

# A Social Science Which Forgets its Founders is Lost

by

Paul Adler

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## C H A P T E R 1

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# INTRODUCTION

## A SOCIAL SCIENCE WHICH FORGETS ITS FOUNDERS IS LOST

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PAUL S. ADLER

ORGANIZATIONS have deep and pervasive effects on our lives at work and beyond. The previous century witnessed a massive transformation of advanced capitalist societies: whereas families and neighborhoods once constituted the basis of society, now large organizations play a pivotal role in every sphere (Perrow 1991). And the most recent decades have witnessed further, equally profound and disconcerting changes in this landscape. The aim of the present volume is to help scholars in organization studies better understand these changes. In particular, we highlight the enduring value of some of the older work in this field.

The field of organization studies has become well established in both sociology departments and professional schools, most notably in business schools. However, in the course of this institutionalization, the field has progressively lost contact with its founding writers. To some, this represents a welcome sign of maturation: they might quote Alfred North Whitehead: 'A science which hesitates to forget its founders is lost' (Whitehead 1916 413). Whitehead, however, was writing about the natural sciences, and the premise of this volume is that the social sciences are in this specific respect quite different, because in our field founders continue to play a crucial role. We have thus reversed Whitehead's warning as the clarion call for this volume.

Without minimizing the potential contribution of more recent scholarship nor the value of earlier scholarship that has been less celebrated, we believe that these classics present unusually rich resources for research today. Most notably, these writers all struggled to make sense of the momentous social changes of their times. In contrast, organization studies today focuses too little on the big issues of our own times and too much on the narrower concerns of academic peers. The classics, this volume argues, serve both as a source of specific insights and also as encouragement to lift and broaden our aspirations.

Alongside some thematic chapters, this volume includes contributions on each of several classic authors. Each chapter addresses the author's ideas and his or her context, the impact of these ideas on the field of organization studies so far, and the potential future research these ideas might inspire. The goal is not reverential exegesis, but rather to examine how these classics can enrich and enliven organizational research—how they can help us make better sense of the social changes currently under way, and perhaps equip us to act more intelligently in our efforts to participate in those changes.

This Introduction first explains why organization studies should reconnect with these classics, and then provides a thumbnail sketch of each of the contributions to this Handbook.

## WHY READ THE CLASSICS?

Organization studies is an interdisciplinary field, bringing together sociology, psychology, economics, political science, as well as other disciplines. The present volume focuses on sociology. Sociology was foundational in shaping the field in its earliest years and has continued to be an important influence. The sociological lens affords unusual depth of insight into the technological, economic, cultural, and political forces that shape organizations both from within and without.

Notwithstanding its interdisciplinary constitution, organization studies suffers from increasing intellectual insularity. Research in organization studies refers increasingly to the field's own journals and less and less to journals in sociology or the other contributing disciplines (Augier, March, and Sullivan 2005). Organization studies is increasingly cut off not only from contemporary sociological research but also from sociology's classics. Statistical analysis of the works cited in articles published in the major journals of sociology and organization studies shows that, on the one hand, the absolute number of citations to the classics (specifically, those classic authors addressed in this volume, taken as a group) has continued at much the same level since the 1950s. At the same time, however, there has been an increase

in both the average number of articles published each year and the average number of citations per article. The net result is that the number of citations to classics as a proportion of all citations has fallen dramatically—to below 2 percent in the most recent period (Kennedy and Adler 2007). Moreover, many of these citations are merely ceremonial (Lounsbury and Carberry 2005).

This trend bodes badly for the intellectual development of organization studies. As Jeffrey Alexander (1987), Art Stinchcombe (1982), and others have argued, social sciences—as distinct from natural sciences—are considerably enriched by rereadings of their classics. There are, of course, many sociologists who argue that their discipline should take the form of a natural science and forget its founders. This aspiration goes back to some of these very same founders of the discipline: it can be traced from Comte through the work of Weber and Durkheim; it was given particularly sharp form in a famous essay by Merton (1967); and it has continued to inspire the ‘positivist’ wing of sociology, for whom the progress of sociological knowledge should rely on the accumulation of empirical, factual findings. According to this positivist view, theory should follow from facts, not precede them, and as a result, more recent theory should replace older theories, and there should be as little space for Marx, Weber, and Durkheim in contemporary sociology textbooks as there is for Ptolemy or Copernicus in contemporary astronomy textbooks.

However, this positivism has been increasingly challenged by a cluster of post-positivist ideas inspired by Kuhn’s (1970) history of science and a variety of movements in philosophy and social theory. Kuhn and subsequent historical and philosophical research showed that natural sciences too relied on prior non-empirical, theoretical commitments, even if it was only in periods of deep paradigm conflict that these commitments emerged as directly relevant to scientific debate. In the social sciences, unlike the natural sciences, such paradigm conflict is endemic: social sciences cannot escape the perennial value-laden debates over human nature, its role in behavior, and the nature of social order. The nature of social sciences’ subject matter thus ensures that they lie somewhere between the natural sciences and the humanities as regards the relative value of the latest research versus the classics (as argued by Alexander 1987, 2001). Classics serve crucial functions for social sciences: as noted by Alexander, they function as signifiers, allowing us to refer parsimoniously to whole world-views articulated in the works of the major classic thinkers; and more substantively, they continue to inspire new theoretical and empirical research because they encapsulate what were and remain unusually deep and compelling insights into human nature and social order.

Some sociologists hesitate to abandon positivism, fearing that this would mean also abandoning any hope of accumulating real knowledge and obliging us to embrace instead some variant of social constructivist relativism. Kuhn too struggled with this challenge in his analysis of the natural sciences, a field where the reality of progress in our understanding is difficult to deny. However, it is not all that difficult to square this circle once we remind ourselves that all the competing paradigms in

sociology take as a key goal to make sense of empirical features of the social world. None of them can claim the kind of success in that effort that we sometimes see in the history of natural sciences, where the success of one paradigm relegates the others to the dustbin of defeated doctrines; but competition within and between paradigms revolves largely around claims to greater success in rendering intelligible the empirical world. The competitive fortunes of different paradigms wax and wane; but for the field as a whole, the result of this process is real progress in understanding.

## WHY NOW?

The contributors to this volume share a concern that organization studies reconnect with broader social issues. Since ours is a society of organizations, many of these big issues are directly organizational, as evidenced by the headlines of the daily news: globalization, outsourcing, the pressure of financial markets on industrial firms, new technologies that obsolete old organizations, the fate of the individual, and the possibility of collective agency in the face of massive systemic forces of change, and so on.

The discrepancy between this list and the list of topics in recent organization studies research is both saddening and troubling. Saddening, because it represents a narrowing of scope, ambition, and concern compared to the founders of the field. Troubling, because this narrowing saps the vitality of the field. A field that hides its head in the sand when its ostensible subject matter is undergoing such massive turbulence is a field that risks losing any credibility (Walsh, Meyer, and Schoonhoven 2006; Clegg 2006). Moreover, it risks losing its 'franchise', its legitimacy as a key discipline in the broader public's effort to make sense of these changes. Organization studies competes with other disciplines, most notably economics, when various publics and policy makers reach for frameworks that help them make sense of social and economic issues, and it does not bode well for either the cogency of these public debates nor the future of the field if organization studies abandons these policy terrains.

It is striking that the sociology classics so directly addressed so many of the urgent social issues of their day. This real-world relevance surely explains much of their enduring appeal. In contrast, organization studies has moved in recent decades towards 'incremental, footnote-on-footnote research as the norm for the field' (Daft and Lewin 1990). In part, of course, the contrast between the classics and the field's present condition is a function of the increasing 'professionalization' of research. The world of research today is so very different from that of Marx's,

Weber's, or even Parsons's time, with the quasi-universality of employment in universities, standardization and specialization of training, formalized standards for promotion and tenure, and the elevation of the craft of academic journal article writing into an esoteric art. The way forward for our field is not to retreat from professionalism to amateurism; but we do need a richer form of professionalism, one that does not turn its back on academic rigor, but brings this rigor to bear on the burning issues of our time.

What is at stake in the present volume is not the ongoing debate about our immediate audience. Recently, we hear calls for the 'public sociology' advocated by Burawoy (2004) or the 'engaged scholarship' advocated by Van de Ven (2007). These are very legitimate in our view; but here we address a distinct, underlying concern: whether in our academic scholarship we are engaging the big social changes of our times, even if we do it in our own ways and forums. Our engagement in public debate adds little if it is not informed by rigorous research on appropriately focused research topics.

Daft and Lewin (1990) suggested several ways for organization studies to break out of the 'normal science straightjacket', in particular that we focus on organization design as a practical task, focus on more equivocal problems, and experiment with heretical research methods. To this list, we add another, arguably more fundamental strategy: reread our field's classics. These provide us not only with paradigms for rigorous engagement with big issues but also with powerful concepts for making sense of these kinds of issues.

## AN OVERVIEW

The body of this Handbook aims to give the reader a sense of what might be learned when we take inspiration from these classics. The volume is organized into two main sections—focused respectively on European and American writers—followed by an Afterword. Within each section, chapters have been sequenced so that the classics discussed are in approximately chronological order. The chapters are stand-alone contributions, and readers should pick their own path among them. The following paragraphs summarize the main ideas in each of the chapters.

In Chapter 2, Patricia Thornton discusses the value of the classics in developing compelling arguments. Thornton returns to Stinchcombe's (1982) six functions of the classics: touchstones, developmental tasks, intellectual small coinage, fundamental ideas, routine science, and rituals. She illustrates these functions with three mini-cases that show the important roles played by the classics in the development of research streams in organization studies. In particular, the mini-cases

outline how the theoretical constructs of disruptive technologies, institutional logics, and status-based markets—the wellsprings for vibrant research and teaching communities—descend from the classics. The mini-cases suggest that students should use the classics to guide the development of compelling arguments in the study of contemporary research problems and the systematic accumulation of knowledge.

In Chapter 3, Richard Swedberg presents Tocqueville as one of the first—possibly the very first—social scientist of any stature to be fully aware of organizations. In Tocqueville's work, especially in *Democracy in America*, the reader finds an elaborate analysis of different types of organizations—economic, political, and voluntary. Tocqueville emphasized the implications of the fact that organizations were composed of people coming together for the purpose of realizing some common goal. This fit well with Tocqueville's personal philosophy of freedom, where organizations were a means towards this end.

In Chapter 4, I address the relevance of Marx to organization studies today. This chapter takes the reader back to the core ideas of Marx and discusses the way they have informed an important stream of work in organization studies. The chapter offers a tutorial on Marx's basic theory and highlights the tension between two readings of Marx, an older, almost forgotten reading that I dub 'paleo-Marxist' and a more recent, 'neo-Marxist' reading. While the latter focuses on class struggle as the motor of history, the former argues that the direction of history—and the forms and vectors of class struggle—are determined by a deeper structural contradiction between the trend towards 'socialization' of the forces of production and the persistence of capitalist relations of production that enshrine private ownership of productive resources. This debate offers fruitful resources for making sense of both the endemic conflictuality of capitalist society and the changes within organizations that might facilitate a transition beyond this form of society. The chapter traces the implications of the Marxist view, in both its variants and in contrast with other theories, for several broad domains or organizational research both within the individual organization and in broader organizational fields.

In Chapter 5, Richard Marens offers a second perspective on Marx's influence. First, Marens broadens his scope to include a wider range of scholars who have been inspired by Marx but are less orthodox—Marxians, rather than formally Marxists—and second, he broadens the focus to address several different time horizons in capitalism's development. Marens argues that Marxian political economy offers a powerful remedy to a blindness increasingly acknowledged by contemporary organization scholars: if the 'environment' is so crucial to the structure and function of organizations, organization studies needs (1) a rich characterization of the structure of that environment and its evolution over time, and (2) an account of how organizations can act to transform that environment. Marxian theory, he shows, can enrich several streams of organizational research by addressing these two gaps.

In Chapter 6, Stewart Clegg and Michael Lounsbury lament the limited use of Max Weber's ideas in organization studies. In contrast to the more suffocating conceptualization of culture proffered by leading neoinstitutional scholars promoting an 'iron cage' imagery via Weber, Clegg and Lounsbury argue that a deeper reading of Weber provides a more nuanced understanding of culture, one that appreciates that culture is often contested and impregnated with power and domination. Revisiting Weber's well-known arguments about bureaucracy, they show that he viewed bureaucracies as culturally diverse and as sites of conflict among different forms of rationalities. In this new perspective, the 'iron cage' appears as more porous, opening up opportunities for new lines of inquiry and multilevel analysis, enriching connections to a broader sociological imagination. Clegg and Lounsbury emphasize that this imagery of a 'sintered' iron cage can be especially helpful in understanding new emergent logics of organizing such as those related to post-bureaucratic forms and associated new technologies that increase surveillance and rationalization beyond traditional bureaucratic incarnations.

In Chapter 7, Paul Du Gay offers a second perspective on Weber. He highlights a crucial ethical value criterion of Weber's sociology, one that has been largely neglected by scholars of organization: *Lebensführung*, the conduct of life. Du Gay argues that Weber's analysis is marked by concern for the survival of particular forms of 'character' or 'personality' whose life conduct unites practical rationality with ethical seriousness. Against the image of Weber as a grand theorist of the instrumental rationalization of modern life, Du Gay cites a growing body of work emanating from the humanities and social sciences, which paints Weber as a historical anthropologist greatly concerned with the ethics of office. In particular, he argues that Weber's analysis remains a key resource for scholars trying to understand contemporary developments in the reformation of organizational life and identity in the public sector, and in the institutions of government. Du Gay underscores the importance of a particular bureaucratic persona to the production of responsible government and highlights the risk that shifts to more flexible, 'post-bureaucratic' organizational forms pose to the practices of responsible government.

In Chapter 8, Pamela Tolbert and Shon Hiatt bring together Robert Michels's classic analysis of power in political parties and Berle and Means's landmark study of the public corporation. Although they focus on very different types of organizations, these works are linked by a common concern with the general problem of organizational leaders' propensity to exploit decision-making power for their own private interests, and thus to govern in ways that are contrary to the stated goals of the organization. This essay explicates the link between Michels's analysis of conditions that give rise to the formation of oligarchies in organizations and the analysis of Berle and Means of problems created by the separation of ownership and control in modern business firms. Reviewing Michels's arguments and synthesizing findings from research based on parties, unions, producer cooperatives, and social movement organizations, Tolbert and Hiatt create a new lens for

making sense of some of the contemporary problems of governance in modern corporations.

In Chapter 9, Frank Dobbin discusses Émile Durkheim and focuses on his masterpiece, *The Elementary Forms of Religious Life*. Durkheim's thesis here was that humans are driven to understand the world through collective classification and meaning-making. This idea informed an important current of post-World War II sociologists of knowledge and organizational sociologists, most notably Erving Goffman, Peter Berger and Thomas Luckmann, Mary Douglas, and James March and Herbert Simon. Since then, Durkheim's influence on organizational sociology has been mainly via neoinstitutionalism and second-generation power theory. This current attacks the rationalist assumption that the modern scientific-rational world is fundamentally different from the spiritual and religious worlds that preceded it: both religious and scientific-rational social systems are collectively constructed. Durkheim is thus an enduring stimulus challenging organizational scholars to step back from the rationalized practices of the modern firm and to ask how we come to believe those practices to be rational. Durkheim noticed that within primitive societies, there were myriad different ways of making sense of the world: the totem could be just about anything. For much of the twentieth century, organizational theorists were expecting organizations, both within and across societies, to converge on a single model. The most advanced organizations seemed to show the way of the future to less advanced organizations, just as, we thought, the most advanced societies showed the way of the future to less advanced societies. But organizational sociologists have come, like Durkheim, to see that organizations can take different forms in different rationalized societies. Durkheim's work is relevant today for our understanding of how rationalized societies arrive at different rationalized forms of organization, which worship different modern totems, whether the bureaucracy, the market, the network, the professions, or the business group.

In Chapter 10, Paul Hirsch, Peer Fiss, and Amanda Hoel-Green argue that Émile Durkheim's insights from *The Division of Labor in Society* can be extended to inform our understanding of the current shift from the nation to the globalized economy. While Durkheim emphasized the potential for material and social progress resulting from greater economic differentiation, he also cautioned against the threat of social instability and disorder ('anomie') that would occur in the absence of a meaningful integration in the economic system. This chapter argues that similar issues arise in the expansion of economic and cultural exchanges to a more global scale. After discussing the central concepts of mechanical/organic solidarity and moral/material density that underlie Durkheim's theory of modernization, the authors review Durkheim's influence on organization studies and argue that the fundamental question of social solidarity has disappeared from current research in organization studies. In response, the chapter offers a call for future empirical analyses and suggests five areas in which the role of solidarity might fruitfully be

explored, including topics such as the rise of outsourcing and the emergence of international institutions that regulate trade.

In Chapter 11, Barbara Czarniawska discusses the work of Gabriel Tarde—an intellectual rival of Émile Durkheim and, judging from many contemporaneous accounts, the more prominent of the pair. Translated into many languages and well known to earlier generations of scholars in social psychology and sociology of law, his work was forgotten by the early 1970s. However, there has recently been a resurgence of interest in Tarde's work: his ideas seem to fit our postmodern times. In Tarde's view, all human inventions arise in individual minds, and they are then imitated, binding individuals who imitate one another. Invention and imitation are thus the crucial movers of social life. Tarde used Leibniz's ideas to introduce a cosmology of monads equipped with desires and beliefs, which allowed him to explain individuality and sociality without added entities such as 'society'. He emphasized difference rather than identity and attributed a central role to the phenomenon of fashion and the processes of communication.

In Chapter 12, Alan Scott discusses the legacy of Georg Simmel. Despite the revival in Simmel's reputation in social theory, he does not enjoy the kind of influence on organization studies of his contemporary Max Weber. So far, Simmel's main presence has been via network sociology's analysis of brokerage and dyadic/triadic relations. Scott, however, argues that Simmel's central concern was freedom rather than advantage. He also argues that the anti-mechanistic and anti-rationalist principles underlying Simmel's social theory can be translated into a cultural approach to organization. The emphasis here is not on rational actions and plans, but upon the ways in which the basic principles that underlie a human community (e.g. an organization) unfold in ways that often subvert the actors' aims and, in the longer run, can undermine the community/organization. Scott shows how Simmel's analysis can help us understand why 'high commitment' organizations present a threat to the sociological conditions supporting a liberal society.

In Chapter 13, Rosabeth Moss Kanter and Rakesh Khurana extend the discussion of Simmel, arguing that Georg Simmel could provide more helpful guidance for understanding complex organizations in the new global information society of the future than better-known classic theorists who helped deconstruct the shift to an industrial era a century ago. The strength of Simmel's approach, the authors contend, is that it points to objective forms and structures that shape actions and outcomes independent of the particular personalities in a social situation, while also recognizing the subjective and emotional nature of social life. The size and complexity of social groupings make a meaningful difference in predictable and reliable ways. Kanter and Khurana examine the implications of size in creating differentiation inside organizations, and the implications for coordination, subordinate–superior relations, and solidarity. They argue, for example, that in large organizations, leadership can channel differentiation in productive ways by creating a

broader context in which individual interests are directed in ways that lead to both more scope for the development of individuality while accomplishing collectively meaningful goals. Finally, the authors highlight Simmel's prescient treatment of roles such as the 'stranger' and behaviors such as 'secrecy' and show how these concepts emerge as factors in the new twenty-first century information society. At a time when sociology is grasping for concepts and constructs that help make sense of our post-industrial economy, Kanter and Khurana suggest that Simmel offers a fruitful starting point, in both method and concepts.

Chapter 14 by Markus Becker and Thorbjørn Knudsen discusses Schumpeter and in particular the relevance of his early work on entrepreneurship. Schumpeter offered three major ideas on this topic. First, he defined entrepreneurship as creating new combinations of productive factors. This idea not only captures product or process innovation but also the way inputs, products, processes, and market choices are combined to produce an overall system, often referred to as an industry architecture. Elaborating on the principles governing the (in)stability in music, fashion, containers, and other industry architectures is a promising avenue for contemporary research. Second, he broadly identified behavioral and cognitive characteristics that stimulate individuals to engage in entrepreneurship: they can be inborn or culturally transmitted. This suggests that an organization can adjust its overall capacity for entrepreneurship by its selection policies, its internal structural arrangements, its training and socialization efforts, and its incentives structure. Third, he considered how organizations can stimulate entrepreneurship by structuring the interaction of individuals who have different motivations and triggers for entrepreneurial behavior. Most notably, entrepreneurship can be stimulated by direct interaction among kindred people, as often happens in the R & D department, or by delegating decision rights to lower levels in the organization. In an era such as ours in which entrepreneurship figures so largely in the discourse on growth, Schumpeter insights are precious indeed.

In Chapter 15, Ad van IJterson assesses the importance of Norbert Elias's theory of the civilizing process. Elias identified a long-term trend in West European societies towards a refinement of social behavior. In his magnum opus, *The Civilizing Process*, he analyzes the formation of the French absolutist state with its concomitant changes in social relations, conduct, and *habitus*. The key vector of change identified by Elias is the psychological internalization of the constraints that accompanied increasing social interdependence: a shift 'from external constraints to self-constraints'. When brought into the context of contemporary work organizations, Elias's approach is rich in implications for the behavioral and emotional aspects of trends towards empowerment, teleworking, 24-hour working day, despecialization, and multitasking.

The second group of chapters shifts from a European to North American frame. In Chapter 16, Gary Hamilton and Misha Petrovic discuss Thorstein Veblen. While Veblen is generally recognized today as a founder of and the main influence in

the Institutionalist school in American economics, and while his ideas continue to influence the fields of industrial organization and development studies, the reception of Veblen's work in mainstream economics has ranged from hostility to indifference. Hamilton and Petrovic claim that Veblen's work contains important elements for building an institutionalist, historically oriented theory of the contemporary global economy. They argue that Veblen's insights are even more relevant today than in his time, in particular as concerns: (1) the importance of analyzing firms as both producers of goods and services (industrial arts and craftsmanship) and market makers (business strategies and salesmanship), and the continuing organizational tension between these two types of activity; (2) the significance of consumer goods markets for driving contemporary capitalism, of the firms that make and organize those markets, and of the concomitant changes in consumption patterns; and (3) the need to revise economic and sociological theories of capitalism and business enterprise towards Veblen's developmental conception of cumulative causation, and away from approaches that rely on equilibrium or productionist assumptions.

In Chapter 17, Stella Nkomo argues while race has always been present in organizations, it has never been adequately theorized in organization studies. She reviews the classical works of W. E. B. DuBois on race, mostly ignored by his contemporaries at the time, to provide insights into the sociology of race that will assist organizational scholars in theorizing and interrogating race in organizations at a deeper level within the complex contours of today's global racial context. Du Bois's work while rooted in the momentous changes taking place within the post-Civil War Reconstruction period in the United States, evolved over several decades to illuminate race's inextricable relationship to the economic and social processes of global capitalism. Racially based social structures of inequality and exclusion persist today not only in the United States but globally despite the sentiment that we are now in a post-race era. Nkomo demonstrates how DuBois's conceptualization of race debunked essentialist approaches, instead stressing the importance of attending to the structural, political, and historical forces shaping any observed differences in the so-called races. The idea that race cannot be studied outside of the specific historical, geographical, economic, and cultural processes that constitute its meaning is a key idea in DuBois's work. Nkomo offers an in-depth discussion of DuBoisian concepts and explores their implications for the study of race in organizations.

In Chapter 18, Andrew Abbott discusses the research on organizations conducted by early Chicago sociologists. He argues that these studies make a powerful case against seeing the social world as a world of organizations, and for instead seeing organizations as an epiphenomenon of underlying social processes. Historically, organizations themselves appear as objects of sociological analysis only in a world that assumes the centrality of large, stable bureaucratic structures—a period that Abbott suggests starts about 1925 but ends around 1975. Since then, we have returned to an organizational world of rapid changes in organization boundaries,

and these boundaries become increasingly blurred and ambiguous. Our world is now, as it was in the early years of the twentieth century, a world of processes rather than structures. The Chicago School studies of that earlier period provide a starting point for grappling with the nature of this world.

In Chapter 19, Arne Carlsen discusses the legacy of William James, arguing that James's work represents a resource for a radically novel understanding of identity dynamics in organizations. Organizational identity theory has been predominantly focused on what James called the 'self-as-object', and Carlsen suggests that there is much to be gained from following James in a shift towards the agentic 'self-as-subject' conceived as a collective authoring process situated in ongoing experience. Following James's path has the advantage of connecting identity to the dynamics of practice. Locating practice as the site for authoring of identity helps us to see not only the habitual dimension of identity but also the role of jolts in experience and novelty as seeds of human growth. A turn towards agency allows us to see the forward-looking motives at play in identity construction, motives that form the basis for people's engagement in social change.

In Chapter 20, Michael Cohen discusses the contribution of John Dewey and the importance of his emphasis on the human faculties of habit and emotion. These concerns contrast with the emphasis in recent decades on cognitive processes. In contemporary organizational research there has been an increasing interest in recurring action patterns, such as routines and practices. The conceptual difficulties this work has encountered are usefully illuminated by Dewey's view of the primacy of habit and its interplay with emotion and cognition. It has all too easy for our theoretical discussions to fall into one or more of four traps: assuming that routines are rigid in their execution, that they are necessarily mundane in content, that they are typically isolated from thought and feeling, and/or that their underlying action patterns are explicitly stored somewhere. Dewey seems to have worked out in the early 1900s a philosophical position grounded in the primacy of learning and habit that (1) makes each one of these presumptions appear quite unnatural, and (2) suggests why in our own time we so frequently fall into them. In our own time many organizations find themselves centrally engaged with changing or improving systems of routine. To increase the agility of manufacturing processes or reduce the accidental death rate of hospital patients confronts organizations squarely with the properties of routinized activity and the dynamics of routines that facilitate and resist change. A clarified understanding of routine that makes use of Dewey's analysis and brings in habit and emotion along with decision making improves our ability to make the kinds of improvements that are so vital.

In Chapter 21, Chris Ansell discusses the work of Mary Parker Follett. An early twentieth-century management theorist, social worker, and political scientist, Follett's ideas about power and authority have been widely influential in organization studies. Yet despite her reputation as a 'prophetic' management theorist, the wider significance of her work is often underappreciated. This chapter argues

that one way to gain a greater appreciation for Follett's work and its contemporary significance is to analyze its ontological commitments. Ansell points out that Follett was perhaps the most philosophical of our classical organization theorists and she translated this *Weltanschauung* into a systemic approach to organization. Although strongly influenced by German and British idealism, Follett's most significant works sought to reconcile this idealism with American pragmatism. In fact, her work on organization, education, and democracy is often mentioned in conjunction with the work of her contemporary, the pragmatist philosopher John Dewey. Acknowledging the centrality of power and conflict in organizations, Follett used her idealist pragmatism to explore possibilities for fruitful social cooperation. The central concept of her work was integration, which she famously contrasted with compromise. Whether analyzing conflict between management and labor or the power of a supervisor over a worker, Follett believed that fruitful social cooperation required a creative integration of different perspectives and interests. In exploring the implications of integration for conflict, control, coordination, communication, and command, Follett's idealist pragmatism created a systematic theoretical framework for understanding non-hierarchical organization.

In Chapter 22, Tim Hallett, David Shulman, and Gary Alan Fine examine classical symbolic interactionist thinkers and their relevance for contemporary organizational studies. They assess founding figures, such as George Herbert Mead, the mid-century contributions of Herbert Blumer and Everett Hughes, and conclude with the later contributions of Erving Goffman, Anselm Strauss, and Howard Becker. The interactionist credo emphasizes that organizations are comprised of people, and that their interpretations of work activities matter because it is based on those meanings that people act and pursue organizational goals. Hallett, Shulman, and Fine argue that the 'peopled' approach of classic interactionism provides an important contrast to the 'metaphysical pathos' that has plagued some strands of organizational studies. Instead of emphasizing disembodied forces, the authors use classic interactionist work to stress that organizations and institutions are inhabited by people doing things together, and these doings suffuse organizations with meaning and significance.

In Chapter 23, Andrew Van de Ven and Arik Lifschitz review the seminal work of John R. Commons, a founder of institutional economics and industrial relations. They identify four main features of his work of relevance to contemporary organization studies. First, Commons introduced a novel and pragmatic theory of institutional design and change that anticipated by more than a generation the relationships between action and structure at both individual and collective levels. Second, he viewed institutional change as a social movement. His history of labor unionization and monopoly busting showed how institutional rules are created to address disputes and injustices among conflicting parties with unequal power and diverse interests. Third, Commons replaced natural selection with artificial, purposeful selection, providing us with a powerful reason why we should turn

organizational ecology on its head. Finally, Commons introduced the collective standard of prudent reasonable behavior, which is a major alternative theory of valuation to that based on individual rational self-interests as well as random environmental events. Commons emphasized that solutions to conflicts among parties cannot be based on individual standards of rational self-interest for that would produce unjust solutions favoring the more powerful parties.

In Chapter 24, Elisabeth Clemens discusses an interesting anomaly: the absence of a classic response to the emergence of the large corporation. The rise of the large corporation fundamentally challenged the foundations of liberalism, with its commitment to a world of rights-bearing individuals embedded in a market society of small enterprises. Yet, despite this opportunity to address a major social change, some of the most important works of political economy of this period—notably Karl Polanyi's *The Great Transformation* and Friedrich Hayek's *The Road to Serfdom*—did not see the corporation. Other legal theorists and commentators did recognize the novelty of the large firm, but then sought to reassure readers that these potentially threatening developments would be counterbalanced by the moral qualities of business leaders, the decentralization of power within the firm, and the application of due process to employment relationships. Thus, the tension between the modern corporation and a political theory premised on the rights of natural individuals was not fully explored. The problem of the large organization and liberalism was left as a classic waiting to happen.

In Chapter 25, Mike Reed discusses post-World War II bureaucratic theory as developed by Selznick, Gouldner, Blau, and Crozier. Reed revisits these modern classics in order to rediscover the strategic sociological, political, and ethical issues that framed the socio-historical context in which they emerged. He identifies the cycle of 'imaginative reformulations' that these modern classics have undergone over recent decades. His main thesis is that these works still inspire new insights into recurring themes or dilemmas such as 'agency/structure', 'power/control', and 'statics/dynamics'. He also identifies the theme of changing and contested forms of organizational governance and control as an issue that would come to dominate public, as well as academic, debate in late twentieth and early twenty-first-century advanced capitalist societies.

In Chapter 26, Heather Haveman examines the research on organizations conducted at Columbia University under the guidance of Robert K. Merton. The work of Merton's students was rooted in Weberian ideas about bureaucracy but moved in directions that Weber might not have expected. Rather than focusing on the technical rationality inherent in bureaucracy as celebrated by Weber, these researchers studied the unanticipated consequences of organizational design; in particular, the dysfunctions of bureaucracy that arise from goal displacement. They highlighted conflicts that ensued both within organizations and between organizations and their surroundings. They saw organizations as the crucible of institutionalization: organizations became valued in and of themselves, far beyond

the technical merits of the things they do. A close reading of three pieces in this tradition—Merton’s essay on bureaucratic dysfunctions, Alvin Gouldner’s *Patterns of Industrial Bureaucracy*, and Peter Blau’s *Dynamics of Bureaucracy*—reveals many insights that can benefit organizational scholars today, notably the reminder that although organizations may be designed as tools, they inevitably take on lives of their own.

In Chapter 27, Charles Heckscher argues that Talcott Parsons’s action paradigm remains the most successful analytic framework for understanding fundamental sociological concepts of trust and commitment within a voluntarist perspective. Parsons specifies the various orientations that are needed to sustain successful social systems, especially ones that are highly complex and differentiated and allow individual choice. The chapter tries to show the continuing utility of the model by applying it to the development of complex relations of influence in knowledge-based business firms, which have increasingly moved beyond bureaucratic orientations describe by Weber to more complex collaborative norms. A central example is the problem of articulation of teams based on collegial influence with the hierarchical structures of power. The Parsonian framework enables us to identify systematically a series of problems posed by this development and to understand some of the constraints that shape potential solutions.

In their Afterword, Gerald Davis and Mayer Zald—taking their cue from Stinchcombe’s piece on the functions of classics in sociology—comment on the functions of a book *about* the classics of sociology and what scholars can hope to take away from this volume. The canon of ‘classics’ is contested terrain in sociology and other disciplines, with certain authors (e.g. Marx, Parsons) being included or excluded according to the intellectual, social, and political environment and the approved forms of rhetoric holding sway at the time. For example, Tarde disappeared for many years then reappeared with shifts in interests and tastes; Simmel found a rebirth thanks to the prevalence of network analysis. Davis and Zald further argue that the classics considered in this volume offer particular relevance to contemporary scholars seeking guidance on how to theorize large-scale economic transformation. Many of the authors considered here grappled with the birth of a ‘society of organizations’ and thus can help our own efforts to understand new forms of globalized post-industrial capitalism.

## WHY THESE CLASSICS?

Our selection of classics has been guided above all by the contributors’ sense of whom our field might learn most from today. However, as Davis and Zald remind

us, any selection of a group of writers as ‘classics’ is a gesture fraught with symbolic and political weight. Readers of this volume may contest the inclusion of some writers whose contributions to the future of the field seem too slight and whose presence perhaps reflects idiosyncratic, personal, or passing enthusiasms. Conversely, there are no doubt writers of great value whom we have excluded. There are clearly writers—Smith, Spencer, Sorokin, and Schutz, just to focus on four alphabetically related names—who warrant attention but who are not discussed here for merely practical reasons. The small number of women and non-European/American writers reflects in part their exclusion from the field in the past but perhaps also reflects prejudices that still mask their potential contribution. Given these risks, we hesitated to use the term ‘classics’ in the title of the volume: our intention is not to create a canon, merely to prompt new readings. However, if the label ‘classics’ prompts such criticisms, we are delighted to have generated such debates.

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