11
PROJECT DESIGN AND MANAGEMENT

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1 Introduction

A project is “a temporary endeavor undertaken to create a unique product, service, or result” (PMI 2013)—i.e., an attempt to do something new, once, and by a deadline. Projects comprise the opposite end of the spectrum of work from repetitive operations, where, at the extreme, the same thing is done over and over again. In contrast, projects are low-volume, high-variety operations (Maylor et al. 2015). However, projects come in many types and vary significantly in their degrees of novelty, complexity, innovation, dynamism, etc. Some projects seek to replicate previous projects, albeit under different circumstances. Other projects seek to create a recipe for a new product, process, or service, which will then be produced and delivered repetitively. Some projects seek to improve ongoing, repetitive operations. All projects contain some elements that are relatively well understood and perhaps even repetitive, but they all also have aspects that make them unique. Projects therefore tend to require a significant amount of creativity and innovation, as opposed to merely following established routines. (Projects thus have much in common with “white-collar” work (Hopp et al. 2009), although not all white-collar work is project work, nor vice versa.) Although projects and repetitive operations have some common characteristics and the dividing line between them is sometimes fuzzy, projects nevertheless require a distinctive perspective and analysis techniques. For example, while it is impossible to assemble two components on a production line when one is missing, it is often possible to do an activity in a project without one of its inputs (by using an assumption instead). In addition, it may be possible to build a model without having the exact values of all of its input variables or to do an analysis without knowing all of the factors completely, although doing so may increase the risk of later problems. Situations such as these give project managers additional degrees of freedom, albeit often at their peril, thus making for challenging decision problems (e.g., Loch and Terwiesch 2005). Projects permeate most modern organizations, and their effective and efficient execution can be a significant competitive lever.

Project management (PM) is “the application of knowledge, skills, tools, and techniques to project activities to meet the project requirements” (PMI 2013). PM includes determining a project’s scope of work; identifying and scheduling activities; acquiring and assigning budgets and other resources; monitoring progress; managing people; and balancing time, cost, and results as a project unfolds. Relative to managing repetitive operations, PM tends to involve greater
uncertainty, ambiguity, and dynamism. Although initial project plans are often erroneous, planning remains a fundamental basis for effective PM. (Even project control is essentially an act of replanning.) This chapter will suggest that projects can and should be designed—i.e., specified and customized based on deliberate choices regarding size, scope, cost, duration, resources, risk, deliverables, objectives, and managerial approach—and empowered to adapt to unforeseen circumstances and events.

Because PM includes a very wide array of transdisciplinary topics (Morris and Pinto 2004; Shenhar and Dvir 2007b; Kwak and Anbari 2009; Söderlund 2011b), this chapter is necessarily broad. Some universities offer an entire Master’s degree in PM, where the curriculum overlaps with operations management (OM), operations research (OR), management science (MS), and decision science (DS) curricula. While PM research appears in many OM, OR, MS, and DS journals, other journals such as the Project Management Journal and the International Journal of Project Management address PM topics exclusively. Moreover, a myriad of topics in the much wider field of general management have a bearing on PM. For example, a great many PM-relevant papers may be found in the Academy of Management journals. On its own, PM has spawned professional societies and bodies of knowledge, such as the Project Management Institute (PMI) and its Guide to the Project Management Body of Knowledge (PMBOK) (PMI 2013), which outlines ten basic areas of PM: integration, scope, time, cost, quality, human resources, communications, risk, procurement, and stakeholder management. Each of these areas individually comprises many rich streams of research. Therefore, it would be impossible to address the vast literature pertinent to PM—with all of its history, topics, theories, findings, best practices, tools, and techniques—as well as comment on its future—in depth within a single chapter.

Rather, this chapter has the much more modest aim of providing a brief report on some important research findings, past and present, as they intersect with the work of scholars in the Production and Operations Management (POM) community. As much as possible, it will point readers to review papers and/or books that summarize particular topics. This chapter will also suggest some directions for future PM research. Hence, it is organized into three main sections: looking back, looking around, and looking forward at PM research. The intent is to provide doctoral students and established scholars with a perspective on the PM research landscape while emphasizing aspects that might not be obvious from a perusal of contemporary compendiums.

2 Looking Back at PM Research

Since their earliest days, humans have created and managed projects for construction and destruction, but it was not until the late 1950s that a delineated, supporting literature for PM began to emerge (e.g., Gaddis 1959; Malcolm et al. 1959). Many of the formal techniques for PM were initially codified through the systems- and OR-oriented approaches to management undertaken by U.S. military projects around that time (e.g., Hughes 1998; Cleland 2004). Formal PM was born out of necessity to address practical challenges. As PM approaches become more popular, they took under their wing other techniques for managing work (e.g., Gantt 1919). Over the last sixty years, PM research has taken a wide variety of approaches, emphasizing analytical and empirical methods and the development of PM tools and techniques. With no claim to comprehensiveness, this section highlights some important streams of this research.

2.1 Activity Scheduling

One of the most widely explored topics in PM concerns the scheduling of activity networks (project processes). Early approaches include the program evaluation and review technique
Tyson R. Browning

(PERT) and the critical path method (CPM) (Malcolm et al. 1959), used to estimate project duration and its variation. Unlike in repetitive processes—where the constraints are called bottlenecks, and metrics such as throughput and cycle time hold sway—in project processes, the constraint is one or more critical paths (composed of critical activities) that govern the key metric of overall project duration. The 1960s saw extensions to activity network methods that accounted for resource constraints and multiple projects—i.e., resource-constrained project scheduling (RCPS) and resource-constrained multi-project scheduling (RCMPS) (see Ballestín and Leus (2009) and Browning and Yassine (2016), respectively, for recent examples and citations)—as well as probabilistic networks that account for path contingency (e.g., Pritsker and Happ 1966; Taylor and Moore 1980). With heavy involvement from the OR community, this research emphasized network modeling, optimization, and the development of heuristics and meta heuristics for computationally-intensive (NP-hard) problems. These problems provided fertile ground for an enormous body of quantitative and analytical literature. Several authors (e.g., Elmaghraby 1995; Herroelen 2005; Hartmann and Briskorn 2010; Schwindt and Zimmermann 2015) have provided extensive reviews and discussions of aspects of this research. Project processes have also provided the basis for many other research topics such as resource loading and leveling (e.g., Woodworth and Willie 1975; Gather et al. 2011); network structure and complexity (e.g., Browning and Yassine 2010); task switching (by multi-project workers) and activity preemption (e.g., Bendoly et al. 2014); and activity overlapping, crashing, and other time-cost tradeoffs (e.g., Pilot and Pilot 1996; Roemer and Ahmadi 2004; Meier et al. 2015). However, beyond PERT/CPM, crashing, and a few basic priority rule heuristics for RCPS, the bulk of this research has not filtered into the mainstream of basic PM textbooks, guides, or practice.

2.2 PM Tool Development

Another huge category of research concerns the development of PM tools. Although many of these tools originated in research papers or government manuals, numerous books now provide comprehensive presentations of these tools and their various extensions and elaborations. Some of the most prominent tools include the project charter (e.g., Meredith et al. 2015); the work breakdown structure (WBS—e.g., Miller 2008); earned value management (EVM—e.g., Fleming and Koppelman 2010); risk management (e.g., Hillson 2006); critical chain scheduling (Goldratt 1997; Raz et al. 2003); stage gates (e.g., Cooper 2001; Chao et al. 2014); capability and maturity models (e.g., Ibbs and Kwak 2000; Chriissis et al. 2011; Mishra et al. 2014); and project evaluation, selection, and portfolio formation tools (e.g., Meredith et al. 2015). The PMBOK (PMI 2013) and various PM textbooks (e.g., Meredith et al. 2015) describe many additional tools. Over the years, empirical research has explored the efficacy and pervasiveness of various tools, often finding that only the simplest tools are widely used and that many tools are not used appropriately (e.g., Liberatore and Titus 1983; Besner and Hobbs 2008; Caughron and Mumford 2008). While not always connected to PM research, the development of PM software tools (e.g., Primavera® and Microsoft Project®) has significantly affected the practice of PM over the past few decades. These software tools deal primarily with activity scheduling and resourcing and intra- and inter-project coordination and are pervasive in many project-based organizations (e.g., Liberatore and Pollack-Johnson 2003).

2.3 Organizational Coordination

The challenge of organizing the people and teams performing projects in ways that facilitate their coordination motivates another stream of PM research (Söderlund 2011b). The now-ubiquitous
matrix organizational structure (e.g., Galbraith 1973) emerged from roots in the field of PM, which had to innovate ways of performing short-term, cross-functional work within longer-term, functional organizations. Early on, scholars recognized the significance of coordination (or integrative) mechanisms, which in turn depend on activity relationships (i.e., the project process) and uncertainties (e.g., Thompson 1967; Galbraith 1973; Allen 1977; Allen and Henn 2007; Shenhar and Dvir 2007a). Some recent efforts along these lines have used design structure matrix (DSM) models to explore the targeting of integrative mechanisms such as co-location, electronic communication tools, heavyweight project managers (Clark and Wheelwright 1993), etc. (e.g., Eppinger and Browning 2012).

2.4 Product Development (PD) and Innovation Management

Another stream of PM research concerns PD and innovation management projects specifically. Although PD projects provided much of the initial impetus for the PM discipline (Gaddis 1959), increased understanding of the process of engineering design prompted a new set of project process models and perspectives, initially separate from those discussed in Section 2.1. PD is an iterative process (e.g., Smith and Eppinger 1997), often with many artificially overlapping activities, pervaded by rework (e.g., Cooper 1993; Mitchell and Nault 2007; Sosa 2014). In contrast to the natural parallelism of independent activities (assuming sufficient resources), artificial concurrency occurs when two or more activities are deliberately overlapped despite one- or two-way dependencies among them. This is often possible in projects, because an assumption can often act as a proxy for an activity’s missing input. Yet, most of the models in Section 2.1 assume “100% yield” and acyclical processes. “Concurrent engineering” (overlapping product design with the design of its production process) and a desire to drastically decrease projects’ durations increased the artificial overlap of otherwise sequential project activities (Starr 1992; Jayaram and Malhotra 2010), thus providing benefits while compounding rework (due to work done with only partial or preliminary inputs). Hence, a different breed of project process modeling frameworks emerged to deal with the realities of iteration, rework, activity overlapping, and other characteristics of complex PD processes—see Browning and Ramasesh (2007) for a review. One sub-stream of such models used temporal DSMs to capture cycles in the project process architecture, which revealed insights such as where greater rework was actually desirable to speed up a project (e.g., Eppinger and Browning 2012). Other researchers (e.g., Ford and Sterman 2003; Lyneis and Ford 2007) employed systems dynamics models to characterize projects not in terms of their activity structure but rather in terms of general amounts of work, rework, and other factors. However, most of the research in this stream of PD project management, while several decades old, has yet to make its way into mainstream PM textbooks and practitioner guides, probably because of its perceived complexity. Unfortunately, even the general concept of rework, which is ubiquitous in many other types of projects such as construction (e.g., Love et al. 2008), has so far received little assimilation into the mainstream PM guidance.

2.5 Project Portfolio Management (PPM)

Project portfolio management (PPM) deals with collections of simultaneous projects. According to Cooper et al. (2002), four goals in PPM are maximizing portfolio value, balancing the portfolio, aligning the portfolio with strategic business objectives, and optimally allocating resources. PPM tools include the Boston Consulting Group’s product portfolio matrix, risk-reward diagrams, bubble diagrams, etc. (Rad and Levin 2006) that visualize the portfolio with respect to multiple criteria. PPM utilizes various project rating and scoring techniques (Fichman
et al. 2005; Terwiesch and Ulrich 2008) as well as mathematical programming and optimization techniques such as non-linear, integer programs (Dickinson et al. 2001; Gutjahr et al. 2010); mixed-integer programs (Beaujon et al. 2001; Jiao et al. 2007); and dynamic programs (Kavadias and Loch 2004). Out of the technological innovation, PD, and entrepreneurship research published from 1954–2003 in *Management Science*, Shane and Ulrich (2004) found that “product planning and portfolios” comprising the second largest sub-stream yet had “found very little use in practice.” Perhaps this is because most PPM research to date has focused on the strategic level (e.g., project selection), the purview of executives, rather than the operational level (e.g., project execution), the domain of project managers. Opportunities exist to extend the PPM literature towards portfolio execution and control at the level of the dynamic, operational decisions important to practicing managers (Verma and Sinha 2002; Anderson and Joglekar 2005; Bendoly et al. 2010; Verma et al. 2011; Browning and Yassine 2016), and there are still opportunities at the strategic level to improve the way projects are leveraged to execute business strategies (e.g., Hutchison-Krupat and Kavadias 2014; Maylor et al. 2015).

### 2.6 Other Empirical Research

PM has provided a rich field for a variety of empirical research. Quite a bit of this work has focused on the factors driving project and PM success and failure (e.g., Morris and Pinto 2004; Dilts and Pence 2006; Pinto 2007; Scott-Young and Samson 2008). Examples of key factors in such studies include planning, executive support, project manager capabilities, collaboration tools (e.g., Bardhan et al. 2013; Peng et al. 2014), and systems thinking (Frank et al. 2011; Bendoly 2014). Empirical researchers have explored varied topics such as the effects of information access (Bendoly and Swink 2007), the project life cycle (Söderlund 2011b), project termination (e.g., Dilts and Pence 2006), the effects of project changes (e.g., Dvir and Lechler 2004), how cooperative planning can reduce rework (Mitchell and Nault 2007), the causes of deviations from project plans (Munthe et al. 2014), the roles of tacit and explicit knowledge in process improvement projects (Anand et al. 2010), and the characteristics of a good project manager (e.g., Dvir et al. 2006). Other researchers have studied project monitoring (e.g., Vanhoucke 2010; 2011), information systems (Ahlemann 2009), project reviews (Rozenes et al. 2006), and the efficacy of blending varied styles of monitoring and control (Lewis et al. 2002). Topics such as team roles (e.g., Goodman and Goodman 1976) exemplify the myriad of pertinent connections with the broader management literature on topics such as teamwork, organizational effectiveness, human resources, etc. Indeed, the project is a common unit of analysis in many types of organizational and management research. However, research findings pertinent to PM have sometimes taken many years to migrate into the mainstream of PM knowledge, theory, and guidance (when they have at all).

### 2.7 Perspective

Looking back at PM research over the last sixty years suggests several generalizations. First, relevant PM research streams exist in many sub-disciplines of management. Söderlund (2011a) reviewed PM research in thirty leading journals and organized it into seven “schools of thought” (optimization, success factors, contingencies, behaviors, governance, relationships, and decisions) that “vary in terms of their main focus and use of the project concept, major research questions, methodological approaches and type of theorizing.” Although it provides the benefit of many theoretical underpinnings and perspectives, this diversity also implies a fragmentation of basic
concepts and definitions, thus prompting a need for greater awareness, cross-fertilization, and consolidation across the streams (Shenhar and Dvir 2007b; Söderlund 2011a).

Second, although originally practitioner-driven, academics found particular PM topics and models, such as the scheduling of activity networks, amenable to their preferred analytical approaches such as optimization. However, applying such methods required abstracting the complex reality of PM with many simplifying assumptions, such as: time and cost matter more than the quality of results, project goals are clear and stable, *a priori* it is possible to identify all of the activities required to reach the goal, decision makers are risk-neutral, and “soft” factors (e.g., worker morale, people issues) can be ignored. This tendency to distill and optimize simplified analytical models was driven by the preferences of researchers, but it also reflects the predilection of reviewers and editors to conform PM research to the conventions of other disciplines whose models are more naturally amenable to optimization (e.g., job shop scheduling).

Third, although PM is often described in terms of balancing project time, cost, and quality/scope/performance (e.g., PMI 2013), most research and tools have focused on the former two dimensions while neglecting the third, because it is more difficult to measure. Almost all of the scheduling literature, and tools such as EVM, do not explicitly address the quality of a project’s results—or ignore it completely. (Mainstream tools such as EVM also ignore uncertainty, risk, and opportunity in all three dimensions.) Fourth, mainstream PM textbooks and guides have focused on standard, generic PM approaches applicable to all kinds of projects but do not yet provide clear guidance on how to tailor or scale methods by project characteristics such as size, type, risk, etc. A “one size fits all” mentality exists in the development and implementation of many PM methods and tools (Shenhar 2001). Lastly, it is worth noting that the perceptions of many practicing project managers seem to be influenced heavily by the current capabilities of PM software tools. 

3 Looking Around: The Current Situation in PM Research

The current situation in PM research is a mix of old and new. The aforementioned streams endure to varying extents, but many perspectives are changing for several reasons. One of these reasons is that, despite the plethora of PM methods and tools, project failure rates remain stubbornly high (e.g., Standish 2001; Mishra et al. 2014). Why? One cause is the continued ignorance of many project managers about the methods and tools—or of how and when to use them effectively. Many project managers find themselves in that role without any formal preparation or awareness of PM methods. Yet many experienced and competent project managers are well aware of the conventional methods and tools but choose not to use them. This suggests another possible explanation for project failures—shortcomings of the methods and tools themselves, especially for particular types of projects and in particular situations. The effectiveness of PM methods and tools is likely contingent on project characteristics: some projects may need a very different set of methods and tools.

Two more causes of high project failure rates stem from the definition of success and information asymmetries. How challenging are a project’s goals (Browning 2014)? How risky are they (Chao et al. 2014)? Were stakeholder expectations inflated to secure project approval (Flyvbjerg 2014)? Setting the bar too high, deliberately or otherwise, makes failure almost inevitable (and setting the bar too low leaves value “on the table”). Whatever the root causes of project failures, the fact that failure rates remain high calls into question the adequacy of the contemporary, mainstream methods, tools, and “discipline” of PM (in terms of capability and/or implementation), which implicitly claim to be normative, rational, and self-evidently correct (Williams 2005). This has sparked calls for a different way of thinking about PM research (e.g., Gerald et al. 2008).
A second reason for recent changes in PM research is the desire of PM-centric scholars to increase its stature in the academic community (e.g., Söderlund and Maylor 2012). Originally practitioner-driven, PM journals are still seen as secondary by many academics. However, the quality and rigor of PM research and journals has improved greatly over the past twenty-five years (Turner 2010), and excellent PM research now appears in PM journals as well as in the elite OM, OR, MS, DS, and management journals.

On the other hand, technically rigorous academic research, published in elite journals, has often been guilty of providing little direct value to practicing project managers. “Practitioner-driven” should not be a pejorative term to academics when it refers to the topics and issues addressed by scholarly research. But addressing important PM topics and issues will require a change in prevailing research methods, including a retreat from the highly-restrictive and over-simplified assumptions made by many abstract models over the past 60 years (Williams 2003). What is the point of worrying about whether a model’s solution is within ±3% of the optimum if the model’s entire input is merely a forecast within ±50% of reality? Scholars need a new openness to improvisational approaches, action, and engaged research (e.g., Cicmil et al. 2006; Maylor et al. 2015), and richer models (which may not be amenable to closed-form solutions or optimization). These changes are already underway and have motivated some of the contemporary research streams highlighted in this section.

3.1 Agile PM

A reaction to the prevailing PM methods and tools sprung from practitioners in the late 1990s in the realm of software development. Going by several names, but most prominently as Agile PM (e.g., Highsmith 2009), these techniques deemphasize the use of formal PM processes and planning in favor of a “learn and adjust as you go” approach—albeit one with its own disciplines and conventions for teamwork and daily and weekly planning. Agile PM recognizes that a project’s objectives are often much less clear than they might seem, that the right work to do is difficult to forecast far in advance, and that the early delivery of prototype results generates value-adding feedback for project direction. Agile PM has many proponents, and studies have shown positive outcomes (e.g., Serrador and Pinto 2015), particularly in the software industry, but researchers have yet to fully explore the theory, mechanisms, implications, limits, and contingencies of Agile PM. It may work less well outside of the world of software, which is much easier to redefine at various points during its development than hardware or physical construction. Large, complex, and safety-critical software projects also seem to require more careful planning than most Agile PM implementations propose (Boehm and Turner 2003).

3.2 Factors Distinguishing Different Types of Projects and PM Methods

On the academic side, researchers have advocated for a contingency-driven approach to PM—i.e., tailored approaches for different kinds of projects—e.g., PD and innovation projects (e.g., Pons 2008; Schultz et al. 2013) or software development projects that can benefit from code reuse (Liu et al. 2007). Scholars have sought to identify the key contingency factors (e.g., project size, risk, complexity, novelty, uncertainty, pace, etc.), resulting project typologies, and implications for PM (e.g., Shenhar 2001; Lewis et al. 2002; Crawford et al. 2005; Pollack 2007; Söderlund 2011b). For example, Chandrasekaran et al. (2015a) recommended managing projects differently based on their learning goals with respect to exploration versus exploitation. Zwikael et al. (2015) found that the effectiveness of planning is mediated by the amount of project risk. The degrees of novelty, complexity, uncertainty, ambiguity, and dynamism in projects have been highlighted as
key contingency factors with implications for PM methods and approaches (e.g., Tatikonda and Rosenthal 2000; Geraldi et al. 2008; Collyer and Warren 2009; Lenfle and Loch 2010; Geraldi et al. 2011; Nair et al. 2011). Liu (2015) investigated different modes of project control in complex situations, and Chandrasekaran et al. (2016) studied how projects should alter management approaches under complexity and dynamism.

Many projects entail a high degree of uncertainty and ambiguity—i.e., “known unknowns” and “unknown unknowns” (or “unk-unks”). While the former are addressed by the conventional methods and tools for project risk management, the latter continue to surprise project managers and derail the best-laid plans. Recently, scholars (e.g., Loch et al. 2006) have provided insights on managerial techniques for projects fraught with unk-unks. Sommer and Loch (2004) distinguished two approaches, selectionism and learning, for executing a PD project depending on its amounts of complexity and uncertainty, and later (2009) they explored appropriate incentive contracts for projects with unk-uns. Complexity, complicatedness, and dynamism, as well as behavioral and cultural factors such as mindlessness and organizational pathologies, increase the likelihood of unk-uns lurking in a project’s future (Ramasesh and Browning 2014). However, by understanding the influences of these driving factors, knowing where to look, and using the tools of directed recognition, it is possible to proactively convert many “knowable unk-uns” into “known unknowns” (Browning and Ramasesh 2015). For example, Loch et al. (2008) provided a rich case study showing how a firm diagnosed unk-uns in a new venture. Overall, we still need to learn much more about how PM methods should be tailored and scaled by project type (Winter et al. 2006).

### 3.3 People, Teams, Behaviors, and Knowledge Management

Other contemporary PM research has begun to address people, teams, and behavioral issues, such as motivation and authority. While providing some new findings specifically for project managers, this research rightly draws from deep streams of general management literature on these topics. For example, Cicmil et al. (2006) suggested the incorporation of social theories into PM, and Ravid et al. (2012) discussed PM issues regarding human resources and teams. Recent interest among OM scholars in experimental and behavioral topics has spread to PM research as well. For instance, Chandrasekaran and Mishra (2012) explored the effects of team autonomy and psychological safety of project workers. Wu et al. (2014) studied team members’ tendencies to procrastinate, the need for managers to reward accomplishments early in a project, and the counter-intuitive benefits of diverse and fluid teams. Hutchison-Krupat and Chao (2014) used an experiment to highlight the importance of project risk characteristics, distributed responsibility, and an organization’s cultural tolerance for failure in constructing incentives for project managers. Bendoly et al. (2010) used an experiment to examine project managers’ psychological attachment to particular resource allocation solutions, finding a negative relationship between difficulty in work-planning and willingness to share resources. Sosa (2014) drew connections between personnel relationships and the task relationships that trigger rework. PM research also connects to the knowledge management literature—e.g., Anand et al. (2010)’s study of the effects of explicit and tacit knowledge in process improvement projects.

### 3.4 Outsourcing and Partnering with Other Organizations

Another stream of contemporary PM research studies outsourcing portions of a project and partnering with other organizations. Some key findings in this area are that contract structures indeed matter (MacCormack and Mishra 2015; Tang et al. 2015), that trust is necessary but not
sufficient for success (e.g., Brinkhoff et al. 2015), that good planning and team stability help (Narayanan et al. 2011), and that partnering scale and scope matter (Mishra et al. 2015). Tripathy and Eppinger (2013) developed a quantitative model of work and coordination times for each organizational unit and used these to optimize the allocation of work across the units. As projects become more diverse and multi-national, partnering and outsourcing will continue to increase in importance for practicing project managers.

3.5 Systems Views and Structural Models

Some recent research has recognized projects as complex systems. For example, Section 2.3 noted PM research involving systems dynamics models, which have provided important insights into the effects of general feedback loops such as rework and schedule pressure. These holistic-view models continue to provide useful insights that transcend a reductionist paradigm (such as a WBS). Nevertheless, decomposition remains a valuable approach to understanding complex systems (Simon 1996), including projects. The main criticism of the reductionist/decomposition approach is that the value of a system is much greater than the sum of its parts. A significant amount of this additional value can be understood and explored, however, by accounting for relationships among the decomposed parts (e.g., Söderlund 2002; Williams 2005). For example, while not captured in a WBS, the input-output relationships among activities matter greatly. Although activity network models do capture some of these dependencies, they tend not to accentuate them nor allow their properties to vary (Browning and Ramasesh 2007).

Yet some richer, structural models are elaborating on the activity-deliverable paradigm for modeling a project’s process system (where deliverables represent the typical outputs of project activities, which become inputs to other activities, giving rise to precedence relationships). For example, Meier et al. (2015) used a rich model of activities and relationships to explore project time–cost tradeoffs due to process architecture, iteration, crashing, overlapping, and work policy (rules about when to start and stop work), finding that the time–cost benefits of crashing and overlapping increase with increasing iteration, and that varied work policies (e.g., rushing and being wrong versus waiting and being late (Loch and Terwiesch 2005)) have enormous time–cost tradeoff implications.

Some scholars are porting complex adaptive systems (CASs) theory over to PM research, discussing how projects exhibit the characteristics of a CAS, such as self-organizing, connected agents that cause emergence and co-evolve with their environment (e.g., Johnston and Brennan 1996; Jaafari 2003; Browning 2007; Saynisch 2010). For example, departing from the conventional assumption that a project’s activity network can and should be planned a priori, Lévárdy and Browning (2009) defined (1) a superset of general classes of activities, each with varied modes (i.e., options for alternative combinations of inputs, duration, cost, and expected benefits) and (2) simple rules for activity mode combination and self-organization. Simulating thousands of adaptive cases provided insights on the emergence of likely process paths across the project landscape, the patterns of iteration along the paths, and the paths’ costs, durations, risks, and values. As these models demonstrate, the CAS paradigm provides a promising avenue for further PM research.

Meanwhile, others are looking at projects from the perspective of other subsystems besides their process, such as their result (e.g., a product design), organization, tools, and goals (Browning et al. 2006). Each of these subsystems has an architecture or structure that can be investigated to increase our understanding of important patterns such as modularity and cyclicity (e.g., MacCormack et al. 2006; Braha and Bar–Yam 2007; Starr 2010; Sosa et al. 2013). Some have studied two or more of these subsystems in tandem—e.g., product and organization (Sosa
et al. 2004; Gokpinar et al. 2010)—and opportunities beckon to explore further implications of these subsystems individually and collectively (Eppinger and Browning 2012).

As models of projects become richer and more complex in themselves, they run into several difficulties such as being hard to populate, maintain, verify, validate, and understand. Little (1970) highlighted the tradeoff between simplicity and completeness in models—and noted that managers prefer the former. However, much as firms such as Dell Computer innovated hybrid production systems that provided advantages while departing from the diagonal of the product-process matrix (Hayes and Wheelwright 1979), research on architecture frameworks (AFs) points towards possibilities for modelers and managers to “have their cake and eat it too.” By providing multiple, simple views of an underlying, complex model, AFs enable modeling experts and knowledge managers to capture and improve big, rich data sets while providing managers (and other users) with access to subsets of the data through simple visualizations of their own choosing (such as Gantt charts). By taking a more complete model and making it seem simple to users, while at the same time maintaining integration and synchronization of the information across all such views, AFs could provide a significant breakthrough in PM research and practice. AFs emerged from the field of information systems and are also used extensively by systems engineers, mainly for product systems, but recent work has demonstrated their efficacy for project process systems (Browning 2009) and in conceptualizing purpose-view alignment as a potential determinant of project success (Browning 2010). Future work could extend the AF approach to the project organization, tools, and goals subsystems, as well as to uniting models of all five project subsystems.

3.6 Measuring Progress and Value

Taking a system view of project processes has also produced insights about progress and value in projects, the measurement of which has been a long-standing challenge. Some contemporary projects use EVM (see Section 2.2) to plan and track progress, but this has the aforementioned shortcomings of ignoring technical performance (quality), uncertainty, and risk, as well as failing to distinguish perceived and actual progress (Cooper 1993). Viewing a project as a value-creation process (e.g., Winter et al. 2006; Winter and Szczepanek 2008), Browning et al. (2002) developed a method for planning and tracking the evolution of project performance attributes in terms of uncertainty and risk—recognizing that progress and the addition of value occur as a project eliminates the risks of not meeting its performance goals. Browning (2014) later extended this framework to fully integrate cost, performance, value, uncertainty, risk, and opportunity over project time. The framework views a project as the finite work done to decrease the portion of the project’s (goal) value at risk, where reducing risk to zero implies having achieved the chosen goals. This view of adding value as removing “anti-value” (i.e., risks to value) has also informed process improvement approaches for innovative projects (e.g., Browning and Sanders 2012). Some have attempted to apply approaches such as Lean and Six Sigma (which were developed to improve repetitive processes) to project processes but have found this transference challenging. A systems view of project processes allows one to escape the reductionist question asked by Lean (“Is this a value-adding activity?”) by acknowledging that value is an emergent property of the process system, not something that can be fully attributed to individual activities. (For example, a perfect, value-adding activity could not add value if it did its work based on faulty inputs or bad assumptions.) Therefore, value is not only a function of how individual activities are done but also of how activities work together in a process (which again signals the importance of the activity relationships). Some recent models (Browning and Ramasesh 2007; Lévárdy and Browning 2009) have begun to adopt this perspective on risk and value.
PM research continues to evolve. Some of the forces guiding the changes include the aforementioned desires to make PM research rigorous and relevant (which affects both topics and methods). Increasing the impact of PM research on practice will require scholars to improve observation, description, and understanding of the real challenges faced by project managers. This calls for rich field studies that account for project work, people, tools, goals, and contexts. It also begs for more nuanced studies to validate PM methods and tools and their implementations. Modeling will continue to provide valuable insights. However, the complexity of PM will require a greater openness to richer models that capture larger amounts of project data and co-evolve with their subject. Empirical research will continue to be essential for validating past and current practices, but research to develop new methods and tools will also be key, as relevance to practitioners depends not only on identifying “best practices” but also on developing “next practices.” (The author first heard this expression from a practitioner at Lockheed Martin Corporation.) Just as projects must integrate across business functions, PM research must continue to span and integrate methods and theories from many relevant disciplines to hypothesize and test new practices. PM research can thereby contribute richly to the wider fields of OM and general management (Geraldi et al. 2008).

PM is an exciting and target-rich research area with no shortage of open questions. Sections 2 and 3 have already mentioned several important topics for future research: tactical portfolio management, strategic alignment of projects, guidance for tailoring PM approaches (including Agile PM) by project type, viewing a project as a complex adaptive system (CAS), investigating project subsystem architectures, and leveraging the concept of architecture frameworks (AFs). In addition to these, this section will now suggest several other promising areas of inquiry.

What are the criteria for judging project success? The opening paragraph of Section 3 mentioned that success depends partly on the chosen goals, how high “the bar” is set (Browning 2014). Projects have multiple objectives pertaining to their cost, duration, path, and scope and quality of results. Some of these results matter more than others. Some may change during or after the project. What initially seems like a catastrophic failure (e.g., the Sydney Opera House which was over budget, behind schedule, and initially deemed ugly by many) may later turn out to be a success—or vice versa (e.g., the Iridium satellite project, which elegantly met its requirements, but few actually wanted its result) (e.g., Shenhar et al. 2001; Jugdev and Müller 2005; Thomas and Fernández 2008). Moreover, executives judge success and failure differently than project managers (Dilts and Pence 2006). How are project objectives chosen? How can failures of project design be distinguished from failures of execution?

How can projects balance control and improvisation, formality versus discretion (Naveh 2007), following prescribed processes versus innovating better ones (Geraldi et al. 2008), and exploiting known approaches and best practices versus exploring next practices (Leybourne and Kennedy 2015)? What is the right amount of project structure and standardization (Browning 2007)? As a project’s required level of innovation increases, conventional PM techniques seem to lose efficacy, but the process structure seems to retain its efficacy (Schultz et al. 2013). How can projects best be designed for adaptability to unforeseen future circumstances? How much investment in such adaptability is appropriate? How can projects increase their agility and flexibility (Lenfle and Loch 2010)? Can commitment network templates provide enablers of fast, generic, baseline project designs (Browning and Ramasesh 2007)? That is, can designing and pre-negotiating general agreements among various participants in a typical kind of recurring project accelerate project planning and adaptation?
Further research questions include the following:

- What are the most important and relevant theories for PM (Jugdev 2008; Söderlund 2011b)? How can appropriate theories be distinguished from practical methods and tools that may or may not depend on them (e.g., Schmenner and Swink 1998)?

- What is the right amount of investment of resources (time, money, personnel) in project planning? Too little planning (fire prevention) is associated with fire-fighting (Wearne 2014) and project failure, whereas too much could be a waste of resources, especially when a project is likely to change direction (and render a detailed plan obsolete). For example, Choo (2014) found a U-shaped relationship between the length of the Define (planning) phase in Six Sigma projects and their overall duration: a longer Define phase (more up-front planning) was associated with the faster execution of the rest of the project—to a point, past which further elongation of the Define phase correlated with longer projects.

- What is the right type of project planning? How can planning be more effective (Lawrence and Scanlan 2007; Leleur 2007; Salomo et al. 2007)?

- When and how should projects incentivize “fast failure” and early expenditures that save money later, versus delaying decisions and expenditures as long as possible?

- How can project value be defined, in terms of benefits and sacrifices, for highly uncertain R&D projects (e.g., Browning 2003; Maniak et al. 2014)? How can this value be balanced among the contrasting preferences of multiple stakeholders (workers, clients, sponsors, etc.) (e.g., Thyssen et al. 2010)?

- How important is clear prioritization across projects in a portfolio? What are the implications of unclear prioritizations for cooperation, coordination (Söderlund 2011b), and project value?

- A program is “a group of related projects . . . managed in a coordinated way to obtain benefits not available from managing them individually” (PMI 2013)—i.e., essentially a mega-project. How should program management be conceptualized in contrast to PM (Pellegrinelli et al. 2011)? What are its distinctive characteristics, challenges, methods, and tools?

- What are best and next practices for project management offices (PMOs—e.g., Meredith et al. 2015), the permanent, “functional” organizations that provide a number of support and guidance roles to an organization’s various projects and project managers?

- How can project managers balance time, cost, and scope/quality/performance for maximum value (Browning 2003; Swink et al. 2006; Liberatore and Pollack-Johnson 2013; Browning 2014; Cohen and Iluz 2014)?

- Out of all the information that is or might be available to project managers, how should they distinguish signals from noise (Sanchez and Perez 2004; Haji-Kazemi et al. 2013; Ramasesh and Browning 2014)? Setting the filters too tightly will miss weak but important signals and early warning signs (e.g., Williams et al. 2012), while taking in too much information can cause overload. How should such methods account for distortions in project status reporting (Snow and Keil 2002)?

- What improvements are needed to software tools to enable improved PM practice? Could research findings be better disseminated into practice by embedding them in software tools?

- What are the best ways to deal with the tensions that emerge between temporary (i.e., project) and “permanent” organizations? Does the temporary nature of projects promote short-term thinking? What are the mechanisms for transitioning workers into and out of projects?
• How can and should knowledge and learning be managed across projects (Sydow et al. 2005; Browning 2009; Swan et al. 2010; Leybourne and Kennedy 2015)? To what extent can project knowledge be captured and reused on similar, future projects? Are particular methods, structures, tools, or incentives more or less helpful?
• How can projects be designed to fit desired characteristics of size, scope, cost, duration, resources, risk levels, risk preferences, objectives, and managerial approach?

This list is by no means comprehensive; it provides only a taste of the possibilities for future PM research.

5 Implications for Managers

Because of the temporary nature of projects, as well as their typical novelty, complexity, and uncertainty, project managers are some of the busiest managers in most organizations. Therefore, many project managers have little time or inclination to digest the latest results from PM research. To be of value to practicing managers, such research findings need to be easily and quickly applicable to new and ongoing projects. It is especially helpful when these findings are distilled into simple rules that managers can apply when making plans and decisions—along with specific guidance on when, where, and to what extent the findings apply—and when such results are embedded in the software tools used to support PM. However, the production of accessible research findings is greatly facilitated when practitioners share their current challenges and concerns through engagement with academics at conferences and symposia.

Of course, the onus is also on practicing managers to continue to learn and improve by discovering and implementing the cornucopia of useful findings emerging from contemporary PM research. General findings have long since concluded that the primary reasons for many project failures include lack of good planning, risk management, stakeholder support, and PM skills. Complexity, novelty, uncertainty, iteration, and rework all make effective planning more difficult, but, as noted at many points earlier in this chapter, much guidance already exists in the POM literature to ameliorate these challenges. Helpful findings on issues in ongoing projects such as project control, resource allocation, tracking progress, and delivering value have already been produced. Good practices for managing across projects, working with partner organizations, managing commitments, and managing organizational knowledge also exist in the literature—but as of yet have failed to be widely implemented in and across projects. Thus, while much further research on PM is needed, many insights from prior research have yet to be put into practice. Indeed, the existing literature is already rife with potential value for practicing project managers.

6 Conclusion

This chapter has provided a look around, back, and forward at PM research, yet all of this is just a sampling, merely “the tip of the iceberg.” Hopefully, this perspective will prompt researchers and project managers to dig deeper into interesting areas, broaden their outlook, and contribute to a field that is both intellectually compelling and practically significant, with room for a variety of research methods. The goal of this chapter is to spark discussion and research—regardless of its eventual directions. It does seem especially useful, however, to encourage the development of methods and tools to help planners and managers design projects with more predictable size, scope, deliverables, cost, duration, resource needs, and risk levels; that apply the most appropriate
managerial styles and techniques for their situation; and that empower more rapid adaptability to unforeseen situations.

References and Bibliography


References and Bibliography
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