

50th Anniversary Article

Five Decades of Operations Management and the
Prospects Ahead

Sunil Chopra

Kellogg School of Management, Northwestern University, Evanston, Illinois 60208, s-chopra@kellogg.northwestern.edu

William Lovejoy

School of Business, University of Michigan, Ann Arbor, Michigan 48109, wlovejoy@bus.umich.edu

Candace Yano

IEOR Department and the Haas School of Business, University of California, Berkeley, California 94720,
yano@ieor.berkeley.edu

Operations and Supply Chains is the current title for a department that has evolved through several different titles in recent years, reflecting its evolving mission from a focus on classical operations research at the time of ORSA's founding 50 years ago toward an embrace of a broader body of theory. Throughout this evolution, the focus on applied problems and the goal of improving practice through the development of suitable theory has remained constant.

The Operations and Supply Chains Department promotes the theory underlying the practice of operations management, which encompasses the design and management of the transformation processes in manufacturing and service organizations that create value for society. Operations is the function that is uniquely associated with the design and management of these processes. The problem domains of concern to the department have been, and remain, the marshalling of inputs, the transformation itself, and the distribution of outputs in pursuit of this value-creating end. Over the past 50 years the department has had a variety of titles, reflecting an evolving understanding of the boundaries of the operations function.

In this article we celebrate past accomplishments, identify current challenges, and anticipate a future that is as exciting and opportunity-rich as any our field has seen.

Key words: history; operations; supply chain management; future research

1. Celebration of History and Accomplishments

It is difficult to pinpoint the origins of our field. The search for rigorous laws governing the behaviors of physical systems and organizations has throughout history featured bursts of activity and periods of quiet. The classes of problems that we are most familiar with today came into high relief after the Industrial Revolution, when managers of large, vertically-integrated businesses faced coordination problems of unprecedented scope. Treatises on organizing, measuring, and managing production in these challenging settings were published by a range of professionals from business and industry. The rise of "scientific management" is usually associated with the work of Frederick Taylor, Frank and Lillian Gilbreth, and others in the late 19th and early 20th centuries. The Ford Harris EOQ model dates at least as far back as 1915.

During World War II these efforts continued, and were amplified, in the form of operations research

groups largely initiated and funded by government and quasi-governmental organizations. These mission-focused mathematicians modeled classes of problems and developed the foundational theories to address them, which created the Big Bang in our discipline. The applied problems motivating the work were concerned with the efficient allocation and control of resources; these were analyzed via mathematical models. Although some papers written in this era focused on descriptive models of system behavior, the dominant paradigm was optimization of system performance in the presence of constraints.

Management Science published its first volume in 1954 and helped to promote and catalog the explosive expansion of optimization theory fueled by interest in these applied problems. Indeed, the first issue of *Management Science* was dominated by topics that are clearly related to important issues in operations management. In the 1950s and 1960s the pages of *Management Science* displayed seminal articles by scholars

now recognized as giants in the field. These included contributions by G. Dantzig on the development and uses of linear programming; by L. R. Ford and D. R. Fulkerson on network flow problems; by A. J. Clark, S. Karlin, H. Scarf, H. M. Wagner, T. M. Whitin, A. Veinott, and D. Iglehart on inventory theory; by R. Bellman, A. Manne, C. Derman, A. Veinott, and E. Denardo on dynamic programming; by C. Derman and S. Ross on machine maintenance; by J. Jackson on queueing networks; and by J. C. Harsanyi on game theory. Many of the methodological developments listed above were motivated by operations management problems and were described in those contexts. For example, Dantzig applied linear programming to machine-job scheduling and aircraft routing. Bellman applied dynamic programming to a warehousing problem while Manne analyzed capacity expansion problems formulated as dynamic programs.

Most of the early research focused on developing algorithms and methodologies to solve optimization problems that arose in a broad range of functional areas. With a few notable exceptions such as the Dantzig-Wolfe decomposition and Harsanyi's work, much of this work involved mathematical analysis and algorithms within the context of a single decision maker. Most of the optimization problems also involved a single objective though there were early exceptions featuring multicriteria problems. The striking feature of this early research is the broad range of areas—including operations, finance, organizational design, economics, and marketing—from which problems originated. The first volume of *Management Science*, for example, included papers on executive compensation, linear programming under uncertainty, the impact of communication nets on task-oriented groups, and an axiomatization of utility. The common theme, however, was the use of a mathematical model to identify how the status quo could be improved.

Much of the initial work within the domain of operations management focused on tactical issues such as line balancing, scheduling, production planning, inventory control, and lot sizing. In some ways these tactical problems were ideally suited for the methodologies that had been developed up to that point. For these problems, the constraints and objective were usually well defined and involved a single objective with centralized control. These early successes resulted in the birth of operations research groups at many corporations, tasked with finding ways of improving performance. Within the academic community, most of the research in these areas initially took place in engineering departments. Gradually, during the 1960s, researchers in business schools began to study more scientific and rigorous approaches for decision making, instigated in part by recommendations emanating from studies by various

private foundations to make business education more rigorous, and efforts by universities to prepare faculty for this task.

2. The Challenges of the 1970s and 1980s and the Response

The period from the late 1960s through the 1970s saw a number of changes in the landscape of scientific computing, technology transfer of operations research tools, business education, and business practice that precipitated important changes in the field of operations management.

Operations research faced two types of challenges during this era. First, whereas the 1950s and 1960s provided a glimpse of the promise of management science to industry, the next two decades saw less success in delivering on this promise to industry. The speed and cost of computing continued to improve dramatically, but data storage and computation remained as practical hurdles to the implementation of many algorithms. Also, in some cases, the models did not keep pace with the evolution of business challenges and practice, and firms began to question the value of these models and methodologies. Second, academic researchers in functional areas such as accounting, finance, and marketing, had increasingly internalized the optimization theory and technology developed by operations researchers in the previous two decades and were using it as part of their research. This period saw many operations researchers move into other functional areas because those were the sources of their problems. As a result, the application of operations research ideas to marketing, for instance, began to be viewed more as marketing. By the 1980s, most corporate groups focused on operations research had shrunk or disappeared. At the same time, the academic research in operations research cum operations management became somewhat less focused on problems arising in a broad range of functional areas and more on problems that were internal to the theory developed in the field.

Simultaneously, industry was seeing the introduction of material requirements planning (MRP) systems, then later concepts such as just-in-time (JIT), the Toyota production system (TPS), and total quality management (TQM), which were having a significant impact on business practice and performance but were not strongly tied to the then-current academic research. Indeed, the ascendancy of the Toyota production system in business practice suggested that the locus of creativity had shifted away from academia.

During this period, researchers began examining operations management issues using non-operations research perspectives, seeking to explain phenomena that could not be explained by the existing theory.

The Toyota production system provided one focus for such research; although it contains features that are compatible with classical theory, it is also a holistic system of physical and human processes that extends its reach into the whole firm in a cross-disciplinary manner. Other researchers were beginning to examine higher-level issues in manufacturing strategy using an empirical approach. By the end of the 1980s, researchers and practitioners were using a broader set of methods and paradigms in their quest to improve operations.

The changes and challenges of the 1970s and 1980s generated a sense of identity crisis in our discipline. This was felt at some level by all of the researchers who lived through this era, but there is no consensus on the totality of its causes or characteristics. Some contributing factors included the natural maturation of the classical problem classes, and a need to reach for the next higher level of complexity. There was also an evolution within business from centralized to more decentralized organizational forms. The theory base for the discipline was expanding and diversifying dramatically. Whatever the causes of the identity crisis, the challenge to our field at this stage was to return to our original mission of using theory to inform current practice.

The first literature to develop in response to this challenge focused on trying to explain JIT and other industry practices in the context of theory that had been developed earlier. This research was valuable because it brought the attention of the field back to issues that were of concern to practicing managers. This refocusing of research questions has been a crucial driver of growth of the field in the 1990s.

Two important developments occurred as a result of this refocusing. The first was a move back toward interdisciplinary research. The second was an explicit recognition of decentralized loci of control and local incentives, and hence the re-emergence of economic equilibrium in addition to sole-owner optimality as criteria of central interest to our community. Both of these influences can be seen in the recent literature on supply contracts. The first papers in this area were motivated by contract forms actually in use by companies for sharing forecast risk, and examined the optimal response of a single party to a particular contract form. Thus, researchers focused on how capacity and replenishment decisions need to be modified for different contract contexts. It was only later that operations management researchers asked questions about appropriate (or optimal) contract forms. The initial papers featured relatively modest refinements to existing economic intuition by adding resolution to some general economic models (e.g., replacing general revenue or cost functions with more operational

detail) and, by so doing, refining the claims that can be derived.

We will speculate as to the future of the supply chain subliterature later in this article, but here it is worthwhile to trace some broad outlines of its development. First, business practice called the existing research paradigm into question. Second, addressing the new problems in some cases required the importation of technology developed elsewhere (e.g., economics). Third, the research focus became more managerial (e.g., focusing on system design, information, and incentives) and less on tactical execution. For example, very simple inventory policies, such as base-stock policies, have often been used as elements of higher-level system-design models. The development of this subfield has been very beneficial to our discipline, at least if one counts research papers, company sponsorships, and popular university courses. In response, the editor-in-chief of *Management Science* created the Supply Chain Management Department in 1997 to promote the cause, rather than assuming that the existing operations department would naturally embrace this new research agenda.

Each of these observations helped inform our department's response to the challenges of the time. Some broad themes are clear. First, the field needs to continually check its research against evolving industrial reality. This is sympathetic with our classical mission: Our academic forefathers in war-time OR teams were very focused on reality out of mission-critical necessity. The elegant mathematics that energized our field responded to real problems and can do so again. Second, and as a consequence of the first, our research will likely become more explicitly interdisciplinary, as it was in our early years, because actual business practice is not cleanly divided into functional problems. As we do this, we need to maintain our focus on the core agenda that defines our field: the design and management of the transformation processes that create value for society.

The department needs to embrace new, exciting research directions while protecting the brand equity of the journal. If we are sufficiently proactive, new departments will not be needed to raise the visibility of new and exciting subliterations. This requires a delicate balance at times. Some of the classical research themes are relatively mature, with very clear barometers of research excellence well known to a large community of scholars. This is not so with some of the newer areas. We might anticipate a period—albeit short—of technology transfers from other disciplines that will naturally raise the question of how we judge the novelty of a paper. This is already happening.

We anticipate that a focus on the issues central to operations management will soon carry us beyond existing technologies and provide the catalyst for

developing new ones. The set of challenging problems is without bounds, as is the upside potential for our field in this new era.

3. The Department's History and Current Editorial Mission

The history of the Operations and Supply Chain Department reflects a constancy of core mission and an evolution in its interpretation. The department has consistently focused on the operations function. The department title, editorial objectives, and implementation policies, however, have evolved with our understanding of what that mission entails.

The first volume of the journal in 1954 featured no separate departments, but rather six editors from a range of disciplines, drawn both from academia and industry, with C. West Churchman as managing editor. The stated mission of the journal was to identify, extend, and unify scientific knowledge that contributes to the understanding and practice of management. By 1959, the number of editors had grown to 11 (5 from industry), and to 40 (12 from industry) by 1968 when Robert Thrall was editor-in-chief. Martin Starr took over as editor-in-chief in 1969, and introduced the departmental structure. This featured separate departments for production management and logistics. Professor Starr published an interesting editorial letter in the 20th anniversary issue in 1974. He emphasizes a consistency of purpose since Volume 1, yet acknowledges criticisms of the field based on an inability to solve very complex problems, lack of implementation capabilities, and an overemphasis on optimization.

These issues remain with us today. The healthy tension, preordained in the practical world of management, between the purity of abstraction and the relevance of detail is not new, nor can we expect it to go away any time soon. It is part of the territory inherent in striving for a theory of management, and an integral driver of our cyclical attractions to theory, then practice, then theory again as we continually adjust to a changing world. This healthy tension is the correcting force that prevents our discipline from becoming too academically self-referential, or too focused on specific rather than universal insights. It is, in short, what makes this business so much fun.

Over the decades since its formation the department has regularly changed titles and editors as it searched for the boundaries of the operations function. Clearly, the transformation process can include input and output logistics, although intermittently one or more of these had separate departments. Does our mandate include design? What is the boundary between design and planning, the latter activity being central to all management? Where does operations

end and finance begin, given that (at least in manufacturing firms) most capital investments are operations related and working capital has a large inventory component? Where does operations end and human resources begin, given that no good manager would ignore the social dimension of the operating system?

The challenge of defining workable boundaries between departments is an inevitable constant. Management is a holistic exercise, and attempting to draw definitive boundaries between its various aspects is a fool's mission. The definition of departmental boundaries turns on the dual attractions of refining existing knowledge via well-established subliterations and encouraging new integrative ways of thinking about management and, hence, new subliterations. It will always be so.

We offer some example punctuation points in this evolution. In 1974 there were separate departments for production management; Logistics; and Dynamic Programming and Inventory Theory. In 1981 these three departments became two: Production and Operations Management; and Logistics, Distribution, and Inventory. By 1985 these two departments seemed to move closer in their missions, being titled Production and Operations Management; and Manufacturing, Distribution, and Inventory. Then, in 1987, all of the above were subsumed into a single department: Manufacturing, Distribution, and Service Operations. (For details on the evolution of the departmental structure of *Management Science*, see "Fifty Years of *Management Science*" in this issue.) The editorial policy of this large department stated that

of particular interest are papers that deal with strategic concerns such as the choice and impact of new production or information technology, and papers that may provide insight or simple models for guiding manufacturing or service policy. The department encourages papers that examine the planning and coordination of activities and resources within a manufacturing, distribution or service operation.

With this, the department anticipated the current editorial philosophy of focusing on senior management issues, which can be seen as a natural extension of this earlier sentiment.

In 1997 the separate Supply Chain Management Department was added to provide a home for what was already a substantial and rapidly growing literature in this area. In 2002, the Manufacturing, Distribution, and Service Operations Department was renamed Design and Operations Management. There were two reasons for this. First, as our understanding of operations matured, we no longer required detailed articulation of its parts (manufacturing, distribution, services). Second, the substantial overlap among many design and operational issues argued against trying to define a boundary between the

two. Finally, in 2003, another redistricting activity resulted in Supply Chains joining Operations to form the current Operations and Supply Chains Department, and some aspects of design included in another renamed department: Technological Development, Product Development, and Entrepreneurship.

Throughout this history, regardless of its name, the department's core mission has been to identify, extend, and unify scientific knowledge that contributes to the understanding and practice of operations management, defined as the design and management of the transformation processes that create value for society. The current editorial policy continues a trend discernable as far back as 1987, when the (then) new macrodepartment for operational and logistical issues adopted a mission focusing on higher-level system design issues, and encouraged the use of parsimonious models analyzed for insights. The current editorial posture reinforces that policy.

The current philosophy differs from the past only in the stringency with which we enforce these stated aims. We specifically encourage articles addressing decisions typically made by senior managers, and retarget to other journals articles that focus primarily on methodological contributions or issues of tactical execution. This policy is not intended to make a statement about the relative value of alternative research missions, as some tactical issues are of interest to upper management. Rather, we recognize the availability of other high-quality outlets under the INFORMS umbrella for outstanding research on classical problems, and wish to encourage new research directions for which the supporting academic infrastructure may not be as complete. When revising the editorial mission and considering how to implement it, we sought to consider *Management Science* not in isolation but rather as part of the portfolio of high-quality, operations-related INFORMS journals.

One simple test of consistency with our current mission is to ask whether an upper-level manager, rather than a scheduler or technician, would be interested in the results presented in the paper. Although we do not expect managers to read *Management Science* papers (our language is too compact and arcane), the research ideas contained in an article should, perhaps with some translation into management vernacular, be of high interest to a senior manager. These will be predominantly issues of investment, system design, and operations strategy rather than of tactical execution. Another intuitive filter is whether one can take the ideas in an article and prepare a one-page summary of key take-aways that would be of interest to senior managers. In fact, these deliverables should be apparent early in the article, reinforced by the presentation. High levels of rigor are, as always, needed

to mount a convincing argument to defend the conclusions, but it is crucial to articulate the significance, applicability, and limitations of the results.

The recent changes in how we implement the editorial policy have not been without controversy. In the early days of our discipline we were energized by asking questions that needed answers in practice, and bringing clear logic (primarily mathematically represented) to bear on those problems. Many of the problem classes forged 50 years ago are still with us and remain important. However, as described above, the natural maturation of those problem classes and the evolution of industrial thinking and practice suggest that we can stand on the firm foundation of the past and reach up to the next level of organizational complexity.

We believe that there are opportunities to encourage important new work that does not yet have its own momentum and needs a high-octane kick-start, like *Management Science*, to help get it off the ground. Lacking that, natural institutional inertia encourages the maintenance of the status quo.

By pursuing this path, it is our intent to proactively encourage the research community to extend its reach without devaluing the traditional strengths that made the discipline what it is. There are dangers. As noted, the standards of excellence are not mature in novel areas of research. The challenge before us, editors and referees alike, is to protect the very high brand equity of the journal, while using that same brand equity to encourage work in new areas.

If we do our job well, this period will be recognized as one of great forward movement and the origination of important subliterations that help define the future of our discipline. Recognizing the clear successes of the past, we embark upon this path with humility and with recognition of, and respect for, contrary views.

4. The Way Forward

We have already mentioned several anticipated consequences as we embark on this new journey. Our research will by necessity become more cross-functional in scope, which will require facility with the tools and concepts that have been developed in other research disciplines, and we hope and expect that we will pass through a stage of technology transfers to a new period of novel synthesis.

Although ultimately it is the problems facing real managers that will define our objectives and techniques, we can already see the broad outlines of potentially new and exciting subliterations. We provide this list not to limit the scope of innovation, but to provide a necessarily incomplete set of examples to demonstrate the challenges and potential in our discipline.

1. *Supply Chains*: Supply chain management, like operations itself, has ill-defined boundaries. In its broadest sense, it has come to be defined as the management of all aspects of providing goods to a consumer, from extraction of raw materials to end-of-life disposal and recycling, including manufacturing, physical logistics, and after-sale service and warranty issues. With such a broad scope, combined with the rapid rate of evolution of supply chain structures in both physical and organizational dimensions, the evolution of legal structures that constrain the terms of trade and pollution, and trade structures that raise challenging issues of globalization, vast opportunities remain to address unanswered and as-yet-unposed questions, many of which involve broader decision scope, more decision makers, inclusion of risk and greater recognition of business realities that have traditionally been ignored. We anticipate that waves of interest in specific issues will come and go, just as they have in the past. The most valuable contributions, however, will involve addressing real problems in real supply chains, and developing the theory to support managerial decision making in those contexts.

2. *OM-Marketing Interface*: Marketing is the key information gatekeeper between operations and the product markets. Marketing is charged with determining what customers value (including cost, quality, and delivery characteristics) prior to product development; product positioning, pricing, and forecasting both before and after product launch; and promotions after product launch. Interdisciplinary research involving operations and marketing decisions goes back many decades, but there is ample opportunity to develop models that are more comprehensive and have greater fidelity than the current state of the art. Many of the key questions at this interface involve behavioral aspects, providing opportunities to incorporate results from the growing bodies of empirical research on related topics.

3. *OM-Finance Interface*: Capital equipment and inventories constitute a sizable portion of the assets of most manufacturing companies. Companies have long recognized the role and impact of these assets in their financial decision making, but it is only relatively recently that operations management researchers have begun to relate financial models and financial instruments to the procurement and management of these assets. Also, as secondary markets for a range of commodities and other products mature, there is increased potential for applying financial insights developed in the context of complete markets to more traditional operational issues.

4. *OM-Organizations Interface*: No plant manager anywhere would ignore the role of good people management in running an efficient operation. Yet, the

research in our discipline has remained largely disjoint from the social sciences literature on human resource management and organizational behavior (OB). Our heritage has emphasized constructing normative mathematical models, and the OB literature is dominated by positive empirical findings. Operations management models have historically invoked oversimplified models of motivation, learning, creativity, and other such aspects of human behavior that are vital to the success of management policies in practice. Models that can maintain high levels of rigor while incorporating these elements will be richer and more realistic. In this area and others, high-quality descriptive and empirical work, including experimental analysis of behavior and decision making, often precedes prescriptive models. We see this integration as a critical need, but recognize that its evolution will be slow. One initiative we have taken is to add to our editorial staff the ability to apply social science standards to empirical research.

5. *Service Operations*: Service organizations are a large and growing part of the world economy. Operations management academics have struggled with a clear definition of what services are—and what research challenges they pose—relative to more traditional manufacturing contexts. Services are difficult to inventory so that variability must be buffered by capacity or time. Also, in many cases, a service transaction features simultaneous production and consumption with the customer an integral part of this activity. This may amplify the human perceptual component of service quality relative to the consumption of manufactured goods. The search for the distinctive attributes of service operations continues, but may be taken up in specific service contexts. Financial services and call centers already have their own subliterations. It is clear that health-care operations will be of increasing economic importance with the aging of the post-war baby boom. There remain many opportunities for research, not only on how to make existing service operations more effective and efficient, but on how to design, deploy, and operate systems offering new services, or old services via new technologies.

6. *Operations Strategy*: There is a large literature on firm strategies in different competitive environments. There is currently less literature on functional strategies and how they interact with each other. There is considerable scope for research on which mosaics of functional (including operations) strategies are self-consistent and aligned with firm strategies in different competitive environments.

7. *Process Design and Improvements*: Many quality programs have process improvement as their core theme, and the key applied tactic is managing the innovation process. Where do new ideas come from, how are they encouraged, nurtured, screened,

and implemented? Process design poses similar challenges, but with fewer constraints on the eventual outcome. Earlier we alluded to the critical relationship between operations and organizations. A good process design must merge the physical flow system, social system, and information system into a self-consistent whole.

As the research community moves into these new areas and others, we have full faith in the classical tools of our trade and the classical objective of applying those tools to help real people make real decisions. We also recognize that we will need to augment these tools to address new challenges as they arise. As these few examples have demonstrated, the chal-

lenges before us are great, and they will call for the same sort of creativity and dedication to task demonstrated by the scholars of the Big Bang. The upside potential for novel, seminal research has never been greater. We hope and expect that years from now an overview will be written acknowledging the significance and contributions of papers now being written.

Acknowledgments

The authors thank Richard Chase, Don Erlenkotter, Warren Hausman, Steve Nahmias, Roger Schroeder, Lee Schwarz, Ed Silver, and Joe Thomas for providing their perspectives on the evolution of the operations management discipline.