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20th Anniversary Invited Article

Interesting, Important, and Impactful Operations Management

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Abstract. Operations management has evolved since the founding of *M&SOM*: new departments have been created in our journals, new tracks have been established in our conferences, and new methodologies have been adopted in our research. Are these changes good for the field? To some, they seem detrimental, yielding a fragmented community that does not always speak the same language nor interact in any meaningful way. Others celebrate our expanded diversity and the new areas of research that it opens up. We argue that neither group is entirely wrong, nor entirely correct. Like the latter, we argue that we must contribute to a growing set of domains using all possible tools of inquiry. But sharing the concern of the former, we view fragmentation as a symptom of a problem. To get out of its rut, to have greater impact, the field needs to ask questions that are important and provide answers that are interesting. In particular, we should (i) avoid the trap of specificity (excellent answers to narrowly defined questions), (ii) expand our horizon beyond our (relatively) small field (connect and actively engage with diverse audiences), and (iii) be bold to pioneer new areas of inquiry. Operations management is at the heart of many of the big issues in society today, and we should be (and can be) central to the conversation.

History: This paper has been accepted for the *Manufacturing & Service Operations Management* 20th Anniversary Special Issue.

Keywords: operations management • fragmentation • M&SOM • new research areas • innovation

1. Introduction

In June of 1996, speaking at the MSOM conference in Hanover, New Hampshire, Professor Lee Schwartz updated participants on the progress toward the creation of *M&SOM*. At that time, the MSOM conference had a single track, and it was preceded by the single-track multiechelon inventory conference. Fast forward to 2019. The last MSOM conference at the University of Texas at Dallas featured 12 tracks with sessions on sustainable operations, healthcare management, revenue management, behavioral operations management, empirical operations management, technology management, and the IS/OM interface, to name just a few. None of these topics would have been part of the 1996 MSOM program. What was a common multiechelon conference, now features 5 tracks organized by Special Interest Groups, all with nonoverlapping topics.

Long gone are the days when we all seemed to work on inventory and supply chain management, or queuing. The same story can be traced to publications in *M&SOM*: Dai et al. (2019) discusses multiple forces that have shaped the evolution of the *M&SOM* publications including economic, social/environmental, and technological forces; and Song et al. (2019) uses text mining to measure the evolution of topics within

the domain of capacity and inventory management. We are now more diverse methodologically, topically, and geographically. Is this a good thing? To us, it is obviously good. Politically correct platitudes aside, our expanded scope connects us to a greater set of issues, which can only create more opportunities for breakthrough research. But it has led to fragmentation—the field seems to operate as a collection of independent islands speaking different languages. And that is not good.

To us, the solution to our funk is threefold. First, we need to be vigilant of the trap of specificity, which leads to work that is neither interesting nor important. A high degree of focus on a specific problem or context may provide a spectacular solution for a particular application, but unless the solution generalizes, it is of limited value (i.e., it is a trap). In other words, it is absolutely critical to emphasize the broader implication of a study, rather than to celebrate a unique example. Second, we need to foster links to all (scientific and nonscientific) communities. If we cannot (or do not) talk to each other, then we need to talk to someone else. Ideas are important only when they are connected to other ideas, and the more connections the better. Failing to make the effort to travel beyond the comfort

zone of our familiar conferences, journals, and communities only relegates us to obscurity. To be part of the conversation, we need to seek the dialogue, not wait and pray that it finds us. Finally, we need to be among the first to address new sets of questions and to venture into new territories. There are many important emerging issues that we are uniquely qualified to address, and we should not be shy to do so. The beauty of having a multifaceted community like ours is that we can take more risks and branch even further into exciting areas of inquiry. We offer examples of some of these potential opportunities at the end of this paper.

2. What Has Changed

In the decade before *M&SOM*, the field focused on quality and productivity. At that time, Japanese production techniques were teaching the world how to reliably and efficiently produce high-quality products (Porteus 1986, Womack et al. 1990, Clark et al. 1991, MacDuffie 1995). At the birth of *M&SOM*, the field was transitioning to its next dominant paradigm: supply chain management (Fisher 1997, Lee and Tang 1997, Lee et al. 1997).

The move away from quality and productivity did not occur because of its lack of importance, but rather because of our success in understanding the key principles. At the time, two forces were at play that fueled energy around supply chains: vertical disintegration progressed rapidly, with firms specializing at one level of the value chain; and internationalization of supply chains and markets expanded distances and complexity. Our field's strength in inventory, capacity, and production management naturally flowed into the study of supply chains. We created a big research pond within our own community, a pond sufficiently large to produce highly influential papers.

Supply chains had a long run, and there is still work to be done (as there is with quality and productivity), but the field again began a transition, probably starting around 2005. However, instead of everyone moving over to a new domain, we splintered into many different groups (maybe even tribes):

- ▷ Communities organized around sectors. The emphasis is in on issues central to one sector of the economy. The primary ones are healthcare, retail, and energy. The field had focused on automobiles (Jordan and Graves 1995, MacDuffie et al. 1996) and semiconductors (Chen et al. 1988), but far less so now.

- ▷ Group(s) with a societal focus. This community emphasizes environmental and humanitarian objectives in addition to profits.

- ▷ Behavioral focus. The default paradigm in operations is one of optimization, but human agents do not always correctly optimize based on all available data. This community studies how departures from neoclassical rational behavior influence operations.

- ▷ Revenue management and pricing. This community tackles the challenges of choosing and varying prices to maximize the value extracted by the firm.

- ▷ Marketplaces. Some firms establish platforms or markets that facilitate trade between suppliers and buyers. Examples include online malls (e.g., Amazon Marketplace, Flipkart, Taobao, etc.), transportation network companies (e.g., Lyft, Uber, Didi, etc.), and house-sharing platforms (e.g., Airbnb). This community studies issues related to market design and optimization.

- ▷ Finance. The resources we traditionally care about include labor, equipment, and inventory. But we are increasingly interested in the interplay of financial and physical assets.

Each cluster has its own set of new phenomena to study, providing a plethora of opportunities, along with its own conference and (special issues of) journals. But what are the commonalities across these clusters? For example, while there are possible connections between behavioral operations and marketplaces, we have not yet found many, and there may not be many to find. And methods used in revenue management can be hard to appreciate if a researcher works in empirical healthcare (and vice versa). Given the limited dialogue across the various clusters, we have fragmented into distinct and, almost by definition, smaller groups. It is possible to be the biggest fish in a small pond, but the biggest fish live in large ponds. The next section provides some evidence that we are indeed swimming in puddles.

3. The Rut

Just from casual observation we know that what we study and how we study it has changed over time. But has this led to greater impact? Table 1 lists the research papers published in *M&SOM* from 1999 to 2017 that have achieved at least 25 Google Scholar citations *per year* since publication date. (Excluded are review and survey papers because their focus is not primarily on new research.) The cutoff of 25 citations is arbitrary, but to us it feels like a reasonable threshold for an influential paper. It results in the selection of 24 papers, which is about 4% of all of the papers published by *M&SOM* during this period.

Five of the 24 papers in Table 1 have “Newsvendor” in the title. Several of the other papers include a newsvendor model even if “newsvendor” did not make it to the title. We love the newsvendor as much as anyone (maybe more so than most), but surely the world involves something more than a newsvendor! And even more concerning is the fact that only 4 of the 24 papers were published after 2010. That means that 20 of 24 of *M&SOM*'s greatest hits happened 10 or more years ago. Some might immediately argue that this is due to our metric. But citations per year

Table 1. M&SOM Research Papers from 1999 to 2017 with 25 or More Citations per Year

Year (Issue)	Title	Authors	Google Scholar Citations	Cites/yr
1999 (2)	Quantity Flexibility Contracts and Supply Chain Performance	A. A. Tsay, W. S. Lovejoy	564	28.6
2000 (4)	Channel Dynamics Under Price and Service Competition	Andy A. Tsay, Narendra Agrawal	671	36.8
2000 (4)	Impact of Uncertainty and Risk Aversion on Price and Order	Vipul Agrawal, Sridhar Seshadri	500	27.4
2001 (4)	Quantity in the Newsvendor Problem Selling to the Newsvendor: An Analysis of Price-Only Contracts	Martin A. Lariviere, Evan L. Porteus	1137	65.9
2002 (3)	Coordination and Flexibility in Supply Contracts with Options	Dawn Barnes-Schuster, Yehuda Bassok, Ravi Anupindi	834	50.5
2002 (3)	Designing a Call Center with Impatient Customers	O. Garnett, A. Mandelbaum, M. Reiman	609	36.9
2003 (3)	On the Interface Between Operations and Human Resources Management	John Boudreau, Wallace Hopp, John O. McClain, L. Joseph Thomas	394	25.4
2003 (4)	Matching Demand and Supply to Maximize Profits from Remanufacturing	V. Daniel R. Guide, Jr., Ruud H. Teunter, Luk N. Van Wassenhove	588	38.6
2004 (2)	To Pull or Not to Pull: What is the Question?	Wallace J. Hopp, Mark L. Spearman	543	36.8
2006 (1)	Extending the Horizons: Environmental Excellence as Key to Improving Operations	Charles J. Corbett, Robert D. Klassen	511	39.3
2007 (1)	A Newsvendor's Procurement Problem when Suppliers Are Unreliable	Maqbool Dada, Nicholas C. Petruzzi, Leroy B.Schwarz	435	36.3
2007 (2)	Competition und Diversification Effects in Supply Chains with Supplier Default Risk	Volodymyr Babich, Apostolos N. Burnetas, Peter H. Ritchken	332	28.3
2007 (4)	In Search of the Bullwhip Effect	Gérard P. Cachon, Taylor Randall, Glen M. Schmidt	299	26.6
2008 (3)	Optimal Pricing of Seasonal Products in the Presence of forward-Looking Consumers	Yossi Aviv, Amit Pazgal	611	58.2
2008 (3)	Learning by Doing in the Newsvendor Problem: A laboratory Investigation of the Role of Experience and feedback	Gary E. Bolton, Elena Katok	322	30.7
2008 (4)	Bounded Rationality in Newsvendor Models	Xuanming Su	344	33.6
2008 (4)	Dual Sales Channel Management with Service Competition	Kay-Yut Chen, Mural Kaye, Özalp Özer	272	26.5
2009 (4)	Consumer Returns Policies and Supply Chain Performance	Xuanming Su	251	27.1
2010 (2)	Dynamic Scheduling of Outpatient Appointments Under Patient No-Shows and Cancellations	Nan Liu, Serhan Ziya, Vidyadhar G. Kulkarni	261	29.8
2010 (3)	Mitigating Supply Risk: Dual Sourcing or Process Improvement?	Yimin Wang, Wendell Gilland, Brian Tomlin	258	30.4
2016 (1)	Analytics for an Online Retailer: Demand Forecasting and Price Optimization	Kris Johnson Ferreira, Bin Hong Alex Lee, David Simchi-Levi	118	39.3
2016 (2)	Supplier Evasion of a Buyer's Audit: Implications for Motivating Supplier Social and Environmental Responsibility	Erica L. Plambeck, Terry A. Taylor	73	26.5
2017 (2)	Service Region Design for Urban Electric Vehicle Sharing Systems	Long He, Ho-Yin Mak, Ying Rong, Zuo-Jun Max Shen	50	28.6
2017 (3)	The Role of Surge Pricing on a Service Platform with Self-Scheduling Capacity	Gérard P. Cachon, Kaitlin M. Daniels, Ruben Lobel	125	83.3

Notes. Google Scholar citations recorded January 15, 2019. Data provided by Song et al. (2019).

actually helps younger papers—it is easier to sprint at a fast pace than to maintain a brisk speed for a long distance. (For this reason we excluded 2018 papers. Had 2018 been included, there would be 6 from that year.) An alternative, and concerning, interpretation is that the field has not been particularly good at publishing interesting papers that are also important. Put another way, while we are more diverse, it is not clear that we are more influential. Why?

4. Interesting, Important, and Impact

The impact of a piece of scholarship is the combination of “interesting” and “important” (Cachon 2012):

$$\text{Interesting} \times \text{Important} = \text{Impact.}$$

An “interesting” paper is one that provides an unexpected answer. Unexpected should be judged relative to what is assumed to be true given the current literature. It is irrelevant whether it follows immediately

from existing results or is trivial to derive. How arduous the journey is does not matter—we care only about the destination. In fact, it is a fortunate (and talented) scholar who documents an interesting result with minimal effort. What does matter for a result is when it occurs. What was interesting at one point in time is far less interesting later on. This essentially follows from the definition of unexpected: once a new piece of knowledge is established, repeating it, or claiming something similar to it, is no longer as interesting. And just like mines eventually run out of ore, a topic can start to run out of interesting results. In that case we find ourselves in a situation where “everything has been said but not everyone has said it.”¹ When that happens, we need to look for different interesting findings. The only way to stay interesting is to change.

The second component of impact is “important.” An important paper is one that finds a result that is of significance, one of broad generality, applicability, or usefulness. A paper can be interesting, but not important. For example, a finding like “X can happen in model M” might be surprising, but is important only if “X is likely to happen in model M.” If unrealistic parameters or assumptions are needed for a phenomenon to occur, then it is uninteresting no matter how unexpected it is. An unimportant result is often due to an answered question that is too narrowly defined, or of limited use. These papers generate intellectual curiosity but are not meaningful for advancing knowledge.

The best way to yield an important result is to study a question that is of interest to as broad a set of people as possible, ideally a set of people that do not even do research. When the context becomes too central for a result, it is not likely to have the breadth needed for importance. This is what we refer to as the “trap of specificity.” When a scholar focuses only on the quality of the answer, the quality of the question suffers. To explain, if you want a perfect answer, then ask a very narrow question. But doing so sacrifices importance, and therefore impact. There is often a trade-off: most interesting (broad) questions cannot be answered with a high degree of precision. For example, think of an answer to a question that is likely on the minds of any CEO: “What kind of leadership traits would help me improve operational performance of a company?” It is unlikely that there is a precise answer, but even an imprecise answer could have significant impact (Ramdas and Williams 2019).

We all agree that we want impact, but what do we want to impact and how should we measure it? In Table 1 we use citations to measure impact on academic research. One may argue that citations are an imperfect measure of the impact of research. This is of course true. But even an imperfect measure can be useful, especially if it is the best among imperfect measures—do not let perfect be the enemy of good.

Others may argue that our focus should not be on academic research, but rather impact on the well-being of society, using some real/actual measure of influence, such as revenue improvement, or cost reduction, or total time saved, etc. We surely welcome a result with immediate and real impact even if (for some very odd reason) it is not well cited in the literature. Nevertheless, we tend to think that academics working at research institutions are primarily in the business of creating novel (interesting) and valuable (important) knowledge as well as communicating it to students (teaching), and not in the business of directly applying knowledge. Nevertheless, there are ways to measure impact of applying knowledge on practice (e.g., media mentions, circulation of industry publications, sales of practice-oriented books, INFORMS Edelman Prize, various industry awards) that should, no doubt, be taken into account when we holistically evaluate our overall societal impact. Important recent initiatives in this direction include the Responsible Research in Business and Management,² the inaugural M&SOM Society Award for Responsible Research in Operations Management, and the recently announced Special Issue of M&SOM on Responsible Research in Operations Management.

In sum, to have impact, a paper should be both interesting and important. Fragmentation of a field risks scholars working in small communities on questions of limited importance. Findings with limited importance, no matter how interesting they are, do not lead to impact. Similarly, academic impact is minimal from findings with importance (e.g., a successful consulting project) that merely apply existing knowledge.

5. How Should We Change?

Our claim is that the field is in a rut because we have fractured into too many small pieces. In doing so we have fallen too often into the trap of specificity. The solution is not for us all to coalesce around a single topic again. We cannot, nor should we want to, put that genie back in the bottle. Instead, we should continue to maintain a broad scope. Indeed, we should keep pushing our boundaries. However, we cannot simultaneously be broad and isolated.

Despite the lack of bridges across our research clusters, each one has an obvious link to a large and vibrant community that does not think of itself as “operations.” For example, behavioral operations links to psychology, operations finance links to traditional finance, and marketplaces links to economics. Ignoring those links throws away an opportunity for us to have a much larger voice. Ideally, we should invite scholars from other communities into our house and journals (as is done at a small number of boutique conferences, like the Marketplaces conference, the Wharton Empirical Conference, or the Utah Operations

Conference). But if we are unwilling to do that, then at the very least we need to reach out to them. To do so requires us to link our research to theirs through citations and meaningful critiques and commentary. Better yet, we should try to publish our work in general-interest outlets like *Science* (Argote and Epple 1990, Wible et al. 2014), *Nature* (Xu et al. 2017), the *Proceedings of the National Academy of Sciences* (Yang et al. 2013, Thorlakson et al. 2018), and the *New England Journal of Medicine* (Ramdas and Darzi 2017)

While talking to other people is valuable (even crucial), it is most effective if we have something interesting and important to say. So what will we have to offer? To us, the best source of material is not at the boundary of our field, it is beyond the boundary—we need to keep pushing broader.

6. What Could Be Next?

We are incredibly optimistic about the prospects of operations management scholarship. Operations management is about the selection, procurement, and control of resources to create value. This applies to any organization, no matter its objective. In short, our field has the opportunity to touch on, and is relevant for, essentially all human activity. But more specifically, where should we look for new questions to ask and better answers to old questions? We see at least five promising paths to expand our boundaries and increase our impact.

Innovative Business Models

A large number of new businesses over the last 25 years have earned their success through some innovation in their business model related to operations management. In each of these cases they have changed the value proposition they offer, which resources they assemble, how they manage those resources, where they locate resources, and/or their acceptance (or avoidance) of risks (Girotra and Netessine 2014, Cachon 2019). For example, Amazon became a giant e-retailer by moving inventory away from customers [and in many cases not owning it (Netessine and Rudi 2006)], and Zara (i.e., Inditex) became the world's largest fashion apparel retailer by controlling more of the value chain that was moved closer to their home market (Caro and Gallien 2012, Hansen 2012, Acimovic and Graves 2015). Li and Fung (Fung 1998, Belavina and Girotra 2012) grew to become the world's pre-eminent supply chain orchestrator by helping buyers manage the risks of global-sourcing. Earlier examples included Dell's assemble-to-order dominance of the personal computer industry and Nike's use of overseas outsourcing to ascend to the top of the sports equipment and apparel industry. Recent innovations in business models have been enabled by ever-advancing digital technologies that

reduce the cost and accelerate the speed of collecting, storing, sharing, and analyzing data; connecting supply with demand via automated processes; etc. Some of these innovative business models involve increasing the utilization of underutilized assets often via a better matching of capacity or service providers with demand, and so forth—for example, Uber and Lyft in ride-sharing (Cachon et al. 2017, Besbes et al. 2018, Bimpikis et al. 2018, Taylor 2018); Airbnb and HomeAway in lodging (Bai et al. 2018, Li and Netessine 2018); Postmates in food delivery; Lime, Ofo, and other public systems in bike-sharing (Freund et al. 2016, Kabra et al. 2018, Zheng et al. 2018); Kickstarter, Indiegogo, and ICOs as new ways to finance product development (Alaei et al. 2016, Babich et al. 2017, Chakraborty and Swinney 2017, Belavina et al. 2018, Gan et al. 2019); and Upwork for knowledge services (Moreno and Terwiesch 2014, Belavina et al. 2019). All of this sharing has implications for the manufacturing of durable goods (Abhishek et al. 2019). Other innovations involve new channels of distribution, such as omni-channel retailing (Gallino and Moreno 2016, Gallino et al. 2017, Bell et al. 2018). In all of these examples, the key to these successful innovations is not a singular new product (as Microsoft did with Windows or Pfizer did with Lipitor), but rather a new process for delivering a relatively mundane product. In other words, they use novel operations to create new markets and to dominate established ones. We should not ignore this even if a business model does not fit nicely into a single optimization problem—business models might be more strategic or “high-level” than we are traditionally comfortable with, but given the impact and interest surrounding new business models, and the key role of operations management phenomena in designing and operating such new business models, we would be foolish not to venture there.

Old Problems with New Scale

Some of our traditional problems include revenue management (Talluri and Van Ryzin 2006), inventory control (Zipkin 2000), and assortment planning (Gaur and Honhon 2006). These all offer computationally intensive, challenging optimization problems, and we have made considerable progress on all of them. But most of the progress we have made assumes and works with “little data.” Can we recommend new solutions that exploit now-available “big data” to achieve noticeable improvements in performance? For example, instead of generic product assortments, can firms select personalized or localized assortments (Dzyabura and Jagabathula 2017, Aouad et al. 2018, Farias et al. 2018)? Or, can microlevel data on performance across tens of thousands of workers be used to tweak wages to reduce costly turnover (Moon et al. 2018)? Further,

these problems are now set in “internet-scale” contexts, such as assortment planning for online retailers (Golrezaei et al. 2014), assignment of AdWords to bidders (Balseiro et al. 2015), real-time content recommendations (Besbes et al. 2016), and optimal medication dosage (Bastani and Bayati 2018), among others. The scale of these problems is such that new advances in computational methods are needed to keep up. Further, the use of cloud-based computing resources has sparked interest in algorithms that facilitate parallelized solution approaches. Finally, in some contexts we must now provide “online” or real-time solutions to problems, which requires a new set of computational approaches.

Move Beyond Profit Maximization

We are most comfortable in the paradigm of optimization. But what should be optimized? Traditionally, we focused on costs and profits. But the objective functions of the future are more nuanced. Ethics, equity and well-being are of importance to many. For instance, we need to contribute to the discussion on the environmental impact of manufacturing, supply chain, retail network, and business model choices (Cachon 2014, Avci et al. 2015, Guo et al. 2016, Belavina et al. 2017, Guajardo 2018, Thorlakson et al. 2018, Astashkina et al. 2019). Business models centered on empowering women can alleviate poverty (Plambeck and Ramdas 2019). Humanitarian logistics and any other operations problems sponsored by donors of nonprofit organizations usually strive to reach the largest possible number of people (Jónasson et al. 2017, Berenguer and Shen 2019, Zhang et al. 2019a). Cities and local governments want to design public transport systems to achieve the highest ridership (Arora et al. 2019). Government implemented dynamic pricing has the opportunity to improve equity of public resources (Feldman et al. 2016). We increasingly pay attention to startups, which often emphasize growth (or survival) over profits (Swinney et al. 2011). Companies operating at the “bottom of the pyramid” usually have the goal of increasing inclusion and achieving equality (de Zegher et al. 2019, Kalkanci et al. 2019, Uppari et al. 2019). Online services often need to be designed to reduce opportunity for discrimination (Cui et al. 2016), and there is growing concern for the welfare of gig economy workers (Daniels and Grinstein-Weiss 2018, Benjaafar et al. 2019, Chen et al. 2019). We increasingly recognize that humans do not always follow rational decision-making models (Baucells et al. 2016, Buell et al. 2017, Shunko et al. 2017). In healthcare and other services focused on well-being, the relevant objectives include decreasing mortality or length of stay (in a hospital) or some quality-of-life measure (Song et al. 2015, Arora et al. 2017, Chen and Savva

2018, Dai and Tayur 2019). In many of these cases the novelty of the solutions follows from the novelty of the objective function.

A Bigger, More Prosperous World

A big story of the past 25 years has been the growth of developing economies, in particular China. China is likely to continue to grow and pull the center of economic gravity further to Asia. Other once less-developed countries want to, and probably will, share in the growth. Meanwhile, the first-tier economies are not showing signs of being able to keep their positions. Will this lead to a “great convergence” in which the gaps between rich and poor shrink (Sachs and Warner 1995)? Or at some point do we enter a cycle of the rich getting richer? The answers to these questions are likely to have significant implications for how companies structure their operations. In a convergent world, companies may become more local and supply chains may shrink because regions can become relatively self-sufficient. A divergent world risks greater inequity and, with that, heightened sociopolitical risks. How do we incorporate such risks into our operational decisions (Tomlin 2006, Bimpikis et al. 2019)? In either scenario, a continued focus on the currently developed economies would be misplaced, and some work has started to analyze data unique to developing economies (Parker et al. 2016, Bray 2018, Moon et al. 2018, Xu et al. 2018, Zhang et al. 2019b).

The Nature of Work

At this moment the nature of work is front and center in our society (Brynjolfsson and McAfee 2014, Autor 2019). How has work changed? And how are those changes related to income inequality and overall well-being? Many people would value some answers, and if there is a topic that we have some expertise in, it is work. For example, how will the balance change across different contractual forms of labor, such as seasonal workers, temporary workers, part-time workers, and full-time employees (Katz and Krueger 2016)? If there is a trend toward lower employment in manufacturing continues, then are productivity gains possible in services? Self-driving vehicles will change how goods are distributed and where they are stored. But in what way? Robotics can change the need and nature for labor in all settings, from the factory floor to a retail store and even a hospital. If we replace workers in fulfillment centers with robots, then is that loss exceeded by the gain in workers/engineers to design and manage the robots? Which types of work are most likely to be replaced? And is there evidence that matches theory (Autor 2015, Allon et al. 2018)? There is simply no shortage of high-priority, important questions in this domain.

In sum, the world of 25 years from now probably will look as different to today as today looks to 25 years ago. This is terrific for our field. While our current fragmentation is an issue, it need not remain. We have a bright future as long as we do not become too enamored with very nice answers to narrowly defined questions (the trap of specificity), we reach out and embrace all scholars that could have an interest in what we have to say (which, fortunately, is a very large group), and we proceed to push our boundaries even further. We have plenty of material to work with. As long as we creatively identify questions related to how resources are managed and how value is generated in organizations, we will have the impact we deserve.

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Endnotes

¹ See https://en.wikiquote.org/wiki/Morris_Udall.

² See <https://rrbm.network/>.

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