility to local authorities for rapidly triggering and enforcing scaled-down lockdowns as soon as a growing number of cases become evident. The hope is that these avoid the imposition of nation-wide restrictions and avert the economic and social disruptions experienced in earlier, broader lockdowns.

The scale of the COVID-19 pandemic is enacted through multiple practices that are situated in particular times and places. As the pandemic becomes performed differently over time and across locations, conditions on the ground change. The apparently global scale of the pandemic is not a pregiven abstraction but an ongoing achievement constituted differently by diverse, recurrent local enactments performed in multiple locations and times.

Scale Is a Contested, Consequential Achievement

The materialization of COVID-19 responses through digital technologies and big data further highlights how scale is not

²The World Health Organization (WHO) defines a pandemic as a serious new illness "occurring worldwide, or over a very wide area, crossing international boundaries and usually affecting a large number of people" Doshi 2011).

³https://www.worldometers.info/coronavirus/; accessed October 17, 2020.

a once-and-done outcome, but an ongoing enactment that entails adaptation, experimentation, and contestation over different political strategies with significant consequences for health, death, and employment. A situation at the Florida Department of Health offers revealing evidence in this regard. In May 2020, the data scientist responsible for the state's official coronavirus database was fired. Rebekah Jones claims she was dismissed because she refused to comply with orders to manipulate the data so as to justify the state's ambitious reopening plans (Luscombe 2020). Since being let go, Jones has set up her own data portal to track COVID-19 cases in Florida, and it contrasts starkly with the official statistics. The differences call attention to how contingently data about the coronavirus are being analyzed within the state (and doubtless in other states and regions), underlining the politicization of scaling strategies advocating for different policies with respect to opening and shutting down economic and social activity.

The ongoing uncertainty associated with the pandemic—its spread, treatment, and management—changes daily as more is learned about the multiple trajectories of the disease manifesting over time and place. Such continued precarity shapes people's everyday practices in dynamically adjusting how they live and work to the changing circumstances of the pandemic. As consequences continue to shift unpredictably, understanding of the pandemic and its scale alters.

What the scale of the COVID-19 pandemic is at any time, is a contested and contingent practical enactment, shifting variously and unpredictably depending on data, computational tools, modeling approaches, response tactics, politics, rhetoric, ethics, and changing conditions on the ground.

Scale Is Entangled with Sociomaterial Practices

The resurgence of cases following the easing of restrictions emphasized the importance of further testing and tracing to proactively "chase the virus." Smart phone apps for contact tracing emerged as an important approach to managing viral spread in communities. Initial attempts to deploy contact tracing apps in a number of countries followed a centralized approach with matching occurring on government servers. This elicited much concern over data privacy, and implementation stalled until an unexpected alliance between Google and Apple emerged, promising better privacy through a decentralized method of matching on people's phones. Further challenges arose when it became evident that the mobile apps are ineffective without follow-up. This entails extensive contact tracing practices that hire, train, and manage hundreds of human tracers to serve as the critical "process boots on the

ground" (ImPACT 2020) for identifying infected people and informing those they have been in contact with to immediately self-isolate and monitor symptoms.

Around the world, the COVID-19 pandemic is being variously managed through collecting and analyzing vast amounts of data from various sources, using many different simulation tools to track infection and mortality rates, modeling and predicting likely scenarios, deploying contact tracing apps and processes, testing and treating patients, and implementing policies, procedures, and mandates in multiple locations and times.

The diverse practices being performed across the world in relation to the pandemic are constituted through multiple, distributed, and extensive digital configurations. The large-scale of the COVID-19 pandemic is thus entangled with ongoing sociomaterial practices and the digital configurations in which they are implicated.

A Practice Approach to Considering Scale in Contemporary Digital Phenomena

Few would dispute that engaging with scale clearly matters for understanding large and widespread contemporary digital phenomena. But, as the COVID-19 example highlights, scale is often taken for granted in our studies where phenomena are simply assumed to "have scale." While scale conceptualized in this way may be adequately addressed by macro social science approaches, such approaches cannot shed light on the ongoing production of scale as an empirical phenomenon. To do this, IS practice scholars must challenge claims that scale can only be studied computationally as a macro abstraction, and expand practice approaches so as to examine how the scale of contemporary digital phenomena is recurrently achieved in practice.

Consider a thought experiment based on recent lived experience (by many readers) of conferencing practices, such as the 2020 annual meeting of the Academy of Management (AOM). Scheduled to take place in Vancouver from August 7-11, this conference (like so many) went online for the first time in its 84-year history. In a non-pandemic year, this conference would have been enacted through the movement of thousands of bodies from around the world to Vancouver, traveling by planes, trains, buses, cars, etc., and meeting in sessions at the downtown Convention Center and adjoining hotels. Instead, the 2020 conference was enacted through digital work practices entailing thousands of participants connecting from their homes via the AOM virtual site, utilizing multiple computing technologies, videoconferencing software, telecommunications networks, and cloud platforms.

In both cases, the large scale of the AOM is not given but an achievement, enacted through the sociomaterial scalar practices of thousands of members and organizers taking specific action involving multiple infrastructures of transportation, hospitality, presenting, discussing, and teleconferencing. What is different across the cases is how the distinct materializations of the pre-2020 and 2020 conferences changed participation possibilities. For example, the nature of engagement shifted from real-time, in-person sessions with discussion before, during, and after sessions to asynchronous taped sessions and synchronous, virtual sessions, with few opportunities for unplanned interaction. While an on-site conference makes attendance challenging for members who are unable to travel, an online conference limits the engagement of members who do not have access to reliable internet service. The different inclusions and exclusions materialized in the sociomaterial enactments of on-site and online conferences points to the politics implicated in scalar practices (Blakey 2020). Understanding the engagement possibilities as well as environmental and financial implications entailed in different materializations of large-scale conferences requires examining how scale is produced and experienced in practice across times and places. IS practice scholars are uniquely positioned to develop these nuanced conceptualizations of scale and how they matter in practice.

Future work by IS practice researchers studying contemporary digital phenomena in the field can examine how scale is enacted through the entanglement of everyday practices with a myriad of digital configurations (e.g., Facebook, Twitter, Zoom, Google, and TripAdvisor to name a few). IS practice researchers can gain insights into these ongoing and situated processes by being attuned to the scalar implications of digital work practices. Such studies would consider the scalar categories and politics that are manifesting in particular local conditions, and how these get (re)produced or changed through processes of scalar structuring involving diverse, distributed, and interconnected digital configurations.

Going forward, we advocate a focus on the relational, practical, and dynamic realities of scale that are critical to explaining contemporary digital phenomena, and which can only be effectively studied through field-based practice approaches. We contend that a practice approach is vital to analyzing scale, and urge IS practice researchers to take on this challenge. If we continue to treat scale as a given "out there," or as something that cannot be apprehended by practice approaches, then we lose the capacity to understand how scale is enacted and entangled with the digital configurations that shape how we live and work.

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THE ECONOMICS OF IT AND DIGITIZATION: EIGHT QUESTIONS FOR RESEARCH¹

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During the past 30 years, information technology (IT) and digitization have profoundly changed the economy. The development of these technologies has not only generated challenging research questions but also provided novel tools for answering them. IT researchers now use transformative IT and ubiquitous digitization to actively contribute to other fields.

Theorists studying the economics of IT and digitization have embraced opportunities for innovative studies of the economy. We have explored basic economics-related methodologies, questions, and emerging topics such as the future of work, the changing boundaries of the firm, online auctions, and online consumer behavior. IT research now drives frontier work in finance, marketing, operations management, and even human resource management. Yet, to what extent are economics of IT researchers leading this theory advancement? Are we running out of low-hanging fruit or creating a combinatorial explosion of innovations?

In this essay, we urge IT researchers to expand economics of IT research's boundary to make it a reference discipline for other fields. New and innovative research projects must push beyond the boundaries that restrict questions and methods. We must focus on novel and interesting research questions,

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adopt correct and useful methods, and achieve important and impactful research outcomes.

Table 1 presents six evaluation criteria for evaluating and advancing new economics of IT and digitization research. They are helpful for us to expand research questions, methods, and outcomes.

Such expansion requires that we embrace distruptive research and shatter our imaginary security and fictitious identity.

Opportunities for Leading Economics of IT and Digitization Research

IT has profound, broad, and multifaceted implications for economic development. Nano data from the digitization of production, market transactions, and human behavior allow rapid data-driven decision making (Brynjolfsson and McElheran 2016; Brynjolfsson et al. 2020). AI and automation technologies boost productivity in all types of businesses, but at the same time disrupt the labor economy. Products and services are merging online and offline experiences, and consumption is becoming more social. Consumers are making better choices by using search engines, recommender systems, and AI-powered decision-support tools to retrieve and process information.

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Table 1. Criteria for Evaluating New Economics of IT Research

Questions: Novel and Interesting

- Is the research question relevant to the disruption caused by new IT?
- · Does the research question address common, new challenges in practice?
- Will the research question offer economists and managers new insights into topics such as choice, coordination, scarcity/abundance, and human and machine rationality?

Methods: Correct and Useful

- · Are the conclusions rigorously supported?
- Are the methods informative/useful for practice?
- Are the findings generalizable?

Outcomes: Important and Impactful

- Are the findings important to key stakeholders of the economy?
- Does the research offer clear guidance for strategy and policymaking?
- · Does the research challenge the status quo regarding current beliefs/norms?

Markets are changing too. Digital markets are lowering prices, increasing variety and transparency, and allowing consumers and businesses to search more efficiently (Brynjolfsson and Smith 2000). New IT such as chatbots, blockchain, virtual reality, and live streaming will further lead to a more open, sharing, efficient and increasingly digital economy.

Just as IT has disrupted the economy in the digital age, researchers studying the economics of IT and digitization should go beyond narrow research silos and confront challenging theories of decision making, industrial organization, labor economics, social welfare and equality, and other subfields of economics. We should ultimately be a reference discipline for research in fields such as marketing, finance, and operations management, particularly by answering forward-looking questions and shifting attention to broadly defined topics.

Table 2 highlights eight important but under-researched questions for the next generation of economics of IT theories. These challenging and disruptive questions suggest promising research opportunities. For each question, we propose some example research topics according to the judging criteria.

Leading the Research Paradigm Shift with IT

Research on the economics of IT and digitization should fully embrace new IT-enabled research methodologies. Digitization and computation technologies have profoundly transformed social science research, particularly economics, and provided new tools for qualitative and quantitative understanding of society. Nano-data, machine-learning, and disruptive digital infrastructures are empowering a paradigm shift that features semi-automatic theory discovery, prediction-oriented research, and massive field experiments (Figure 1).

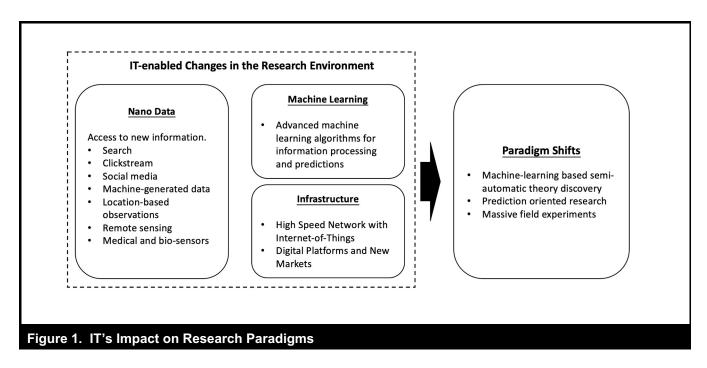
Nano-data from search engines, clickstreams, social media posts, and IoT open new frontiers for accurate predictions. Remote sensing and mobile device data provide comprehensive spatial information once difficult to obtain. Faster mobile broadband connections, higher-resolution cameras, and smarter digital machinery continue expanding full-spectrum, real-time observations of socioeconomic activities. Even better, digital platforms yield real-time quantitative data. Massive online field experiments conducted on such platforms offer invaluable opportunities for theory development (Gupta et al. 2018; Karahana et al. 2018).

Advanced machine learning algorithms may disrupt methods for discovering and testing theories by providing abundant and surprisingly effective tools for analyzing nano-data. Economics of IT research has just begun to embrace applications of machine learning algorithms to generate new variables, inspire innovative questions, suggest new theories, empirically identify causal relationships, predict counterfactuals, and simulate policy outcomes.

Nano-data, machine-learning, and digital infrastructures drive disruptive paradigm shifts in theory development. Researchers can now conduct extensive field experiments beyond previous capacities. They can analyze real-time, nonstructured data for new insights, innovative patterns, and accurate theory- and data-driven predictions. Such changes will continue challenging the current paradigm of economics of IT and digitization research.

Table 2. Challenging Questions for Investigating the Economics of Digitization and IT				
Challenging Questions		Criteria and Examples		
1. How does IT change organizations?	New/ Interesting	 How would IT-enabled decentralized decision-making change work and management practices? 		
	Correct/ Useful	How should we collect data/conduct experiments in newly emerging organizations?		
	Important/ Impactful	 How do the new forms of organizations and business models change society? How do virtual organizations and remote work impact labor force participation and gender wage gaps? 		
	New/ Interesting	How should we design markets to accommodate disruptive technologies?How should data transactions be organized?		
2. How does IT create new markets?	Correct/ Useful	Should we revise economics theory to embrace the sharing economy and markets based on blockchain technology? If so, how?		
	Important/ Impactful	What new policies are needed to regulate market participants?		
How do Al and big data transform decision-making?	New/ Interesting	 Al and data-driven decision making (DDD) augment mental processing. How do the changes compare with the disruptions that occurred during the Industrial Revolution? How will Al and DDD change managerial mindsets and actions? How do Al and DDD change rationality levels among economic agents? 		
	Correct/ Useful	 Will IT limit workforces to those who instruct machines and those who follow the instructions? How do Al and DDD change previous rational and behavior models? 		
	Important/ Impactful	 Will AI and DDD increase market efficiency? What ethical and legal issues are associated with the use of AI and DDD in markets and organizations? 		
	New/ Interesting	 What roles do environmental, social, and corporate governance (ESG) play in the new economy? When does computational power generate superior decisions? 		
4. How do IT and the new digital divide affect inequality?	Correct/ Useful	 What practices will best enable IT to reduce social, economic, and informational inequalities? Should policymakers address inequalities created by first-mover advantages in AI investments? 		
ancot mequanty.	Important/ Impactful	 Does IT development generate industry superpowers? How can we limit market power but promote innovation in big IT companies? Is IT an equalizer or polarizer? How can we ensure equal access to information and social resources? 		
	New/ Interesting	 Who owns the rights to individual data? How can we measure the value of data? Are data transactions similar to the economics of goods and resources? 		
5. What principles should be estab- lished regarding data ownership and	Correct/ Useful	 How should economics research empirically account for data privacy? Can first-degree price discrimination be achieved? What are the economic consequences? When and how do people choose between privacy and convenience? 		
privacy?	Important/ Impactful	 Should we encourage platforms to provide privacy through rewards or punishments? With more data being collected by various devices such as IoT, should data ownership be regulated? If so, how? 		

Table 2. Challenging Questions for Investigating the Economics of Digitization and IT (Continued)					
Challenging Questions	Criteria and Examples				
6. How does tech- nology create or mitigate cyber- balkanization, filter bubbles, and echo chambers?	New/ Interesting	 Should developers of IT systems introduce bounded rational IT to prevent cyberbalkanization? How do collaborative filtering algorithms create social chasm? 			
	Correct/ Useful	 Is optimal choice an illusion despite accurate predictions from recom- mender systems and preference-revealing algorithms? 			
	Important/ Impactful	Should filter bubbles and echo chambers be prevented? If so, how?Who should determine optimal levels of social interactions?			
7. How should we measure the digital economy?	New/ Interesting	How do digital business models generate previously uncaptured variables?			
	Correct/ Useful	 How can we correctly measure intangible social value when value creation itself is more and more digital? How should national income accounting measure non-monetary transactions of digital goods and services? 			
	Important/ Impactful	 How can government regulations promote and/or discourage digitization of the economy? 			
	New/ Interesting	 What alternative research methods might be used beyond analytical modeling and empirical studies of observational data and experiments? Can Al generate theories based on access to nano-level economic data? 			
8. How can we recon- ceptualize the Economics of IS research field?	Correct/ Useful	 As new technologies develop rapidly, is causal inference still important or even more so? Should we extend the unit of analysis beyond individuals, groups, and organizations to include ideas, tasks, and algorithms? 			
	Important/ Impactful	Should economics of IT research be generalized and morphed to quantitative IT or the science of IT?			



Concluding Remarks

Next-generation innovation requires economics of IT and digitization researchers to lead in embracing new research methodologies and answering new questions.

The future requires all community members to recognize the value of interdisciplinary research, ask the right questions, accept new methodologies, and contribute to other disciplines.

To achieve our goal to be a reference discipline for other fields, we must expand our questions, methodologies, and outcomes. Our research can and should bring IS/IT knowledge or methods to management disciplines outside our domain. We must consider questions that may appear to be more relevant to marketing or finance if the questions address novel and important challenges in the digital economy. Simultaneously, we should adopt unfamiliar methodologies that appear to address economic questions in data science and machine learning. Such openness and confidence will create important interdisciplinary research opportunities that contribute to all fields.

Our doctoral programs should promote interdisciplinary training. Beyond mastering theories and econometrics, students must be able to work with big data, advanced datamining techniques, and deep-learning methods, while adhering to business operations, working on-site, and collaborating with practitioners in building IT systems with sound economic rationale. Our journals should use simpler

review processes that will allow publication of APIs, software packages, short original ideas, and reports of fieldwork with quick iterations. Fast track publication opportunities should go to research that targets pressing issues such as inequality, data ownership, and new disruptive mechanism design. We may also open review processes and engage more researchers in the development stage of large projects.

Research on the economics IT and digitization should take the lead in embracing new research methodologies and answering new questions. After all, next-generation innovation can happen only when we are willing to take such calculated risks.

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FOCUSING ON PROGRAMMATIC HIGH IMPACT INFORMATION SYSTEMS RESEARCH, NOT THEORY, TO ADDRESS GRAND CHALLENGES¹

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Exponential changes brought about by technological advances and increasing convergence of digital and physical technologies have moved our world into the realm of smart automation, artificial intelligence, and machine learning. "Big Data" generated from various objects and people leaving digital traces of their actions are powering the engines of the Fourth Industrial Revolution. This confluence of unprecedented developments in technology and data is generating an abundance of research opportunities in the Information Systems (IS) field. These developments are enabling us to expand the horizons of design science with innovative methods and artifacts to address many compelling challenges of these exciting times.

More than ever, we must move the IS field forward by focusing on solving the grand challenges facing society and avoid the current fixation on theory. IS has to lead with *high impact research*. We must create research programs that are interdisciplinary, multi-methodological, and, above all, programs that solve grand challenges and generate solutions that impact the world beyond academics. Knowledge will be advanced and new theorizing *may* occur. However, theory development must not be a prerequisite of the effort. To support this goal, the IS publication practice needs to be revised. When it comes to publishing, the design science paradigm has to be considered at face value: as the creation of solutions and artifacts, not advancement of theory.

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What is high impact research? While impact of research is defined in different ways, we choose to adopt the following broad and inclusive definition from the Higher Education Funding Council of England (REF 2014).

High research impact is "any effect on, change or benefit to the economy, society, culture, public policy or services, health, the environment or quality of life, beyond academia" (REF 2014, p. 6). To put it simply, **impact is the difference that academic research makes to the wider world**. It goes beyond citations in one's own academic field.

Examples of high-impact research in the sciences and medicine range from the human genome project to the invention of the internet and the World Wide Web. An excellent example of high impact research related to the IS field is the creation of the entity relationship model (Chen 1976), which became the cornerstone of the design and implementation of information systems in organizations and is used in several scientific disciplines as a standard to capture the abstractions of the real world into database structures.

Closer to home, the University of Arizona's MIS Department (UAMIS), founded in 1974, is considered one of the pioneers of the IS field. It is also widely recognized as a powerhouse of IS research that is relevant to practice, and one that has achieved wide prominence. Unique aspects of UAMIS research are (1) it has received sustained funding from a variety of government agencies and industry, (2) it has spawned several start-up ventures, and (3) it has resulted in impactful design science artifacts that stand the test of time. Nunamaker et al. (2017) describe 50 years of success

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delivering high-impact research to address problems that have immense practical impact. The Arizona way is based on the "big science" approach for high-impact research. It is built around comprehensive research programs that are conceptualized around grand challenges and big questions. These programs are long lasting, comprehensive, multidisciplinary, and multi-methodological. The measures that evaluate the difference that academic research makes to the wider world include grants and sponsored research totaling over \$200 million over the last 25 years, industry partnerships, patents, successful launches of start-ups, hundreds of Ph.D. dissertations, Master's (MS), and undergraduate (UG) theses, and thousands of publications in a wide variety of high impact journals in many different fields. Table 1 presents a summary of the various high impact research programs of UAMIS over the years.

Evolution of Research Program on Heterogeneous Data Integration over the Last 20 Years

To give readers a good sense of how high impact research programs evolve, we describe here one of the most successful of Arizona's programmatic, multiyear efforts.

With the advent of the internet and distributed computing, one of the grand challenges back in 2000 was to seamlessly connect islands of heterogeneous data. This gave rise to a programmatic research program that developed models, methods and implemented tools facilitating semantic integration of data from multiple sources. High impact research in this area ranged from developing ontologies to explicitly modeling the semantics of heterogenous datasets (Ram and Park 2004), automatically detecting semantic conflicts and embedding them into systems that were used in practice in large Fortune 500 companies (Park and Ram 2004), developing machine learning methods to identify semantic conflicts from heterogeneous data (Zhao and Ram, 2004), and adapting these models and methods for domains such as biology and medicine, formal modeling of semantics of time and space for heterogeneous data (Currim and Ram 2012; Khatri et al. 2014) and then extended into semantics of data provenance and its application (Liu and Ram 2017). Additionally, it has paved the way for design science coupled with data science to address problems in chronic disease prediction and management (Ram et al. 2015; Srinivasan et al. 2018; Zhang and Ram 2020). Much of this research was funded by grants from NSF, IBM, Raytheon, Ford, Office of Research and Development (ORD-CIA), NIST, and private foundations. It has led to adoption of many of the developed tools for use within the organizations funding the research and development.

This systematic program of research on semantics, heterogeneous data integration and development of tools was a key component of very large scale research program called the iPlant Collaborative (now named Cyverse), one of the largest interdisciplinary collaborative research projects (\$50 million) funded by the National Science Foundation (Goff et al. 2011). iPlant, a plant science cyberinfrastructure collaborative led by the University of Arizona, utilizes new computer, computational science and cyberinfrastructure solutions to address an evolving array of grand challenges in the plant sciences.

Lessons Learned from Developing Programmatic Research

Throughout our work, we have learned several useful lessons. Here we identify and articulate the recurring themes:

- 1. Theory development is not a requirement of high-impact research. Research is about creating knowledge and providing impactful solutions that address grand challenges. In some instances, theory validation is achieved, or pre-theory findings are uncovered, as a by-product of the overall research approach. However, there is no requirement for *a priori* theory that dictates the approaches to be used.
- Programmatic approach to research that starts with grand challenges and identifies subproblems to solve is long term; it is not opportunistic, rather it is thoughtfully and deliberately designed, and is facilitated by funding from a combination of federal and private sources.
- Research that addresses grand challenges is, by design, interdisciplinary and requires collaboration with many partners from different domains across the university campus and broader community.
- 4. Design of artifacts, systems, algorithms, and models is at the core of the multidisciplinary, multi-methodological approaches. These may lead to patents and to tech transfer and/or commercial ventures. They may also lead to direct impact to organizations that adopt the solutions and tools that are generated.
- 5. Rather than being opportunistic in acquiring a dataset and then developing a research question that can be addressed by the specific dataset, we advocate starting with a grand challenge and collecting various kinds of datasets to provide a 360 degree view of the problem and developing a solution.

Table 1. High impa	act IS Research Programs at the Univ	Practical Impact:	
Research Program	Grand Challenge Big Question	Start-ups, Patents, Industry Adoption	
Group Decision Support Systems	How to use technology to facilitate collaboration and decision making (pre-Internet)	GroupSystems launched and successfully commercialized.	
Semantic Data Integration	How to automatically connect and make use of heterogeneous data from disparate sources	Direct utilization of knowledge developed by industry sponsors.	
iPlant/Cyverse	How to design a robust cyberinfrastructure for scientific collaboration to address the grand challenges of plant science: feed the world, climate change impact, etc.	The collaboration platform, now named <i>Cyverse</i> , continues to be supported by NSF (now \$100M for more than 10 years) and successfully supports thousands of plant and bio scientists all over the world.	
Law Enforcement Cyber Security and Cyber Terrorism	How to monitor and detect criminal activities from several disconnected data sources	Start-up COPLINK, adopted by over 1000 Police Departments in US, acquired by IBM.	
Trust and Deception Detection	In a super connected world including IoT, how to use technology to enhance trust and detect deceptions in interactions and transactions	AVATAR, a kiosk packed with physiological sensors to detect human deception. Deployed by DHS at several locations, being commercialized. NeuroID start-up launched. Detection of anomalous and suspect behavior in human—computer interactions. Commercially used to detect fraud of applicants and insider threats.	

The Design Science Paradigm in the Era of Big Data and Data Science

Today we live in a world rife with data of many different kinds from objects and people leaving digital traces of their activities. Sensors embedded in objects—the internet of things (IOT)—have made the line between humans and physical objects all but invisible. Exciting new developments in AI and machine learning such as supervised and unsupervised techniques have started leveraging these digital traces. These are now combined to produce inventions such as self-driving cars, chat bots, and smart organizations and cities.

This world of smart automation and the Fourth Industrial Revolution offers us an unprecedented opportunity to transform the IS academic discipline to embrace programmatic research to tackle grand challenges, such as the development of future work environments to seamlessly connect humans and machines; the detection and management of chronic and infectious diseases, such as COVID-19; rapid drug development and deployment; protection of our social structures, for example our elections, from cyber-threats; devising alternative energy sources and their use; detection and prediction of weather shifts associated with climate change; design and operation of smart cities, smart buildings, smart campuses; AI interpretability and explainability: fairness, detection and

avoidance of bias, and transparency of machine learning algorithms.

As we have learned from the Arizona experience, at the heart of high impact research programs to address grand challenges is the *design* of solutions, artifacts, models and algorithms. To address the grand challenges of our times, design science has to be enhanced and will need to integrate with data science.

With its roots in Herbert Simon's work *The Sciences of the Artificial* (1996), design science IS research focuses on problem solving rather than explanation of phenomena. While design science research had been conducted in the IS field since its inception, in 2004 the design science paradigm was formalized by Hevner et al. (2004). It paved the way for broadening the IS field to technical research in the early 2000s. The design science research paradigm has primarily focused on systems design and implementation.

We need to examine the ties between design science and theory in the era of data science and Big Data. In a pre-Big Data era, Gregor and Hevner (2013) outline contributions to knowledge by design science and articulate that these are not necessarily theory-based or theory forming. Most knowledge contributions from producing artifacts and solutions can

create design guidelines that could be generalizable, but from our extensive experience with high impact research with a focus on solving problems, we believe that calling a wellestablished set of design guidelines a "theory" is more of a terminology issue.

In a comprehensive coverage of how Big Data interacts with IS research, Abbasi et al. (2016) provide specific ways in which traditional guidelines of design science need to be challenged and adapted when the focus is on artifacts based on data science. One of the main ways that Big Data based design science interfaces with theory is the pre-theory phase of emerging phenomena (Rai 2016), which relates to generating inductive knowledge from insights and uncovered relationships. In our experience with large high impact multi-year research programs that involve multidisciplinary, multimethodological approaches and extensive data sources, we uncover insights and relationships, which we value as potential pre-theory findings. These are important but they are typically not the main focus of the research.

The relationship between design science and theory is still largely misunderstood in the IS discipline. Anecdotal evidence abound that data science submissions are expected to follow theory development arguments by top journal reviewers. Saar-Tsechansky (2015) prescribes ways for authors and reviewers to address these gaps. We also think that despite efforts from the leadership in top IS journals to appoint design science and data science experts to the editorial boards, the number of submissions and the correlated number of acceptances of design science/data science papers is still very small.² Based on this analysis and our experience, we propose several calls to action to move the IS field forward.

Calls to Action

- 1. Create special "Grand Challenges and High Impact Research" tracks in the top journals of the field.
- 2. Develop long term programmatic research programs and identify problems and questions that can be integrated to lead to solutions for the identified grand challenge. To do this business schools and IS department need to foster a different culture of research that embraces several important components. These include encouraging inter-

- disciplinary research that has a longer term horizon, provide support and mentoring for grant proposals, and, valuing and providing incentives for publications in interdisciplinary journals in addition to the major IS journals.
- Develop interdisciplinary research partnerships among academic colleagues and get industry partners as well.
 This involves a lot of effort in learning each other's language and communication to resolve differences in cultures across disciplines and partners.
- 4. Create a plan for research publications from the start of the project. Federal funding agencies encourage research publications, however agreements have to be created from the start with industry sponsors to have them realize the importance of disseminating the research in the form of conference and journal publications.
- Plan on collecting data relevant to the grand challenge and developing data science methods and implementing them in a system to be used, evaluated, and validated with industry partners.

In conclusion, the IS field has a unique opportunity to lead by embracing a new approach for solving grand challenges of our times. While the suggested actions above depict a roadmap for the necessary programmatic research, the IS academic field needs to avoid the fixation on theory being a prerequisite, or an end result, for publication in top journals. Data science based design science research has to be judged by the criteria for evaluating grand challenges based programmatic research, as advocated by the National Science Foundation: intellectual merit and broader impacts on society.

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LET'S CLAIM THE AUTHORITY TO SPEAK OUT ON THE ETHICS OF SMART INFORMATION SYSTEMS¹

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Smart information systems abound today and are becoming ubiquitous. There is increasing public awareness that these systems have great potential for harm as well as good. Despite decades of work on the development and implications of information systems, the IS field is not yet seen as having a distinctive contribution to make to the discourse on the ethics of smart information systems. If these issues are to be addressed in a comprehensive fashion the IS field must claim the authority to speak distinctively on these matters.

The Ethics of Smart Information Systems Needs IS Attention

Smart information systems (SIS)—systems that incorporate artificial intelligence techniques, especially machine learning and big data analytics—have a significant and growing impact on our world. They raise possibilities of curing diseases, revolutionizing transport, optimizing business processes, and reducing carbon emissions. A prominent domain of application is healthcare, where SIS are set to revolutionize the ways doctors deliver services, the classification of diseases, the monitoring of patients' vital signs, and support for independent living (Haque et al. 2020; Topol 2019).

At the same time, SIS raise many ethical and societal concerns, ranging from worries about unlawful discrimination to the undermining of democracy. Many of these issues have

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been discussed since the start of digital computing (Wiener 1954), but the characteristics of SIS—such as their opacity, their apparent autonomy, and their data requirements—are generating new levels of concern. A case in point is the use of contact tracing apps during the coronavirus pandemic. Contact tracing has proven its effectiveness in disease containment for 500 years, but the application of advanced information technologies elevates public concerns about privacy, discrimination, and exclusion from essential public services to entirely new levels (Morley et al. 2020).

Ethical reflection on SIS is a prominent topic of research undertaken by several fields and disciplines. Among the most authoritative voices on SIS ethics today are philosophy, law, social studies, and computer science.

The discipline of philosophy has been home to ethical reflection for millennia and has proposed the major ethical theories for reflecting on moral questions of good or bad, right or wrong. These theories include ones that focus on duty (Kant 1788, 1797), consequences (Bentham 1789), and virtue (Aristotle 2007). Philosophers have actively developed bridges to technical disciplines in order to find ways of addressing ethical issues related to computing, many of which are relevant to SIS (Floridi 1999; Johnson 2001; van den Hoven 2010).

Legal scholars are also viewed as leading authorities on SIS ethical issues. Their most prominent focus is privacy and data protection, probably because of the significant amount of current legislation such as the European Union's General Data Protection Regulation or the U.S. State of California's recent

data privacy legislation. Machine learning-based SIS require access to large amounts of data for training and validation, which renders data protection issues pertinent (Kaplan and Haenlein 2019). However, those ethical issues of SIS that are of interest to legal scholars are by no means confined to data protection. SIS also raise numerous other legal questions, such as health and safety, liability, intellectual property, and human rights. One high-level human right, the right not to be unfairly discriminated against, figures prominently in the SIS debate, due to the possibility of biases in data or algorithms (Latonero 2018).

Social scientists are increasingly vocal in the current intense international policy debates on SIS. Political scientists have become active participants in the proliferation of committees and other bodies dealing with SIS policies and ethical codes (European Parliament 2017; Executive Office of the President 2016; HLEG 2019; House of Commons Science and Technology Committee 2016; Jobin et al. 2019). Other social scientists have become authorities on the new types of surveillance and business models facilitated by SIS (Zuboff 2019).

Not least in prominence in the discourse on SIS ethics is the ACM-affiliated FAccT (Fairness, Accountability, Transparency, see https://facctconference.org/) community that seeks to bring computer science expertise to bear on issues of SIS ethics (FAT/ML n.d.).

These intellectual communities (and a few others) are currently viewed as the leading experts on a subject matter about which IS scholars have much to say. Each community makes a distinctive contribution to discourse on SIS ethics by drawing on a unique disciplinary orientation, specific vocabulary, and methodological approach. At the same time, these discourses, singly and together, have limitations that IS scholars are well positioned to address. Philosophers, legal scholars, social scientists, and computer scientists often lack in-depth understanding of how SIS are developed and deployed in organizational and interorganizational settings and of the business and economic issues surrounding SIS deployment. In some cases, they also lack detailed knowledge of the underlying technologies. These limitations are understandable, but they are gaps that need to be filled. The IS field can and should become an authoritative voice on SIS ethics by actively addressing current gaps in SIS ethics discourse.

The Challenge to the IS Field

The IS field has the capacity and the knowledge base to overcome the limitations in current discourse on the ethics of SIS. The community of IS scholars has a long history of research that spans the full range of SIS topics: building SIS tools and techniques, applying SIS tools and techniques to explore application areas like health care or marketing, and researching the use of SIS tools and techniques by individuals, organizations, and interorganizational communities. This wealth of experience is unparalleled in other disciplines and offers the potential for unique insights into the ethical issues arising for creation and use of SIS. Furthermore, the sociotechnical orientation and business-economic foundations of the IS field provide the necessary background to fully appreciate the organizational and interorganizational context of SIS design decisions with ethical implications. This knowledge base therefore positions IS scholars well to take a leading role and acquire an authoritative voice on SIS ethics.

However, the IS field is not yet in a leadership role and not yet viewed as an authority with a distinctive perspective on SIS ethical issues. There are several reasons for this situation. The IS field does not have a unified orientation or approach to SIS ethics. IS scholars with an interest in ethics tend to focus on specific technologies/applications (e.g., big data) or topics (e.g., privacy) and tend to draw from other disciplines (e.g., philosophy, law, etc.) for conceptual frameworks. While such research is of course legitimate and can add to knowledge, it tends to reinforce the prestige of the reference disciplines, and it has not yet led to the creation a coherent and distinctive IS body of knowledge on the ethics of SIS. Without a coherent and distinctive perspective, approach, and body of knowledge, the IS field cannot come to be heard as having an authoritative voice on SIS ethical issues.

Toward Responsible SIS Research

The IS field can overcome these challenges and establish for itself a leading position in SIS ethical research by building and adopting an integrative and unified conceptual framework from which to approach SIS ethics issues. We call this framework "responsible SIS research." This framework builds on the work of the other disciplines discussed earlier (e.g., philosophy, law, social science, and computer science), but integrates them and brings in other dimensions that play to the IS field's strengths, such as interorganizational dynamics, business models, and technology future studies.

A starting point for building such a framework is a set of four "core values" that draw on different aspects of ethical discourses and have relevance to SIS ethics.

The first core value is that of compliance with the established and formalized rules that express ethical positions.
 Such rules can be found, for example, in research ethics and its institutional review processes, but also in other

types of regulation or legislation, such as data protection law.

- Beyond compliance there is the core value of acceptability, which goes beyond compliance and focuses on areas that remain open and contested. A key question of acceptability is which possible options are best aligned with ethical principles and preferences. This question will often not have a clear and universal answer, but asking it enables researchers and practitioners to reflect on how they can avoid doing harm.
- The third core value is what we call proactivity, namely
 the active intention to do good, to improve the state of
 the world in some ethically relevant way. Whereas compliance and acceptability are reactive and seek ways to
 align with existing rules and principles, proactivity aims
 to go beyond the current state and actively shape future
 social reality.
- One key question that arises in this context is how one could know whether one state of the world is better than another and how answers to this question can be integrated into SIS research and practice. This is an important aspect of the fourth core value of *reflexivity* which, based on the Socratic maxim of "know thyself," aims to stimulate critical reflection on all aspects of research and practice. Reflexivity is also crucial to better understand and develop the other core values.

The responsible SIS research framework must not be misunderstood as an attempt to establish a new ethical orthodoxy, in which the IS field becomes the arbiter of what is good or bad, right or wrong and can dictate to developers, deployers, or users of SIS what to do. It is the nature of ethical questions to remain uncertain, contested, and dynamic. Unequivocal ethical positions on SIS will be the exception rather than the rule.

Instead, we put the responsible SIS research framework forth as a systematic and integrated way of thinking about and approaching SIS ethics, which is enriched by the IS field's unique competencies and aims to provide insights across the range of SIS technologies and applications. The framework can help IS researchers develop sensitivity to topics, issues, questions, and theories relevant to the ethical issues of SIS. Not every research project will need to address all aspects. However, by adopting a broader framework, the field as a whole will be able to assess progress and identify gaps. It provides a setting in which intelligent conversations can be held about the desirability and acceptability of a key technological development of our time. And it offers the opportunity for the IS field to come together and take leadership

based on its core competencies that will benefit other disciplines and society as a whole.

Conclusions

This provocation points to an important area where the IS discipline fails to perform to the best of its ability. We believe this situation is detrimental to the IS field, and we call for the IS field to lead the discourse on the ethics of SIS. SIS are a crucial set of technologies and applications that will drive many future technical, organizational, and societal developments. They are central to the IS field, which cannot afford to be marginalized in this discourse either from a research or a practice perspective. We suggest that the way to avoid being irrelevant is to develop and communicate a distinctive take on SIS ethics that reflects the field's distinctive competences in the domain of SIS.

The IS field has all the ingredients needed to provide leadership, but this needs to be recognized and acted upon for change to occur. We have briefly sketched what we believe will be key components of the framework for responsible SIS research, but the task of defining it in detail is still ahead of us. Further development of the framework will itself need to draw on the four core values, that is, it will need to be compliant, acceptable, proactive, and reflective. It will need to draw on expertise within the field and reach out to external experts and stakeholders of SIS more broadly. It will allow for an emphasis on what is distinctively IS in our analyses, for example, deep knowledge of organizational and economic dynamics, as well of technology.

Accepting our provocation will require the IS field to take the ethical issues of SIS seriously and to come together to create substantive and methodological insights into the ethics of SIS. The prize for accepting this challenge will be the opportunity to take a leading role in a crucial societal debate that is shaping how modern societies will make use of the next generation of digital technologies. We strongly believe that it is in the best interest of the IS field and its individual members to accept the challenge of adopting and building out a distinctive and unified framework for responsible SIS research and thereby contribute to making the world a better place (Walsham 2012).

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ALL INFORMATION SYSTEMS THEORY IS GROUNDED THEORY

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Everybody does grounded theorizing, but not everybody knows that they are doing it. Ask yourself where theory comes from. You might have taken a doctoral seminar on research methods that had a session devoted to this topic, where you learned that, usually, theory comes from prior theory in your field (e.g., IS), other fields (e.g., economics, sociology, or psychology), or both. Obtaining theory from existing theory might be implicit if your get your theory from your advisor. If your methodological training included qualitative methods, you also learned that theory may come from data, especially when it concerns poorly understood phenomena. Finally, some of us might have learned about a possibility of "in between," where parts of the theory are drawn from prior theories and parts from data. What I will argue in this piece is that IS theory should be of this "in between" variety and should be developed through a particular process, namely, through grounded theorizing. Moreover, I will illustrate how two of our field's prominent theories, TAM and IT productivity, were developed by following the logic of grounded theorizing, even if not implementing the formal procedures of this research approach. I will argue that we will all benefit if we embrace this logic in our work and publications.

In graduate school, I became familiar with grounded the theory method (GTM) as part of my preparation for the qualifying exam. The original book on this topic (Glaser and Strauss 1967) gave me a remarkably clear guide on how to go about conducting qualitative research using both data and extant theory. Yet, as I was trying to publish my papers, I was told that the "in between" approach to theory development was not well-accepted in journals and that the GTM label

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created a lot of confusion and misconception (Urquhart and Fernandez 2006; Walsh et al. 2015). It was best to claim that either my key theoretical insights came from extant theory or that they were developed in a grounded way from the data. I was confused. I knew that both played a role in my research process but ended up writing my papers to fit reviewers' expectation around boxes that they needed to put qualitative research in (Sarker et al. 2018). In one paper, I stuck to the claim that GTM was used, introducing external theory only after the data was presented (Levina and Ross 2003). In another, I explained that my work was guided by an *a priori* sociological theories, even though I knew that I brought in relevant parts of these theories specifically to explain the most interesting emergent themes in the data (Levina 2005).

Twenty years after submitting my first *MIS Quarterly* paper (Levina and Ross 2003), I still find myself debunking myths about GTM, justifying why I stuck to it, and explaining why impactful IS theories are likely to be developed in this way, even if only some of us are admitting it openly in writing.

GTM: What Is It?

Briefly, GTM was formalized by its founders, Glaser and Strauss, as a response to frustrations each of them had personally experienced in legitimizing their approach to theory development. Glaser conducted a quantitative dissertation on careers of scientists in organizations by following research methodology advocated by his advisor, Paul Lazarsfelt, who is acclaimed as the co-founder of mathematical sociology. Glaser, however, found that the most interesting part of his data was in analyzing survey responses not directly corresponding to his hypotheses—something many of us can relate to. He was struggling, however, in legitimately writing about

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theoretical insights that were not derived on the basis of *a priori* theory. Later, Glaser came across Strauss, who was a qualitative interpretive researcher also experiencing similar challenges in publishing his work. As Glaser writes,

Part of the trend (in 1960s) toward emphasizing verification was the assumption by many sociologists that our 'great men' and theorist forefathers (Weber, Durkheim, Simmel, Marx, Veblen, Cooley, Mead, Park, etc.) had generated a sufficient number of outstanding theories on enough areas of social life to last for a long while (2012, p. 1).

Why was following the traditional route not acceptable to Glaser and Strauss? They felt that bringing an abstract theory to write about contemporary phenomenon in a deductive way without knowing much about this phenomenon resulted in poor insights!

Learning about the phenomenon in a haphazard way, however, would not solve this problem either. To provide theoretical insights on a contemporary phenomenon, one needed to get close to this phenomenon by collecting data, be it qualitative or quantitative, so as to investigate it systematically and critically. This involves focusing on problems or puzzles in data and looking for consistent patterns, themes, and explanations addressing these problems. Formalizing GTM meant outlining how one should do this analysis systematically, including how to find interesting questions in data, define a sampling strategy, and conduct systematic comparisons in data to find insightful patterns and verifying their consistency. The next step was to abstract out conceptually on the basis of these patterns. Contrary to the popular myths about GTM (Urguhart and Fernandez 2006), extant theory plays a critical role in this process as it helps challenge emergent findings, suggests ideas for new data collection, and enables better conceptualization of emergent patterns. At the same time, the researcher must work hard to resist the temptation to "force" data to fit their own or reviewers' favorite theories (Seidel and Urquhart 2016).

One of the biggest challenges of GTM is how to avoid conceptualizing patterns in data in a way that results in theories that are already well known in the field. First, addressing a really challenging practical problem with a good conceptualization is often a worthwhile pursuit in its own right even if it does not result in a novel theory. Yet, we are in academia, so Glaser and Strauss offered us guidance on developing a formal theory that is likely to result in a publishable contribution. They argued that in abstracting out from a given context, *one should apply many relevant existing theories* to the emergent patterns in data (Charmaz and Bryant 2011).

This exercise can provide useful insights on the data or lead to further data collection. If existing theories do not match the core emergent patterns or need to be extended to match them, it opens up room for a novel theoretical contribution. This is likely to happen if the phenomenon at hand is relatively new or poorly understood with prior theories—something typical of many IS phenomena. While in the original writings on GTM, the analytical focus of the researcher was supposed to stay on the struggles experienced by participants in the study, subsequent writing on the GTM broadened the method and suggested that the focus of investigation could be based on researcher's own interests and passions (Charmaz 2006). For those of us interested in making a novel theoretical contribution, we should focus our investigation on unresolved struggles in our academic discipline (Davis 1971). Whether the research focus is driven by practical or academic problems, if GTM scholars want to develop insightful theories, they must be extremely well-read. Exposure to a wide set of prior theories reduces the chance that a scholar will narrow in on a problem that is easily addressable by an extant theory. At the very least, addressing this problem would require integrating multiple theories, potentially producing an interesting theoretical contribution in its own right.

I posit that IS scholars who develop impactful theoretical insights on contemporary phenomena have a very similar approach to theory development even if we don't call it GTM. We may learn about a new technology-related problem from exposure to specific organizational settings, public media, academic publications, or our daily lives, or we make come across data sets that present puzzles. We then start wondering what is going on here. What do we know about similar phenomena? What is different? We narrow in on particular aspects of this phenomenon that are unfamiliar. We start thinking about what dimensions should guide our investigation based on our prior knowledge, prior theory. If we do hypothesis-testing research, these dimensions need to be fixed fairly soon, so a priori theory is brought to bear to design survey questions, extract certain variables from archival data, or set up experimental conditions. If we do exploratory quantitative and, especially, qualitative research, we may go into the field and seek data on the new phenomenon. We start seeing some patterns in data that we deem interesting or puzzling. We often have our favorite "grand theory," which may lead us to see the new phenomenon immediately through the lens of this theory, or we may be able to hold off putting this lens on and explore the phenomenon a bit further. All combinations of the above are possible, but, if we agree that IS research is a research on contemporary phenomena, then we all—whether we are doing quantitative or qualitative scholarship—do some degree of grounded theorizing.

I will discuss the development of two highly impactful theories in our field that were published as hypotheticodeductive theories and demonstrate that the development of both of these theories followed the principles of grounded theorizing, even if not reported as such in publications.

Technology Acceptance Model

The first theory is the famous technology acceptance model (TAM), which, in its published form, appears to have been built deductively based on *a priori* theory in psychology. It may be a surprise for many to learn that TAM has its roots in an inductive qualitative research project conducted by the theory's founder, Fred Davis. The story is documented by Davis in a book chapter (2006). Before joining his Ph.D. program, Davis worked as IS development and implementation consultant in late 1970s. He observed an empirical puzzle: In spite of the growing power of computer-based decision support models, the software implementing such models was rarely used by practitioners (*ibid.*, p. 395). This observation motivated Davis to pursue his Ph.D. starting in 1980.

As part of his studies, Davis "interviewed numerous end users regarding their acceptance or rejection of various technologies" (*ibid.*, p. 396). In his recollection of how the key theoretical insight crystalized, he writes:

One day I was returning by helicopter from New Hampshire to Massachusetts after completing a day of interviews. Watching a brilliant sunset, a simple but important insight occurred to me. Although interviewees expressed it in many different ways, the dominant reasons they cited for accepting or rejecting a new system at work strongly hinged on two issues: how useful and easy to use they found the system to be. We landed, the attendant opened the helicopter door, and my folder of interview notes spilled out. While the spinning rotor blade scattered yellow sheets all around, I was amused instead of concerned because the simple insight about usefulness and ease of use was now in my head. I recovered most of the interview notes, but never typed them up. Instead I turned my focus to the literature on MIS attitudes and implementation success to craft the idea into a dissertation topic (*ibid.*, p. 396)

Comparing this data-driven insight to published research in IS, Davis had noticed that dominant IS theories in the early 1980s emphasized top management support and user involve-

ment as the two most important factors driving implementation. These two theories did not jell with his data, but he discovered a significant body of research on "MIS implementation attitudes." This work could be related to the emergent concepts of "usefulness" and "ease of use." Yet, published studies in this area produced mixed results in terms of the relationship between implementation attitudes and system adoption. Why did published result not fully align with strong patterns in the qualitative data?

To address this puzzle, Davis drew on existing theory in psychology that focused on attitudes to further theorize emergent concepts. Psychological theories predicting behavior on the basis of attitudes were gaining popularity in organizational scholarship, and the theory of reasoned actions was one of them. It was a good fit for explaining the role of attitudes (perceived usefulness and ease of use) in producing behavior (system adoption). Davis also had developed deep expertise on this theory, having worked on measurement issues pertaining to this theory in his pre-thesis work (Warshaw and Davis 1985). This is how TAM emerged: by bringing an existing theory to formalize grounded emergent theoretical themes. The combination helped Davis develop new empirical instruments enabling quantitative data collection.

A fascinating part of the story is that the software firm that Davis was working with had just suffered a costly software product adoption failure. This presented the young researcher with a *core concern* of how to improve product development so as to prevent such failures in the future. This led Davis to develop the last piece of the puzzle: the administration of the TAM instrument with video prototypes to avoid costly investments into software that users would never use. The approach had proven very relevant to practice.

The rest is history: over 150,000 Google Scholar citations to Fred Davis' collective works and unprecedented intellectual influence on our field. Davis writes,

TAM is often mentioned as an example of a true IS-specific theory, which is ironic given the extent to which TAM was derived from reference field theory such as the theory of reasoned action (*ibid.*, p. 399).

What is also ironic is that TAM, perhaps the most popular theory for hypotheticodeductive scholars in our field, is actually a grounded theory. Yet, because it was written as a hypotheticodeductive theory that is based on extant theory in psychology, it is hard to recognize that its original insights came from our discipline's practice: IS implementation failure. Perhaps had Davis reported in his original publications the inductive parts of his theory development, our field

would have moved faster toward unpacking the role of organizational and personal factors in influencing key TAM variables, an extension of TAM that took a decade to develop (Venkatesh 200).

IT Productivity Theory

The second theory I would like to discuss does not have a familiar abbreviation, in the original publications it was referred to as "the theory of IT and organizational architecture." While the name is not familiar, the theory is well-known to IS scholars as a key theoretical advancement in resolving the IT productivity paradox. The theory is outlined in a series of papers related to Lorin Hitt's thesis (Hitt 1996) that were published in late 1990s and early 2000s. The theory posits that firm-level investments in IT are likely to be more valuable when coupled with other organizational transformations such as decentralization of decision authority, emphasis on subjective incentives, and increased reliance on skills and human capital (Hitt and Brynjolfsson 1997). Hitt's collective works have resulted in over 30,000 Google Scholar citations to date.

At the time when Hitt started his graduate program, his advisor, Erik Brynjolfsson, was focusing on untangling the IT productivity paradox, which refers to the statement that "despite enormous improvements in the underlying technology, the benefits of IS spending have not been found in aggregate output statistics" (Brynjolfsson and Hitt 1996, p. 541).² Paradoxes found in primary or even secondary data are excellent opportunities for grounded theorizing. Brynjolfsson and Hitt started exploring this empirical paradox by considering three ideas: (1) returns on IT investments may be reflected in performance measures other than the usual productivity measures; (2) IT investments may need several years to result in a productivity gains; (3) returns on IT investments may be uneven across firms and industries (Brynjolfsson 1993). The GTM approach would suggest collecting data to explore these possibilities. This is exactly what they have done by analyzing a quantitative data set they already had on hand with a different measure and by subsequently collecting longitudinal firm-level data (Brynjolfsson and Hitt 1996; Hitt and Brynjolfsson 1997). Analyzing the existing data set with new measures resulted in two new puzzles. The analysis revealed, first, that contrary to the IT productivity paradox, the rates of return on IT were actually quite high, and, second, that there was a great degree of heterogeneity where firms making similar investments got very different outcomes.

Resolving these puzzles took qualitative insights, new quantitative data, and several extant theories. The qualitative insights came from Hitt's prior experience in management consulting, teaching cases focused on the idea of business process reengineering (Hammer and Champy 2003) and qualitative research studies of IT in organizations (Orlikowski 1992; Zuboff 1988). They all pointed to the idea that when IT was introduced into organizations, it was likely to lead to organizational change that might, in turn, lead to higher performance. To theorize what kind of organizational changes may positively influence performance, Hitt turned to the human resources literature, which at the time focused on the new idea of innovative work systems (Ichniowski et al. 1996). The final piece of the puzzle came from the newly developed economics theory of complementarities by Milgrom and Roberts (1995), which formally modeled how certain inputs into a firm's production function were complementary, while others created conflicts. Putting all these insights together, Hitt and Bryjolfsson pursued new data through a survey instrument focusing on IT investments and HR practices at the firm level. The survey data combined with firm performance data indicated that higher returns on IT investments were gained primarily by firms that also invested in transforming their human capital management practices.

While GTM was not explicitly used by this research team, the theoretical insights were developed in accordance with core GTM principles: (1) getting in-depth understanding of the phenomenon through data and finding patterns, (2) identifying key concerns or puzzles, (3) proposing how these concerns can be addressed using patterns in data and extant theories, (4) pursuing more data specifically to verify or challenge these emergent theories, and (5) continuing this process until a satisfactory answer is obtained. Yet, published papers based on this project present a "cleaned up" and "modularized" story of theory development. Hitt and Brynjolfsson's 1997 paper comes closest to explaining how various conceptual pieces end up fitting together, but even this story does not reveal the full picture. Instead of starting with the IT productivity puzzle as the motivator, it begins by asserting a correlation between flattening of organizational hierarchies and the rise of modern computing technology (Hitt and Brynjolfsson 1997, p. 82). The paper then concludes by saying that organizational IT investments are associated with organizational architectures that include decentralized authority. The reader unfamiliar with other papers from this project would make an inaccurate conclusion that firms that make higher investments in IT also change their organizational architectures; whereas, the takeaway of the overall

²The story related below is based on my personal correspondence with Lorin Hitt (May 2020).

project was that *only* firms that change their organizational architecture when they invest in IT reap performance benefits from their IT investments.

The Way Forward

Given the profound influence these two theories had on our field without referring to GTM, or perhaps even being aware of Glaser and Strauss' writings, what is the added value of engaging with this formalized theory development method? Indeed, research processes may not require formal codification as long as the knowledge of the process can be learned through tacit aspects of academic life such as advising, workshops, and published papers. The knowledge, however, is not readily accessible in our field. Had I not come across the back stories about the development of these two prominent theories through personal communication, I would have inferred that they were the result of following the process of identifying a theoretical gap in the published literature, filling this gap by applying extant theories from reference disciplines (psychology and economics respectfully), and testing these "ivory tower" theories with quantitative data. This would have been quite far from the reality.

Given that science is filled with judgment calls, a hallmark of scientific writing is that we clearly explain and justify the choices we make so as to enable the readers to judge the appropriateness and implications of our choices. Being transparent in this way should apply to every part of the paper from how we formulate problems to the conclusions we draw. If we present post hoc rationalized stories, the readers could make incorrect inferences about the meaning of our research. Yet, our publications force us to comply with a set of fairly rigid expectations about the form our theory development section ought to take, especially within the hypotheticodeductive tradition. Even for scholars who do not follow this tradition, such as myself, only a handful of theories can be published in the way that they were really developed. This is because there is a great degree of confusion among interpretive scholars about the use of existing theory as a valid and necessary part of grounded theorizing (Charmaz and Bryant 2011; Urquhart and Fernandez 2006).

Our field would advance the state of theory building if we asked the authors to demonstrate rigor in their theory development process beyond weaving together a logically convincing sequence of abstract arguments. Because our field is focused on contemporary phenomena, such rigor can be achieved by following GTM principles. I have provided a short summary of such principles, but it would take our field some time to properly adapt these *conceptualization process*

rigor criteria to suit different types of IS scholarship while not making such criteria overly rigid. The first step toward achieving this goal is to ask the authors to report their theory development process. Such reporting does not need to be voluminous as it can be integrated with the actual presentation of the theory, perhaps even making such presentation livelier. As we are working toward these changes, theoretical advancement in our field would benefit from exposing all students, without regard to their disciplinary training and favorite methodological tools, to grounded theory building ideas. This should in no way replace extensive exposure to existing theories in our own and other fields, but, instead, should be seen as a promising way of building theory of contemporary phenomena for long-lasting impact.

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WHO NEEDS THEORY?1

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Almost everyone needs theory except seers and dataphiles. Seers include Nostradamus (Michel de Nostredame, 1503-1566), whose 1555 bestseller Les Prophéties has never been out of print. Some say it predicted COVID-19: in a twin year (2020) a queen will arise from the east spreading a plague that will bring dust to a country of seven hills. Being a seer is nice work, if you can get it, but seers are not now revered by academics. You seldom hear academics argue seriously that they could use Nostradamus about now. The prophetic gift is rare and few can convince anyone that they have it. On the other hand, dataphiles are popular in this age of "big data" and "data science," despite the fact that jumping from data to knowledge on Ackoff's (1989) DIKW pyramid (data, information, knowledge, wisdom) is difficult. It is even harder to skip both information and knowledge and go straight from data to wisdom.

Data are needed, but so is theory. The persistence of that need is a provocation. Burdensome calls for theory do not attract people. Theory should be attractive so people pursue it for positive reasons rather than fear of being yelled at. This paper is about enlightenment from relaxing the constraints. It is addressed to researchers, but also to practitioners like Florence Nightingale, a practicing nurse and statistician whose analyses and visualizations altered medical practice (Cohen 2006). Her data stimulated theory that showed Nightingale how to use data to change the game. The paper relaxes the distinction between research and practice and provides perspective on getting published for being useful.

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Nullis in Verba

The right balance between theory and data was summed up in the 17th century when The Royal Society of London for Improving Natural Knowledge (now simply The Royal Society) adopted its motto *nullius in verba*, Latin for take nobody's word for it (Royal Society 2020). Since somebody's words in publication are often sources, the *quality* of the words matters. An 18th century Enlightenment insight was that good words use systematic mental schemes (theory) to account for facts (data). Data explained by theory can be thought of as knowledge (Oxford English Dictionary 2020).

The review process sometimes dwells on "good theory," "bad theory," and "no theory." Theory is too often overblown, used as a scourge and grounds for rejection. Yet, theory is the inquisitive person's friend, facilitating reasoning by analogy and improving efficiency. When B is akin A, knowledge of A might ascribe to B. Knowledge reuse means less reinventing the wheel. By reasoning from particular to general and back, things become understood backward and forward. Theory facilitates disciplined imagination (Cornelissen 2006). Theory has many synonyms: conjecture, postulation, hunch, and so on. Without data, theory is unmoored.

Data cannot stand alone, either. Data collection often depends on theory (e.g., sampling theory, experimental design theory, grounded theory). Theory is usually required to normalize data taken from different studies, collected by different instruments, or if collection follows different protocols at different times. All data are from the past. Theory is required when data are used to predict the future. If the past sometimes embodies problems (e.g., discrimination on race, ethnicity or sex) theory allows such problems to be addressed. Theory underlies algorithms and other analytical techniques. Theory and data are both essential to *nullis in verba*. Original data establish veracity and data are used to show replicability that tends to prove results are not simply someone's word.

¹This paper was invited and editorially reviewed by the Special Issue Senior Editors: Andrew Burton-Jones, Brian Butler, Susan Scott, and Sean Xin Xu.

Yet theory demands can be off-putting, even if wellintentioned. The core recommendation of this paper is to relax expectations to make inquiry at once easier and more disciplined. Theory is insight, even if the highest and best form of insight. Backed up by data—observations collected via senses, even when augmented by microscopes, telescopes, sensors, etc.—theory takes on persuasive power. Often the magic is the fine line between insight and observation, where people say, "Oh, I see." This is facilitated by the right presentation of data that produces insight. A good example is Mendeleev's Periodic Table of Elements. It that does a number of important things simultaneously, showing the relationship between observation (e.g., an element's atomic number) and what that number implies (e.g., elemental behavior seen in the columns). Weick sets the stage for Cornilissen, emphasizing imagination and insight in theory development, and later noting that insight and observation form understanding (Weick 1989, 1995). This is a delicate dance.

The Pas-de-Deux

Pas-de-deux is French for stepping together, a dance for two. In this case, insight and observation come together in a pas-de-deux of successive approximation toward one or more goals over time. The goals might change as more is learned or new interests emerge. Contrary to what was once thought and acted on, insight seldom involves a flash of creative genius (Seabrook 2008). (A U.S. Supreme Court opinion written by Justice William O. Douglas (1941) articulated this flash of genius.) Insight is everyday. We concentrate on the main effects to get rid of unhelpful information. This gives insight room to work until things stand to reason; that is, insight and observation correspond. Inquisitive people reflect on what they observe. They use their training in insight (including in theory) to spot correspondence. They match insight and observation. Becker (2008) has some tips for this.

Some of the deepest insights come from comparatively mundane observations. For example, an inquisitive person viewing a modern steam engine might wonder about the purpose of the condenser, which is comparatively boring compared to the eye-catching firebox and moving parts. The condenser embodies physicist Sadi Carnot's insight that all heat engines exploit a *cycle* (now called the Carnot cycle) between hot and cold. Steam engines are heat engines, as are nuclear power plants and internal combustion engines in cars. This cycle is used to determine theoretical limits for all heat engines. It came a century after the condenser, suggesting that scientist Lawrence Joseph Henderson was onto something when he said in 1917 that science owes more to the steam

engine than the steam engine owes to science (Gillispie 1960). Insight does not require a flash of genius that occurs in a moment, but often comes over time.

Routine observation is important. For example, it is routinely noted that information technology (IT) is spreading systemically into nearly everything. This might be an animating force for reflection on information systems. Reflection accelerates learning, especially through exploration of anomalies. Anything that violates expectation (e.g., the laws of nature) yields new insight and knowledge (Hume 1748). Over time confusing terminology gets sorted out and observation and insight come together. Scholarship expects both. Two examples illustrate this.

Autonomous Vehicle Ubiquity

Sometimes more than one thing is being said. It pays to sort things out. A good example is the prediction that autonomous vehicles (AVs, also called self-driving vehicles that use computers, sensors, actuators, and algorithms to replace human drivers) would be ubiquitous by 2021. Ubiquity might happen someday, but timing of the claim (by 2021) failed. Why? AVs have long been around on rails (elevators, subways, people movers, etc.). AVs on roads are feasible. It is less feasible that road-based AVs will quickly change a large and complicated motor transport system that evolved and became deeply embedded over more than a century.

The existing motor transport system gives control of vehicles to human drivers, individuals who operate under rules, regulations, social conventions, and relationships with third parties (e.g., insurance, law enforcement). These individuals follow "rules of the road," interacting with and coordinating driving infrastructure (roads, signage, control systems like traffic lights). They use navigation aids (e.g., maps, the Global Positioning System, cellular telephone tower triangulation). Humans are animals, able to spot and anticipate animal behavior in other animals (humans, deer, etc.). Infrastructure is predicated on humans. Can humans and AVs share infrastructure simultaneously? This is not answered. To achieve ubiquity humans might have to build an "overlay" network that separates human-driven vehicles from AVs. This is not merely an engineering problem.

There are many unknowns. Will humans tax themselves to pay for expensive road infrastructure upon which they cannot drive? What if some (e.g., driving-age youth) demand the "right to drive." Insurance companies have played a major role in motor transport evolution, but are largely silent on AVs. Declining news on AV advancement has been matched

by increasing demand for human-driven electric vehicles (EVs). Perhaps human-driven EVs do not require the infrastructure transformation of AVs. Beyond AVs, can humans be substituted for by embedded algorithms (e.g., Brynjolfsson and McAffee 2014)? That hope is more belief than reasoned argument. Throughout the history of technology, supplementation has been as common as substitution due to the power of incumbency, installed base, path dependency and so on. Supplemental driver assistance is becoming ubiquitous; substitution takes longer and might not happen.

Given the sheer size of motor transport in the United States, it makes sense to consider scale in the timing of ubiquity. The average age of motor vehicles in use is now more than a decade. Few can be used as AVs. It will take years to replace non-AVs with AVs or find an alternative to replacement. U.S. Department of Transportation demonstration AV projects conclude the AV concept is in principle feasible, but cannot yet be scaled. If AVs must be linked to roadways by wireless signal, retrofits will cost many millions of dollars per mile. Even if such costs are reduced by 90%, the number of miles of U.S. road would make the cost of a full retrofit close to 2019 U.S. GDP. It is already politically difficult to agree on repairing existing infrastructure. Deciding who gets what improvements will be more difficult.

What About the Toilet Paper?

Insights are usually first used to help explain observations, but they can go beyond to shape the search for future observations. Shoppers observed short supplies of toilet paper on retail supermarket shelves when COVID-19 hit the United States. Toilet paper demand, like food demand, is steady. Short supplies were most likely explained by changes in consumer behavior (e.g., hoarding), or supply/demand chain processes. Commercial supply chains shifted to residential, and toilet paper returned to supermarket shelves. This suggested a supply chain transition was too slow to "keep up" with a fast demand shift caused by public health stay-at-home directives. In competitive markets sellers cannot increase revenue by raising prices or increasing sales. They can enhance profitability by reducing costs through things like tightly coupled, lean supply chain management that reduces storage resources and excess inventory. These used to provide the "slack" that covered the problems of disruptions.

Sometimes insights suggest further consideration. Tightly coupled supply chains are increasingly part of systems that Integrate logistics, payment, compliance, shipping, inventory control, and point-of-sale, often run over the Internet. They replace weakly coupled interorganizational systems (IOS)

using Electronic Data Interchange (EDI). They are now common in industrial, commercial, and government functions. For example, utilities (water, sewer, electricity, natural gas, etc.) often use computerized SCADA (supervisory control and data acquisition) systems for routine operation.

The pas de deux: observed shortage of toilet paper triggered insight about the shortage. It also triggered thinking about the consequences of disruption more broadly. Sociologist Charles Perrow (1981, 1984) suggests that tightly coupled systems are prone to "normal accidents." This insight helped explain the loss of the space shuttle *Challenger* (Vaughn 1996). This insight can be applied broadly, even to higher education where there are tightly coupled systems between instructors and the registrar, learning management, video conferencing, and an academic calendar grounded in an agrarian-based production system that starts in the fall after harvest and ends in the spring before planting. The pas de deux enables the inquisitive to "go beyond" the observation.

Getting Published and Promoted While Making a Meaningful Impact

Promotion in higher education requires publication, which in turn requires reviewers and editors unconvinced by seers like Nostradamus. Reviewers and editors prefer a foundation of prior insights and new observations that lead to additional insights. This is true even in the era of "big" data run through "scientific" analytical tools. Good accounting of prior insights and careful presentation of the contributions of new insights is needed to keep the conversation going, or in rare instances, bring the conversation to a close by settling the matter.

Publication value does not come from being formulaic. Still, the customary formula (motivation, literature review, methods, data, analysis, discussion, conclusion) can be useful. A check-list can help inquisitive people organize and express their insights and say why more than just their word backs them up. Still, no amount of revision will save work that lacks insight. It might be published, but will have little impact. Successful publications match old insights and new observations to create new insights. This remains true even in fields with division of labor (e.g., theorists and experimentalists). People are seldom promoted for adherence to canonical rules; in fact, some violate the canon and are still highly regarded for their insight. The canon helps organize and communicate insightful work. A check-list occasionally helps inquisitive people get back on track after wandering off. But nothing substitutes for insight.

Conclusion

Theory starts with insight. Relax constraints, concentrate on insight first and theory later. This can make it easier to do theory. Inquiry is a kind of work that some people do. By paying attention to insights and making observations to produce more insights, inquisitive people stress-test the match of insight and observation to form new insights and shape further observations and insights. Some insights become theory. Inquisitive people do not worry about that right away. Most observation and insight goes on the shelf, to be written about later.

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