

History in Organization and Management Theory: More Than Meets the Eye

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Abstract

There has been a growing debate about the role of history in management research with several authors making suggestions on how to bring the two (back) together and others even highlighting the need for a “historic turn”. What we argue in this paper is that, while history was indeed sidelined by the scientization of management since the late 1950s, it started to make a comeback from the 1980s onwards and is increasingly employed in a number of research programs. We stress that the crucial question for management scholars engaging with history (or wanting to do so) is how it relates to theory. First of all, we present a systematic overview of the way history has been used—both at the micro (organizational) and macro-levels of analysis—distinguishing between what we refer to as “history *to* theory” and “history *in* theory”. In the former, we consider those research programs,

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such as (neo-)institutionalism, where history serves as evidence to develop, modify or test theories. In the case of “history *in* theory” we identify research programs where history or the past are part of the theoretical model itself as a driver or moderator, with “imprinting” as a prime example. Second, we also identify a growing number of studies that go further by displaying what we call “historical cognizance” in the sense of incorporating period effects or historical contingencies into their theorizing efforts. Finally, drawing on our broad overview, we make more specific suggestions for increasing the visibility and influence of history in organization and management theory.

Introduction

Since the 1990s, there have been increasing calls suggesting to bring (business) history and organization and management theory (back) together. These calls were issued initially by a number of management scholars, notably Zald (1990, 1993) and Kieser (1994)—with the suggestions by Marquis and Tilcsik (2013, p. 230) in this journal for “taking history more seriously” providing a more recent example. Also more recently, (business) historians have joined the fray, debating a closer relationship with management studies among themselves (e.g. Godelier, 2009; Kipping & Üsdiken, 2008; Popp, 2009), and also appealing for more respect and consideration within management scholarship and discussing ways to achieve that (e.g. Decker, 2013; Kipping, Wadhvani, & Bucheli, 2014; Wadhvani & Bucheli, 2014). Beyond these calls, there has also been some action in the form of special issues or edited collections with contributions that examine various management sub-fields from a historical perspective (Kahl, Silverman, & Cusumano, 2012a; O’Sullivan & Graham, 2010; Üsdiken & Kieser, 2004; Üsdiken, Kipping, & Engwall, 2011; Van Baalen & Bogenrieder, 2009; Weatherbee, Durepos, Mills, & Mills, 2012). What these various contributions have done is to

- (1) examine the reasons for the apparent gap between historical scholarship and research in organization and management theory (as well as social science more generally)—a gap that, as we have pointed out elsewhere (Kipping & Üsdiken, 2008; Üsdiken & Kipping, 2014), was not there when these disciplines were established in the early twentieth century but arose and gradually widened from the 1950s onwards when “business administration” turned itself into “management” and emulated the hypothesis-testing natural science model as a way to increase its legitimacy both within academia and the broader business community (see also Augier & March, 2011; Khurana, 2007). Business historians noticed and debated these developments in organization and management studies, but while some argued in favor of espousing a more scientific approach

- (Redlich, Glover, Johnson, Taylor, & Overton, 1962) and others made attempts to provide both more systematic data and generalizations (e.g. Chandler, 1962), the majority did not follow the hypothesis-testing path.
- (2) argue strongly in favor of a closer relationship—a reversal of sorts—to a large extent due to a growing dissatisfaction with the science paradigm, seen as overly dominant, even stifling, at the methodological, epistemological, and ontological levels. This prompted Zald (1993, p. 514), for instance, to suggest that “organizational studies needs to be reconceptualized as a humanistic as well as a scientific area of study”. Focusing more specifically on history, Kieser (1994) proposed to draw on the Weberian tradition of using historical cases as a way to generate new or alternative hypotheses or develop and apply “ideal types”. Going even further, a number of European-based management scholars have argued that organization studies as a whole need a “historic turn” (Clark & Rowlinson, 2004; Rowlinson & Hassard, 2014). Drawing in part on developments in the broader history discipline (see, e.g. Fridenson, 2008; Lipartito, 1995) they suggest a post-modern, narrative approach. Last but not least, there is another—also mainly European-based—set of scholars who derive insights and conceptual frameworks, such as “genealogy”, from Foucault’s historical studies (e.g. Carter, 2013; McKinlay, 2013; McKinlay & Starkey, 1998).
 - (3) make a range of more specific suggestions as to how history can be incorporated into the study of organizations and management, which usually include some form of inter-disciplinary or trans-disciplinary approaches (for the latter and its difference from the former, see Leblebici, 2014). They also tend to include calls for more openness and disclosure, notably in terms of the methodology used to analyze historical sources (Kipping et al., 2014; Rowlinson, Hassard, & Decker, 2014) and the ontological assumptions underlying both the historical studies themselves and the way they are used in organization and management theory (Coraiola, Foster, & Suddaby, *in press*). Nevertheless, and this needs to be stressed here, in-depth elaborations of historical approaches and methodology remain marginal within the core of organization and management theory (see Kipping et al., 2014). There are a few notable exceptions, namely Stinchcombe (2005), who includes historical methods among “the four main methods of addressing causal questions in social science”, in addition to quantitative, ethnographic, and experimental ones (see also Suddaby & Greenwood, 2009).

Among these intense debates and suggestions, what is still missing is a systematic and comprehensive overview of how history—broadly understood as an empirical and/or theoretical concern with and/or use of the past—has actually been employed in organization and management theory. There are a few earlier surveys, which we will discuss in the subsequent section, but, as we will show,

they remain rather limited, namely in terms of their temporal scope, focusing on recent work only, and of the type of publications considered, including only top, usually U.S.-based journals. Our paper therefore aims to provide a more comprehensive overview—without claiming it to be entirely exhaustive. What we will show is that, while there has been little explicit reference to history or use of historical research in organization and management theory—except for a few, relatively isolated pockets of interest (Üsdiken & Kipping, 2014), history has actually featured in a broad range of research programs, some of them preceding the recent calls for a historic turn. Thus, history has been used as empirical evidence (something we refer to as history *to* theory) and/or as part of theoretical models (which we call history *in* theory). Our second, more forward-looking finding is that, while much of this work remains within the dominant science paradigm, there are also an emergent and growing number of publications displaying what we call “historical cognizance” by acknowledging historical conditionality for their theorizing or by formulating their hypotheses in a context-specific manner.

In what follows, we will first present our approach toward structuring the extant historical approaches in organization and management theory—not an easy task, given their diverse and, at times, latent nature. The main part of the paper then presents the results of this systematic survey, structured using a combination of empirical and theoretical reasoning. The final part of the paper will briefly summarize the main insights from the comprehensive overview and, more importantly, draw out a number of specific suggestions as to how history can continue to make a valuable and potentially even broader contribution to organization and management theory.

Our Approach: History *to* Theory, History *in* Theory

Ours is not the first attempt to survey historical research and publications within organization and management theory. Among the earliest reviews was the introduction by Üsdiken and Kieser (2004) to their special issue of *Business History*, where they suggested distinguishing three uses of history. They labeled them “supplementarist”, “integrationist”, and “reorientationist”, denoting the different ways, in which history was envisaged to engage organization and management studies. Thus, the “supplementarist” position referred to approaches, which viewed history as possibly making substantive and/or methodological contributions to the theory-driven, social scientific aspirations of organizational analysis. The “integrationist” stance argued for identifying the domains, where history and historical research had a central place and could be combined with theorizing on organizations and management. The “reorientationists” tended to view a “historic turn” as one of the ways to distance organization and management studies from its earlier trajectory built on the natural science model. While this classification was useful in

capturing the diversity inherent in the growing interest in bringing history (back) into organizational analysis, Üsdiken and Kieser (2004) could only point to a limited number of exemplary studies and commentaries that represented the state of the art more than a decade ago.

Subsequently, there have been similar special issues of other journals. Their introductions did not provide structured overviews of the literature though; they rather offered more normative suggestions of how to bring about a closer interaction and engagement between (business) history and management studies (see, in particular, O'Sullivan & Graham, 2010, who cite various advocates from both sides; also Booth & Rowlinson, 2006, who lay out a 10-point agenda for the future direction of management and organizational history in the inaugural issue of the eponymous journal). And in their introduction to the recent volume on *History and Strategy*, Kahl, Silverman, and Cusumano (2012b) also aim "to postulate how history and strategy research can inform each other" (p. ix), while including a short list of exemplary historical studies on various "prevalent strategy topics" with Chandler (1962)—not surprisingly—occupying the most prominent position (p. xi).

There are, however, two recent more systematic and comprehensive reviews, one by Rowlinson and Hassard (2013), the other by Leblebici (2014). Both provide lists of publications that they identify as "historical" from a small number of "top" management journals, the *Academy of Management Journal*, *Administrative Science Quarterly*, and *Organization Science*, and, for Leblebici (2014) only, *Organization Studies*. The former used a simple word search (for the term "histor*" in the title or topic), which yielded 92 actual articles for the period 1991–2010, from which they removed 34 since they were judged to make only passing references to history (Rowlinson & Hassard, 2013, p. 118). Leblebici (2014) appears less strict in his selection criteria, which are not explicitly delineated, and therefore finds a total of 102 articles "based on historical data, analyses, or narrative" over half of the period (2000–2010), albeit with one additional journal. He highlights an avoidance of the term "history" in many of these papers—indicating an overwhelming, if not exclusive concern with theoretically motivated and conceptual rather than "traditional historical" questions (pp. 60–61). He also identified dissertations completed during the same time period in U.S. business schools "using historical data or analyses", but yet again concluded that, out of the 100 he found, most used traditional panel or time-series data and very few incorporated historical methods or primary archival sources (pp. 58–59).

But the main purpose of both articles is to discuss the distinctive features of what Rowlinson and Hassard (2013) refer to, respectively, as "historical neo-institutionalism" and "neo-institutionalist history" and Leblebici (2014), once again more broadly based, calls "organizational theory" and "business history". Both articles highlight significant differences between these approaches, with the former pinpointing the methods (more formal for

historical neo-institutionalism) and, relatedly, the sources used (published vs. primary), and the latter focusing on the different types of “understanding” and notably the way narratives and cases are used as “an application of a specific theory” by organizational theory scholars and “to identify unique causes of events or historical processes” by business historians (Leblebici, 2014, p. 74). He nevertheless expresses some hope for both disciplines to collaborate on their own terms without fudging or eliminating these differences but instead “appreciating the richness each side contributes to scholarly research” (p. 76; see also Coraiola et al., *in press*; Greenwood & Bernardi, 2014)—an issue and a suggestion, to which we will return in the concluding section of this paper.

In terms of approach, we concur with Leblebici (2014, p. 61) in that “traditional database searches (e.g. ISI) fail to detect all the papers that have historical dimensions”—something we also found in our own attempts to conduct such searches. We therefore took a rather pragmatic approach drawing on (a) the knowledge/expertise of the authors, which was quite complementary; (b) recent thematically relevant special issues or edited collections (see above) and (c) snowballing techniques, i.e. following up on references in already identified publications. This allowed us to extend the scope of the extant surveys in three directions: (i) disciplinary: adding notably the relevant strategy literature; (ii) geographic: balancing North American with some more European publications; and (iii) temporal: stretching further back than the 1990s. In terms of the actual publications, we also included books, absent from any of the previous surveys, despite their well-recognized lasting influence on research in organization and management theory (Eisenhardt, 1989). As an illustration, take Chandler’s (1962) book on *Strategy and Structure*, which examined the origins of the multidivisional or M-form of organization, combining a survey with four in-depth case studies, and continues to be held up as a leading example for comparative, case-based research (Eisenhardt, 1989). The book (a) prompted a series of major empirical research projects, which examined the development of strategies and structures over time and space (see Whittington & Mayer, 2000, p. 12); (b) sparked an ongoing debate about the efficiency implications of the M-form adoption (for a recent overview, see Kipping & Westerhuis, 2012); and (c) became foundational for a variety of—sometimes contradictory—theoretical approaches such as structural contingency and transaction cost analyses (see Üsdiken & Kipping, 2014).

Most importantly, unlike the earlier survey publications and the aforementioned calls for a “historical turn”, we were *not* concerned with comparing (business) history and organization and management theory—and bridging the gap between them. Taking a broader perspective, our aim was to uncover and systematically review the ways in which history featured in organization and management theorizing, irrespective of the specific methodologies employed. While the articles mentioned above had referred to different

research programs such as (organizational) ecology or neo-institutionalism, they either focused on one of them (e.g. neo-institutionalism for Coraiola et al., *in press*) or lumped them together as a single “social science” research paradigm (see above). What we did instead was to identify the differences between these research programs in the way they referred to and/or used history (explicitly or latently) and tried to cluster those we found to be making similar uses. What appeared as the main, and—given the need for theorizing in organization and management studies—ultimately not all that surprising dimension in our clustering exercise, is the relationship between history and theory, where we distinguish two broad approaches:

- (1) The use of historical data (both quantitative and qualitative) to develop new or modify or test extant theories. The point here is that these theories themselves remain timeless and general, but that the historical data are somehow well suited to contribute to theory building or testing—or might just reflect a lack of suitable cross-sectional data. We refer to this approach as “history *to* theory”.
- (2) The use of the past as an integral part of the theoretical model itself, such as in imprinting or path dependence, which we refer to as “history *in* theory”. Again, while history is included in the theory as a driver (or moderator), the theory itself is generally meant to be universally applicable—regardless of context.

These two approaches can be seen as located on a horizontal axis. In addition, we put the level of analysis on a vertical axis, using the distinction made in organization and management theory between “macro” and “micro” perspectives (e.g. Astley & Van de Ven, 1983; McKinley & Mone, 2003) or between studies at the “ecological” and “organizational” levels (Scott & Davis, 2007). Macro approaches refer to theorizing and research that examines aggregates of organizations at the level of populations, fields, or communities. The micro level relates to studying the processes of or the features within individual organizations and/or

Table 1 Categorizing History in Organization and Management Theory

	History <i>to</i> theory	History <i>in</i> theory
Macro/ ecological	Longitudinal/time-series data to test, modify, develop theories about, populations, fields, communities	Past conditions as drivers for present patterns in aggregates of organizations, e.g. industries, populations
Micro/ organizational	Historical data used to examine and theorize about organizational features and/or processes	The past as a determinant or moderator for subsequent/current behavior of organizations

their interaction with the external environment—but not behavior *in* organizations, as the term “micro” is often taken to mean. This ultimately results in a two-by-two matrix presented in [Table 1](#).

What needs to be stressed here is that we conducted this initial categorization not at the level of individual articles or papers, but at the level of the various research programs or streams. Put differently, we consider that a research program belongs to a certain category, i.e. a box in the matrix, if *most* of the studies conducted in that area display the characteristics outlined above. This does not preclude certain articles from that research program displaying somewhat different characteristics. For instance, while some process studies are “historical” in the sense of using data from the past, there are many others that rely exclusively on contemporary data, e.g. through participant observation.

What also needs to be noted is the possibility that a research program or stream includes studies that belong to more than one category/box. A case in point is organizational ecology. A large portion of ecological research can be considered “history *to* theory” since it has involved inferring general theories by studying the histories of organizational populations over long periods of time. On the other hand, ecological studies examining founding conditions or organizational processes seem to fit better into “history *in* theory”, as they consider how the past of a population or organization shapes future outcomes. Likewise, in terms of the levels of analysis research streams based on the idea of imprinting have considered the effects of founding conditions at both macro and micro (organizational) levels. In the following, we therefore consider such cases within multiple categories.

While we believe the resulting overview to provide an accurate and comprehensive reflection of the current state of history in organization and management theory, we also aim to lay out a more dynamic, forward-looking view by paying attention to specific publications, in particular recent ones. In doing so, what we found was an emergent and growing third approach, situated between “history *to*” and “*in* theory”, which we suggest calling “historical cognizance”. While being originally grounded in one or the other of the two basic approaches, these publications are “taking history more seriously”. This means that authors were conscious that their results (in the “history *to* theory” approach) or their theoretical models (in the “history *in* theory” approach) were influenced, even determined by the historical context and its idiosyncrasies. Put differently, they were aware of and explicitly considered the limits to generalizability resulting from the use of history, which is why we refer to them as having “historical cognizance”. To be included in this category, publications coming from history *to* theory, need to use past data not just to test or develop *universal* theory, but make that theory more *contingent* on the changing context, while those from history *in* theory need to see history

as a driver in a more *nuanced* manner, paying attention to the kind of past, the specific context at a certain time and how they might have influenced subsequent developments.

In what follows, we will present the research programs belonging, respectively, to history *to* theory and history *in* theory, subdivided into macro- and micro-level approaches, illustrating each of them with a number of exemplary studies. We then discuss publications that we consider to belong to the “historical cognizance” category—with this “third way” also of considerable importance for our overall conclusions and suggestions, since it opens novel—and potentially very promising and far-reaching—avenues for incorporating history more deeply into organization and management theory.

History to Theory: Testing, Modifying, and Developing Theory

There has been a long-standing relationship between history and the social sciences—exemplified, for instance, as Kieser (1994) points out, by the fact that Max Weber was professor of both, sociology and history (see also Adler, 2009). This is also true more specifically for what was at the time, i.e. in the first half of the twentieth century, called “business administration” (see Üsdiken & Kipping, 2014). One could argue that history was also present and important at the origins of modern-day organization and management theory and of strategy: the former through its role in what is now called “old” institutionalism—often associated with the work of Selznick (1957)—and the latter with, among others, the early research by Chandler (1962), who was a sociologically influenced historian (see, respectively, Djelic, 2010; Farjoun, 2002b). But during that same period, as noted above, the growing scientization of what was now called “management” increasingly marginalized historically based research—a marginalization that is exemplified by two quotes: One from 1952, when Herbert Simon upheld the utility of “historical data appealed to by the Weberians” but suggested they “*need supplementation* by analysis of contemporary societies, advanced and primitive”; the other from 1960, when the author of a methodological research note in the *Administrative Science Quarterly* attributed only “second-level priority” to historical research compared to the study of “current and immediately observable organizations *in the interests of full and rigorous data*” and also forcefully argued for the need to employ the “*usual scientific canons* of validity, reliability, generality, parsimony, explanatory power and usefulness” (quoted, respectively, in Kipping & Üsdiken, 2008, p. 100; Üsdiken & Kipping, 2014, p. 37; emphasis added).

Thus, from the late 1950s onwards organization and management studies increasingly used contemporary, cross-sectional data and applied so-called “variance” approaches (Mohr, 1982) that aimed at explaining organizational phenomena in terms of relationships between independent and dependent variables (Üsdiken & Kipping, 2014; Ventresca & Mohr, 2002). But from the

1980s onwards, new research questions and, ultimately, research programs arose that drew—at least partially—on history “as an empirical laboratory to test their specific theories” (Leblebici, 2014, p. 69). These programs, which we regroup under the notion of “history to theory”, emerged both at the macro- and micro-levels of analysis. For the former, there was a growing interest in studies of populations and fields, where the phenomena of interest required research covering a long-term horizon or where longitudinal data provided more variation than cross-sectional ones. Most of the studies in what came to be known as ecological and institutional research relied on time-series data (e.g. Hirsch & Gillespie, 2001; Isaac & Griffin, 1989; King & Haveman, 2008; Rao & Dutta, 2012), while some others constructed historical case studies or narratives (e.g. Hargadon & Douglas, 2001)—often based on published primary or secondary sources (Rowlinson et al., 2014). At the micro/organizational level, the departure from variance approaches was driven by those seeking explanations for change phenomena, including the role of leadership, and strategy-making, in the temporal sequencing of actions and events (e.g. Pettigrew, 1990; Van de Ven & Huber, 1990). While the use of historical data seemed to be the logical way for what came to be known as process theorizing, much of the actual research ended up examining unfolding events in real time, using ethnographic methods based mainly on interviews and (participant) observation (see e.g. Langley, 1999). In the following, we discuss each of these research programs and their use of history in some more detail, based on a number of exemplary studies.

History to Theory—Organizational Populations and Fields

Organizational ecology. As pointed out above, organizational ecology has been a forerunner in expanding the historical scope of organization theory by studying the entire histories of populations and of the organizations that constitute them. As Hannan and Freeman (1989, p. 10) have put it in the early stages in the development of the ecological approach, “(r)esearch at the population level leads naturally to a concern with history because the study of population dynamics frequently requires analysis over long periods of time”. Yet, in this engagement with history, the ultimate aim of the research program has been to develop general theories, as the leading proponents made very explicit: “We are interested in developing and testing general arguments, ones that *apply to all kinds of populations in all kinds of contexts*” (Carroll & Hannan, 1989a, p. 546; emphasis added). Or, as Hannan and Freeman (1989, p. 19), have phrased it, “organizational change has a timeless, ahistorical quality”. This approach to population-level phenomena therefore makes the ecological perspective a prime example of what we consider “history to theory”.

The study of organizational evolution in this manner has led to well-established general formulations, such as the density dependence model, where the

main concern has been to account for rates of founding and mortality in organizational populations. In a nutshell, the density dependence model proposes that initial increases in density in the early stages of a population—and the accompanying increase in legitimacy—increased founding and reduce mortality rates; but, as the population grows further, higher levels of density result in greater competition, which in turn lowers rates of founding and increases rates of mortality (see Baum & Shipilov, 2006; Carroll & Hannan, 2000; Hannan & Carroll, 1992 for reviews and exemplary studies). In assessments of these population-level processes, ecologists have also considered environmental effects such as resource availability, technological innovation, and political change (e.g. Carroll & Hannan, 2000).

Yet, these external processes have usually been framed in general theoretical terms and incorporated into empirical analyses as time-varying conditions or in the form of period effects (e.g. Carroll, Feng, Le Mens, & McKendrick, 2009). And even in cases, where periods have been identified in historically specific ways, they have been incorporated as additional influences or control variables for a more robust specification of empirical models (Isaac & Griffin, 1989). Dobrev (2001), for example, in studying the rates of founding of Bulgarian newspapers in 1846–1992 identified three distinct historical periods: pre-socialist (1846–1948), socialist (1949–1989), and post-socialist (after 1990). Such a periodization, Dobrev (2001, p. 423) argued, addressed the oft-levied criticism of “ahistoricism and contextual imprecision” in ecological research. The main point was that this was appropriate for “cases where tumultuous environments have interfered with the *natural* organizational processes” (emphasis added). That the density dependence model did not hold in the socialist period thus received only passing mention. Also, the separation between the pre- and post-socialist periods served as a basis for examining how the time frame of legitimation and competitive processes varied with the extent of political activity and the emergence of a new organizational form in the pre-socialist era as opposed to its revival in the post-socialist period.

Given their strong preference for quantitative research and, more recently, formalization (e.g. Hannan, Pólos, & Carroll, 2007), ecologists have rarely turned toward historical cases to develop theory. An early exception is Langton (1984), who employed ecological ideas in attempting to account for bureaucratization in Josiah Wedgwood’s British pottery firm in the late eighteenth century and to examine its spread to the whole pottery industry during the industrial revolution. More recently and more germane to the central concerns of the ecologists, McKendrick and Carroll (2001) studied disk-array producers in the U.S.A. as a historical case to extend theorizing on the emergence of organizational forms. Following the turn in the ecology literature toward defining organizational form as “externally-enforced identity” (Carroll & Hannan, 2000, p. 68), they drew upon their case evidence to assess extant

institutional and ecological accounts, arguing that, somewhat different from predictions based on these theories, a distinct form could not emerge because the origins of extant firms were in other industries and there were only few companies with a focused identity as disk-array producers.

Institutional theory. This is a very broad research program with many variants, which has been extending into many areas of research (see Greenwood, Oliver, Sahlin, & Suddaby, 2008). At the outset of its neo-institutional version, which is currently the most influential in organization and management studies, history seems to have had little room, since researchers were mainly concerned with future convergence and diffusion processes within organizational fields (Hirsch & Lounsbury, 1997; cf. Djelic, 2010, who points to earlier more history friendly institutional approaches). Research of this kind has been mainly characterized by testing theory through quantitative analyses and a predominant focus on the effects of contemporaneous institutional environments on organizations (see Heugens & Lander, 2009 for a review). But when interest shifted to institutional formation and change, historical data and historical cases became more central, often used to develop or modify rather than testing theory, typically at the field level. Research of this nature has relied very much upon published primary and/or secondary sources, though some studies have also used retrospective interviews in constructing their historical accounts. On the other hand, research on diffusion, for example, has relied extensively on quantitative methods. There have also been studies, more so recently, which have moved toward combining historical evidence with some form of quantification and hypothesis testing (see Schneiberg & Clemens, 2006).

DiMaggio's (1991) study on art museums in the U.S.A. and Brint and Karabel's (1991) work on American community colleges provide early examples of historical studies—both pointing to the lack of attention in neo-institutional research to issues of change and contestation within organizational fields. Based on primary historical evidence, DiMaggio (1991) showed how a new model of the art museum emerged in the U.S.A. as an alternative to the one that had been established by the 1920s. He also demonstrated how this “reform” was promulgated through a professionalization project and the struggles that it generated at the field level. Brint and Karabel (1991) examined the transformation of community colleges in the U.S.A. toward a vocational orientation in the 1960s and the 1970s. Theoretically, these authors were drawing upon “old” institutionalism, which, as a whole, had exhibited more interest in and use of history than the neo-institutional version did at the outset (see above). What primarily distinguished Brint and Karabel's (1991, p. 355) historical analysis was the view they took of organizational fields as “arenas of power relations”. Thus, not only did they make an early call for greater attention to studying the development of organizational forms and

the processes of institutional change within the neo-institutional project, but also highlighted the value of taking a historical approach in doing so.

Another oft-cited study drawing upon historical research to advance neo-institutional theory has been Leblebici, Salancik, Copay, and King's (1991) work on the U.S. radio broadcasting industry in the period 1920–1965. The central concern of this study was institutional change within an organizational field. Starting with identifying the main coordination mechanisms and focusing on conventions, their historical study showed that institutional change was triggered by the marginal players in the field. Another example, concerned with the difficulties of neo-institutional theory in accounting for organizational change, is Holm's (1995) study of the development and decline of the "mandated sales organization" in the Norwegian fishery sector over the period 1930–1994. And in revisiting Leblebici et al.'s (1991) proposition that change within organizational fields emanates from peripheral actors, Greenwood and Suddaby (2006) studied the emergence of a new organizational form within the business services field in Canada—the multidisciplinary practice. Providing an example for the combination of historical research with other qualitative methods (such as interviews and content analysis), they showed that, in this case, the new organizational form was initiated by actors at the very center of the field.

Greenwood and Suddaby's (2006) case analysis provides a perfect example of what we consider as "history to theory" in that they were able to derive specific theoretical propositions with respect to the conditions under which central organizations in mature organizational fields are likely to initiate and achieve institutional change relative to the more marginal actors. Wright and Zammuto (2013) recently addressed the same issue in the context of County Cricket in England over the period 1919–1967. Based on the archives of a London-based cricket club that "governed English cricket" as well as published primary and secondary sources, they construct what they refer to as a "process model" of institutional change within, again, a mature field (p. 311), demonstrating the role of "middle-status" actors in between those at the center and the periphery in processes of institutional change (p. 322).

Likewise, Hargadon and Douglas (2001) examined the introduction of Edison's electric lighting system as a case of institutional entrepreneurship and, eventually, of institutional change. They developed two central arguments from studying the period between Edison's announcement of his discovery in 1878 and 1892, when electric lighting displaced gas lighting in New York: (i) that innovations are more likely to alter or replace established institutions when they also contain elements of pre-existing meanings and rationales; and (ii) that the design of innovations and the concrete elements they embody serve to reconcile between the old and the new. David, Sine, and Hageman's (2013) article on the development of the "professional management consulting firm" in the U.S.A. provides yet another case of studying institutional

entrepreneurship based on historical data. Indeed, a historical orientation has characterized research on institutional entrepreneurship that Hardy and Maguire (2008, p. 199) identified as “process-centric”—as opposed to “actor-centric”. These field-level process approaches are exemplified by Lounsbury and Crumley’s (2007) study of the creation of “active money management” practice in the U.S.A. and Maguire, Hardy, and Lawrence’s (2004) research on the emergence of “consultation and information exchange” practices in advocacy on HIV/AIDS treatment in Canada. Both of these studies have relied—to different degrees—on historical data as well as interviews, with the former, in particular, drawing upon primary sources such as Congressional hearings.

There has also been recourse to history in developing theory on the emergence of and changes in institutional logics as well as their persistence. An example is the study by Rao, Monin, and Durand (2003) on the replacement of the classical French cuisine by the institutional logics and role identities of *nouvelle cuisine*. Based on secondary sources, they first trace the development and institutionalization of classical cuisine from the aftermath of the French Revolution to the 1960s and use interviews to examine the subsequent change, ultimately theorizing on the role of identity movements in such institutional change and identifying the mechanisms that prompted actors to shift from an old logic to a new one. Other examples include the work by Sine and David (2003) on the effects of external environmental jolts on alterations in institutional logics, based on the electric power industry in the U.S.A. between the mid-1930s and the late 1970s; the study by Dunn and Jones (2010) on multiple (science and care) logics in American medical education, which combines a narrative, historical analysis with hypothesis testing; and, showing a similar long-term persistence of multiple, often contested logics within a field, the work by Marquis and Lounsbury’s (2007) on the present-day consequences of the long-standing opposition between community and national logics in U.S. banking, and by Lounsbury (2007) on the “trustee” and “performance” logics associated with Boston and New York, respectively (see also Greenwood, Raynard, Kodeih, Micelotta, & Lounsbury, 2011).

Neo-institutional theorizing has also strongly influenced research on the diffusion of management practices, ideas and fashions—a research program that has made some use of historical evidence, which should not come as a surprise given the historical nature of its research questions. Among the earliest examples is the literature on the diffusion of the M-form. Much of this literature relies on quantitative, longitudinal data and many employ standard statistical techniques. An interesting example is Fligstein (1985), who tested five theoretical models, including transaction costs, organizational ecology, and neo-institutional theory, for their explanatory power regarding the spread of the M-form among large U.S. firms between 1919 and 1979. He found empirical support for a mimetic effect, but only within a specific industry, and for

what he termed the “power perspective”, which basically suggests that those who have most to gain from the particular organizational change, in this case, sales, marketing, and finance personnel, would be its strongest backers. While originally rooted in the science paradigm and aiming to test theory, Fligstein’s subsequent work on the evolution of large-scale organizations in the U.S.A. shows more consideration of contextual, historically contingent factors and aims to develop rather than test theories. This is also the case for a host of other, usually book-length studies of organizational change and its diffusion—addressing questions first raised by Chandler (1962, 1977) and challenging and/or modifying his findings, which will therefore be summarized and discussed in our section on “historical cognizance” below.

Linked to the research program on diffusion, and similarly grounded in neo-institutional theorizing, is a literature that has focused on management ideas rather than organizational forms. Sparked by the work of Abrahamson (1991, 1996) researchers have tried to develop theoretical models explaining both the frequent succession of new management ideas, conceptualized as “fashions” or “fads”, and the increasingly powerful position of a “fashion setting community” or, as Engwall, Kipping, and Üsdiken (in press) call them, “authorities on management”, which include business schools, management consultants, and the media. Empirically, many of these studies have tended to rely on citation counts (e.g. Abrahamson & Fairchild, 1999)—an approach, which has invited some criticism due to its methodological limitations and its underlying assumption that managers can easily be tricked into constantly espousing new fashions (e.g. Clark, 2004).

History hence has been fairly central to these studies of the diffusion of organizational forms and of management fashions, which have tended to cover parts or all of the period from the late nineteenth century onwards. But since these research programs have aimed at developing theories about the behavior of managers and the influence of outside agents and forces, we have categorized them as “history *to* theory”. A good example for this timeless—and somewhat mechanistic—use of history is the widely cited article by Barley and Kunda (1992), who provide a detailed overview of what they see as a pendulum-like swing between rational and normative managerial discourses and then causally linking these to long-term shifts in economic growth (using Kuznets’ waves), with upturns promoting investment in technology and the related use of “rational” discourses and downturns seeing managers employ “normative” discourses to maximize the use of extant technology. The same is true for a recent study focusing on the changes in the management consulting industry since the 1930s (Kipping & Kirkpatrick, 2013). While based on a wide range of historical evidence from the UK, including archival documents, the authors demonstrate little interest in history per se, aiming instead at establishing the causal connection between “the less regulated, more open field conditions” in management consulting (as compared to other professional services,

see Hinings, 2005), the entry of new firms and their “consequent effects on the dominant forms of organization” (Kipping & Kirkpatrick, 2013, p. 779).

Institutional ecology. This research stream emerged from a critique of the density dependence model, in particular, its apparent “ahistoricism” and lack of attention to sociopolitical bases of legitimation (Baum & Powell, 1995, p. 532; see also e.g. Marquis & Lounsbury, 2007). These concerns have bred a range of studies that have combined institutional ideas with ecological analyses or strived more deliberately to integrate the two perspectives, though not necessarily referring explicitly to the label institutional ecology suggested by Baum and Powell (1995). Some of this research has employed historical data to empirically assess and theorize the effects of institutional conditions on organizational evolution (e.g. Lounsbury, 2002). In keeping with the ecological tradition, these studies have relied on quantitative analyses, either of an exploratory or hypothesis-testing nature. Nevertheless, as the exemplars we review below will demonstrate, they have also included varying degrees of historical analysis, typically based on secondary sources, to provide either a background for or context-specific considerations in the development of research questions or hypotheses.

Haveman and Rao (1997), for instance, have studied the thrift industry in California during the period 1865–1928 to show how institutions coevolved with organizational forms. These authors were primarily concerned with integrating institutional and ecological theories by focusing on the interaction between technical and institutional pressures and examining whether selection or adaptation drove population evolution. Yet, they also brought in a historical perspective not only by studying organizations well in the past, but also by demonstrating how the Progressive movement in American history influenced the rise and demise of different organizational forms. In an extension of this study, Haveman, Rao, and Paruchuri (2007) examined how Progressivism in California led to the expansion of bureaucratic forms, which differed from the original logics of the thrift industry. They showed, based on data for the 1906–1920 period, that new foundings and conversions to the bureaucratic form were mediated by intermediate institutions, namely the Progressive media and the city-manager form of government.

Another example is a study by Dobbin and Dowd (1997), who drew upon the history of railroads in Massachusetts in the nineteenth and the early twentieth centuries to theorize on the effects of public policy on competition and business strategies. They showed that, of the three policy regimes they identified, public capitalization (1826–1871) encouraged railroad foundings by expanding resource availability, whereas pro-cartel policies (1872–1896) did the same by dampening competition. On the contrary, anti-trust policies (1897–1922) reduced foundings by increasing competition within the industry. Notably, the predictions of the density dependence model only held

when alterations in policy regimes were considered. Finally, in theorizing and empirically assessing the effects of social movements on organizations, Hiatt, Sine, and Tolbert (2009) examined the consequences of the activism of the Woman's Christian Temperance Union (WCTU) on two separate organizational populations, namely breweries and soft-drink manufacturers in the U.S.A. in the period 1870–1920. Their quantitative analyses tested hypotheses derived from a historical investigation of the WCTU based on published primary sources. What they showed was that the activities of the WCTU led to changes in the institutional environment, which then resulted in increasing failures of breweries, generating at the same time opportunities for the founding of manufacturers of non-alcoholic beverages.

History to Theory—Organizations

Process theorizing: Organizational change and strategy-making. Although process approaches have been employed at the macro-level of analysis, particularly more recently (e.g. Greenwood & Suddaby, 2006; Wright & Zammuto, 2013), the vast majority has been conducted at the micro-level, focusing, in particular, on organizational change (for an overview, see Üsdiken et al., 2011). Here, history has played a relatively strong role almost from the outset. Thus, Van de Ven and Huber (1990) were among the first to explicitly challenge the dominance of a model that looked at the inputs and outputs when studying organizational change, suggesting to complement it with “a ‘process theory’ explanation of the temporal order and sequence, in which a discrete set of events occurred based on a story or historical narrative” (p. 213). At the same time, they made it clear that such use of historical data was not intended to examine the peculiarities of any particular change process but to identify its “underlying generative mechanisms or laws” (p. 213)—which is why we have put process theory into the “history to theory” approach.

A similar position to Van de Ven and Huber (1990) is taken by Pettigrew, who actually wrote a history of the British company Imperial Chemical Industries (ICI) (1985). Again, rather than elaborating on the specificities of the ICI case, he used it, for instance, to critique the extant literature on the role of leadership in organizational change and to propose a new theoretical model which linked the content of the change with its process as well as the organization's inner and outer context (Pettigrew, 1987). In another, methodologically oriented article, he actually made a very clear distinction between “case studies” and “case histories”, stressing the need to go “beyond chronology to develop analytic themes” (Pettigrew, 1990, p. 277). Likewise, in a more recent call to develop more “historical studies of industrial, institutional, and organizational change”, Pettigrew, Woodman, and Cameron (2001, p. 700) see it as the main purpose for “historical investigation” to “provide long

time series”. But while historical evidence indeed seems particularly well suited to study processes of continuity and change (see, e.g. the contributions in Üsdiken et al., 2011), much of process theorizing has continued to rely on contemporary evidence and other methods, namely participant observation and interviews (e.g. Langley, 1999).

The same is true for process research within strategy, where a historical, longitudinal approach seemed promising, but ultimately remained marginal. Thus, Mintzberg in his pioneering research on strategy-making and strategic change drew on in-depth, long-term case studies, often stretching from the 1930s into the 1970s (reprinted in Mintzberg, 2007). By contrast, in his study of the internal corporate venturing (ICV) process in a diversified organization, Burgelman (1983) relied largely on the observation of ongoing projects over a 15-month period, but did also retrace their history. Interestingly enough, while claiming a Mintzbergian heritage, the research program investigating “strategy-as-practice” (e.g. Carter, Clegg, & Kornberger, 2008; Whittington, 2006) has so far made little use of historical data and methods—with very few exceptions (Whittington & Cailluet, 2008). Equally, if not more interesting, Burgelman (2011) himself recently made a strong case for combining grounded theorizing, longitudinal data, and historical methods to study complex social systems—incidentally characterizing his own earlier ICV research as “quasi-longitudinal” (p. 594).

Thus, all in all, history has played a relatively important role for testing, modifying, or developing theory in a number of research programs that emerged in organization and management theory since the 1980s. Out of these, at the macro-level, ecological studies have possibly made the most extensive use of historical data, but have also done so in mostly quantitative form (as time series) and in a very abstract manner (to generate universally valid theories). At the same level, neo-institutional theory was fairly ahistorical at the outset, but became more interested in using both quantitative and qualitative historical evidence as the research program broadened its reach in terms of interests and areas covered. It is also here, where we can find quite a few publications, which exhibit what we call “historical cognizance” and which we will discuss in the corresponding section below. Finally, at the micro-level, process studies examining mainly changes in organization and strategy seemed like a natural fit for a historical approach given their interest in sequences of events, but that potential has—somewhat surprisingly—only been realized to a very limited extent.

History *in* Theory: The Past as a Driver or Moderator

Different from the approaches and studies that we considered above, history has also been integrated into theoretical models based on the premise that the past influences the present in organizations and

organizational aggregates. Others have made reference to what we characterize as “history *in theory*”, including, for instance, “using history as a fundamental explanatory building block” (Hirsch & Gillespie, 2001, p. 74), “history matters” (e.g. Marquis & Tilcsik, 2013, p. 194) or “historical theories”, which “make time-dependent events or processes critical in explaining later stages and events of organizations” (Zald, 1990, p. 102). In general, in the research programs belonging to this approach, history is not considered solely as “data”, but as a “variable” in its own right, gauging either, at the macro-level, the past conditions within populations, fields, or communities or, at the micro-level, past characteristics of organizations. At the same time, this treatment of history remains very much in line with the predominant concerns of organizational and management theorizing in turning phenomena into constructs and operational variables (Ingram, Rao, & Silverman, 2012).

Much of the research that looks at history in this way has been building on Stinchcombe’s (1965) idea that organizations reflect the conditions, in which they have been founded. Indeed, Stinchcombe’s (1965) insight, which later came to be labeled as the “imprinting hypothesis” constitutes one of the prime examples of what Zald (1990) referred to as “historical theories of organization” (see also Marquis & Tilcsik, 2013). Stinchcombe’s (1965) thesis involved two main propositions: first, the rate at which new kinds of organizations were created and their structural properties, he argued, were influenced by the “enviroming social structure” (p. 145), which he interpreted as “any variables which are stable characteristics of the society outside the organization” (p. 142). Thus, industries or organizational populations, which appeared at different times in history varied in their structural characteristics.

Second, these original features persisted over time, resulting in an association between contemporary structures of organizations of a certain type and the age of that particular organizational form. This stability could have to do both with effective functioning of these structures and the “traditionalizing forces, the vesting of interests, and the working out of ideologies” that served to preserve them (p. 169). In this original version, the focus was on the influence of social and economic conditions at the time of founding on industries or organizational populations (see also Lounsbury & Ventresca, 2002). Yet, Stinchcombe’s (1965) propositions have prompted research not only at the macro-level of analysis dealing with organizational aggregates, but have also been extended to the micro (organizational) level with a focus on the effects of founding environments as well as founder characteristics and early key decisions on later organizational outcomes (see Marquis & Tilcsik, 2013). We will therefore review the resulting research at both levels. We also look at a number of other research programs, which developed since the 1980s, namely at the macro-level,

organizational ecology and co-evolution and, at the micro-level, structural inertia, path dependence and the capabilities/resource-based view (RBV) in strategy.

History in Theory—Populations, Fields, Communities

Imprinting. While Stinchcombe developed his ideas in the 1960s, they only found more widespread application since the 1980s—when, as we have seen, most other research programs engaging with history in some way also emerged. Thus, at the macro-level, two studies by Baron, Dobbin, and Jennings (1986; Baron, Jennings, & Dobbin, 1988) constitute early examples, providing a historical examination of how forms of worker control varied across industries in the U.S.A. in the period 1935–1946. They showed that, very much in line with Stinchcombe's (1965) formulation, control regimes were associated with the period, in which an industry was founded (Baron et al., 1988). These authors also demonstrated that, once bureaucratic forms of control made inroads into an industry, they tended to persist despite changes in environmental conditions (Baron et al., 1986).

There has been a more recent revival of interest in macro-level studies of the imprinting hypothesis—with the edited volume by Lounsbury and Ventresca (2002) playing a big role, as has the increasing attention to community level analyses of institutional and organizational phenomena (see, e.g. Marquis, 2003; Marquis & Battilana, 2009). For example, Marquis, Davis, and Glynn (2013) examined the relationship between local corporations and non-profit organizations in the same locality. Their assessment of the effects of various community institutions, including the density of corporations, demonstrated that cities, which had more corporations in 1905, were likely to experience greater growth in non-profits between 1987 and 2002. Moreover, in cities where the corporate community was established early, the positive association between contemporaneous corporate density and the growth of elite-oriented non-profit organizations turned out to be stronger. Along similar lines, but reversing Stinchcombe's (1965) thesis, Greve and Rao (2012) have suggested that the early founding of particular types of non-profit organizations in a community generates a positive imprint for future action toward establishing non-profits of another kind. This is based on the premise that history plays a central role in creating an “institutional infrastructure” for collective action. Their central argument has been that the extent of earlier foundings of non-profits generates variations in the institutional infrastructure that communities possess in developing different non-profit organizations later. The study showed that municipalities in Norway were more likely to establish consumer cooperatives in the first half of the twentieth century the earlier they had created village fire associations and savings banks in the previous century.

Organizational ecology—density delay theory. Stinchcombe's (1965) proposition concerning founding conditions has also constituted one of the backbones of organizational ecology (Hannan & Freeman, 1989). In theoretical and empirical terms, it has featured in the extension of the density dependence model as well as in the idea of structural inertia, which we shall take up below. Different from the recourse to history as a source of data that we discussed above, in this instance history (or the past) are treated as an element in theories of density dependence and structural inertia. In the case of density dependence, past conditions have been incorporated as a particular set of exogenous influences on organizational populations, conceptualized as "density delay" and operationalized as the population density at the time of organizational foundings (Carroll & Hannan, 2000). The argument has been that, in addition to effects of contemporaneous density, high population density at the time of founding is associated with higher failure rates for organizations. More intense competition due to high levels of density during founding implies that the particular organizational cohort confronts resource scarcity and often has to settle for inferior resources. These conditions are then likely to leave their mark, making these organizations weaker competitors over their entire lifetime and leading to persistently higher rates of failure (Carroll & Hannan, 1989b; Hannan & Carroll, 1992).

In a recent revision of density delay theory, Dobrev and Gotsopoulos (2010) suggested that the effects of high density at the time of founding are likely to vary according to when organizations enter into a population in its history. As new populations suffer from lack of legitimacy, the authors argued, for organizations founded in this early stage higher density should actually serve to reduce chances of failure. For those entering the population after it matures, on the other hand, mortality rates were likely to increase when density was higher, as the original version of density delay theory would predict. Empirical analyses of U.S., French, and German car manufacturers supported these ideas. Thus, density at the time of founding and its imprinting effects mattered but differently depending on the time of entry of organizations into the population.

Co-evolution. Finally, while more marginal as a research program in comparison to imprinting and organizational ecology, co-evolutionary theory has given a much more central role to history than the former. It originated with studies at the organizational level, aiming to bridge the apparent contradiction between the insights from organizational ecology, which seemed to give individual firms little hope to alter their fate, and the literature on strategic change/choice (e.g. Burgelman, 1991). Subsequent studies have focused more on aggregates of organizations, usually industries, and have relied on historical and comparative research (see, for a theoretical foundation, also Nelson, 1994).

For example, Djelic and Ainamo (1999) compared the evolution of the fashion industry in France, Italy, and the U.S.A., examining how environmental challenges drove firms to develop new organizational solutions—solutions, which displayed some common elements but were also deeply embedded in their local and national institutional contexts, in turn creating powerful legacies conditioning their subsequent trajectories and limiting the potential for convergence.

And there is, in particular, the work by Murmann (2003, 2013), who has investigated the origins of the synthetic dye industry in the UK, the U.S.A., and Germany in the nineteenth century. He shows how the co-evolution between firms and national institutions, in particular patent laws and university-based research, allowed the German companies to decisively move beyond those from other countries, capturing 90% of global markets at the eve of World War I. He has also issued calls for a wider application of this approach as a way to introduce history into strategy research (Murmann, 2012). But he and the others mentioned have found few followers—most likely because of the heavy data collection involved, covering not only all the firms in the industry, but also their evolving institutional context (see, as an exception, the contributions in Lamberg, Näsi, Ojala, & Sajasalo, 2006).

History in Theory—Organizations

Imprinting at the organizational level. Stinchcombe's (1965) original idea concerning the birth of industries and of organizational forms has also been transposed to the organizational level. In this version, imprinting has been construed in terms of the effects of the environments, in which individual organizations were founded, as well as the influence of founders and of the early decisions pertaining to organizational features such as strategy and structure (Marquis & Tilcsik, 2013). As our following review will show, studies of this kind, where the origins of organizations are considered as a key historical influence on their subsequent development and current state/behavior, have been, almost invariably, quantitative and have included very little or no historical research, other than constructing data from secondary sources.

In the first ever study that built on and, at the same time, extended Stinchcombe's ideas to the effects of founding environmental conditions on individual organizations, Kimberly (1975) examined organizations for the handicapped in the U.S.A. that were established in two different time periods. He showed that the organizations studied differed in the primary orientation of their activities, i.e. production or rehabilitation, based on whether they were founded before (or during) or after World War II. And in a series of articles based on a sample of U.S. semiconductor firms, Boeker (1988, 1989a, 1989b) considered for the first time founder characteristics as well as initial choices of strategy

as possible sources of imprinting in addition to environmental conditions at the time of founding. He showed, for instance, that both the functional background of the founder and the historical period, in which the firm was founded, had significant influences on the initial importance of different business functions. Moreover, results suggested that functional importance was more likely to persist in better performing firms and those that were younger and had founders with longer tenures (Boeker, 1989a).

Contributing to the turn in the imprinting literature toward the effects of founders rather than environmental conditions, Baron and colleagues researched a sample of emergent, high-technology Silicon-Valley companies based on data collected through surveys and retrospective interviews, examining the influence of the templates entrepreneurs imposed in early stages on various long-term organizational outcomes. Baron, Burton, and Hannan (1996), for example, showed that the initial employment models shaped later human resource policies. Likewise, Baron, Hannan, and Burton (1999) examined the effects of initial employment patterns on later bureaucratization. In terms of the persistence of these effects, the findings need to be interpreted with caution, since at the time of the research none of the start-ups had a life-span longer than 10 years. Extending the time-span and the database for the same sample of firms, Beckman and Burton (2008) moved beyond the individual founders to the founding teams. Their study showed, among others, that there was a strong relationship between functional backgrounds of the founding team and later top management teams as well as between the functional structure at founding and in later stages.

Another version of linking founder characteristics or early choices to later outcomes has been a “genealogical approach”, which considers what founders bring to newly founded organizations from the ones where they had worked previously. In two separate studies of Silicon-Valley law firms over the period 1946–1996, Phillips (2002, 2005) quantitatively examined the relationships between what was transferred from the parent firm to the offspring and the effects of these transfers on survival outcomes and gender composition. The central idea is that these founders carry over characteristics of the firm that they have left to the newly founded organizations. So, firms established by founders, who came from parent firms that had a history of placing women in high-level positions, for example, were more likely to promote more women to top levels, suggesting that practices may persist by diffusion through generations of firms.

Diverging from the more prevalent founder perspective on imprinting, Marquis and Huang (2010) recently returned to considering environmental effects at founding, though with a focus not on resource environments, as has typically been the case, but on institutional conditions. They advanced the idea of what they called “exaptation”, meaning that organizational capabilities developed as a response to founding environments may then be put into a

different use following institutional changes. Based on this theoretical reasoning, they showed that the involvement of U.S. banks in acquisitions after deregulation in 1978 was associated with variations in state-level restrictions on branch banking at the time these banks were founded. Marquis and Huang's (2010) study also indicated that the main effect of the founding environment was moderated by the extent of modernization (transportation infrastructure and urbanization) and the political culture (agrarian influence and Progressive support) in U.S. states at the time when the banks were founded. What this study has shown from the perspective of institutional theory is that organizations are influenced not only by their present institutional environments, but also by their past ones.

Structural inertia and organizational history. With an expanding attention to adaptation as well as selection, ecologists have also turned toward studying organizational level processes. Actually, issues around age dependence, for example, have been a central concern from the outset, remaining however, as Hannan et al. (2007, p. 291) put it, a "still recalcitrant problem" despite an extensive range of studies (see Baum & Shipilov, 2006; Carroll & Hannan, 2000 for reviews). Likewise, structural inertia theory, which constitutes a history *in* theory approach in organizational ecology, has posited that organizational histories constrain change particularly in old and/or large organizations due to standardization and institutionalization (Hannan & Freeman, 1989). The idea of structural inertia, as we pointed out above, has also been invoked in considering imprinting effects. Yet, in testing this theory on the role of the organizational past, ecologists have moved away from considering founding effects. Instead, they have turned toward researching how entire life histories of organizations influence outcomes of interest, such as survival and organizational change.

Thus, Kelley and Amburgey (1991), for example, examined the idea of change momentum, i.e. cumulative prior changes, on subsequent organizational change in the context of the U.S. airline industry during the years 1962–1985. They found that organizations with greater prior experience of changing were more likely to make changes of the same type in their product-market strategy. Moreover, they also found that prior core organizational changes did not decrease the chances of survival. Yet, as structural inertia theory would predict, older organizations were less likely to engage in changes in core properties. However, a prior history of more changes enabled managers to undertake further change. Overall, these results pointed to the value of taking a "historical perspective" in studying organizational change in ecological research, that is, the need to consider the entire history of organizations' past experiences (Kelly & Amburgey, 1991, p. 609). Other studies in the same vein showed how prior merger experience was positively associated with later mergers of the same type among large U.S. firms

(Amburgey & Miner, 1992); and how past changes drove subsequent changes in the content and in the frequency of publication of Finnish newspapers over almost two centuries—even if in the latter case, as more time passed after a previous change, the probability of further change decreased (Amburgey, Kelley, & Barnett, 1993).

Although, strictly speaking, not part of the organizational ecology research program, some recent studies have pursued the same route by considering the entire past experiences of organizations as conditioning their later actions. Thus, in a study of Australian retail banks between 1981 and 1995, Roberts and Amit (2003) examined the relationships between the level, composition, and consistency in their history of innovative activities and their competitive position. They showed that there was a significant association between the banks' history and their financial performance. Likewise, in a study of board reform, which diffused in two stages in large Canadian firms, Shipilov, Greve, and Rowley (2010) showed that firms, which adopted new practices in the first stage, were more likely to also adopt practices diffusing in the second stage. Based on these results the authors suggested that later stage diffusion might have less to do with mimetism of other firms and more with the effects of prior adoption.

In addition, based in part upon the preceding ideas, history has also featured in the conceptualization of and research on organizational identity. To be clear, the ultimate concern in work on identity as well as identity formation and change has been to develop general theory, with little interest in historical specificities. Yet, at the same time, history has been conceived as a key element constituting organizational identity. For instance, in their extensive review of the literature, Gioia, Patvardhan, Hamilton, and Corley (2013, pp. 125–126) have suggested that an “important part of identity is history because an organization can only know if it is acting ‘in character’ if it has a history of action consistent with its founding or adopted core values”. As this observation indicates, the formation of identity has been associated, at least in part, with the influence of the founders and its subsequent persistence with internal and external inertial forces. A recent study by Phillips and Kim (2009), based on data on jazz recordings in the U.S. Midwest between 1920 and 1929, is very illuminating in this respect. The authors distinguished between what they referred to as “Victorian Era” and “Jazz Era” firms—depending on whether they were founded before or after 1917, when jazz began to be commercialized. They then showed through quantitative analyses that the Victorian Era firms tended to preserve their high-status identities even at the cost of not fully benefiting from emergent business opportunities, engaging with the latter only through deception, i.e. producing recordings under pseudonyms.

Path dependence. Yet another perspective that has been employed in examining and theorizing the effect of history on organizations has been

path dependence. The notion itself has originated in economics and economic history in accounting for how a technologically inferior standard can become prevalent (e.g. David, 1985), though it has actually been questioned subsequently. Within the organization and management literature, path dependence has been interpreted and employed in different ways. Some authors (e.g. Farjoun, 2002a; Suddaby & Greenwood, 2009; Teece, Pisano, & Shuen, 1997) have construed path dependence in a generalized sense of “history matters”. Others (e.g. Beckman & Burton, 2008; Greve & Rao, 2012) have tended to equate the concept with long-lasting effects of initial conditions. Some of the newer literature, however, has argued that, in order to be theoretically useful in explaining how past events may drive current or future organizational outcomes, the idea of path dependence has to be further specified (e.g. Sydow, Schreyögg, & Koch, 2009). In this view, it needs to be seen as a “process”, made up of distinct phases such as a triggering critical event, self-reinforcing feedback and, finally, lock-in (see also Vergne & Durand, 2010). As this particular formulation indicates, path dependence approaches aim to provide generalizable explanations for phenomena such as lock-ins, rather than being concerned with historical periods, particularities, or contingencies. The same has been the case with the alternative path creation perspective that it has instigated, which accords greater room to agency—and less weight to history—in influencing all three of the phases referred to above (see, e.g. Garud, Kumaraswamy, & Karnøe, 2010).

In terms of actual research, path dependence (and creation) approaches have often relied on “historical” case studies, typically combining documentary evidence with interviews (cf. Vergne & Durand, 2010). Some of this work has spanned long periods of time using organizational archives, as in Schreyögg, Sydow, and Holtmann’s (2011) case study of Bertelsmann, or has examined the more recent past with greater reliance on interviews, such as Koch’s (2011) study of two German newspapers (the names of which were not disclosed). As these studies exemplify, within organization and management studies the path dependence approach has been mainly employed at the organizational level, though there have been instances where it has been used at the field level. A case in point is Farjoun’s (2002a) historical study of the institutionalization and de-institutionalization of pricing based on “connect-time” and its rudimentary replacement by the “flat-rate” in the on-line database industry, where path dependence features as a core element in the “dialectical process model” that is proposed.

Dynamic capabilities. Finally, as a kind of parallel to ecological studies at the population or industry level, there have been a variety of approaches at the organization level, drawing and building on the notion of “routines” introduced by Nelson and Winter (1982). This led to work on so-called “dynamic capabilities”, which were seen as enabling companies to adapt to changing

environments (e.g. Teece, 2007; Teece et al., 1997, for a recent discussion, Jacobides & Winter, 2012), even if many empirical studies showed the difficulties, even inability to change—explained very often through theories of cognition or mental models. What matters in our context is that a number of these studies have drawn on historical cases (e.g. Danneels, 2011; Langlois, 1997; Tripsas & Gavetti, 2000; for a study using longitudinal panel data, see Rothaermel & Hess, 2007). Within these cases, the past—through a sort of historical “lock-in”—becomes part of the theoretical model itself.

Relatedly, in the RBV of the firm (e.g. Barney, 1991; Wernerfelt, 1984), which has exercised a significant influence on strategy research over the past decades, history displays a similarly ambiguous influence on a firm’s sustainable competitive advantage. Thus, Barney (1991, pp. 107–108) posits that what allows firms to obtain resources that are difficult for others to imitate are their geographic location and a “particular unique time in history”—explicitly giving the past a theoretical status akin to the one it holds in research on founding conditions and path dependence. However, while Barney drew on some previous studies (1991, pp. 107–108) to derive his model, subsequent empirical research within the RBV paradigm has done little to explore or test the centrality afforded to history. Moreover, others, drawing on insights from institutional theory, have highlighted that “firms can be captives of their own history”, ultimately leading to sub-optimal choices (Oliver, 1997, p. 700). Yet others, while equally drawing on institutional theory as well as social memory studies, have challenged this view, pointing out instead that a rhetorically constructed history can contribute to a firm’s internal and external legitimacy and hence become a source of competitive advantage (Suddaby, Foster, & Quinn-Trank, 2010). These authors subsequently illustrated this argument through a historical case study of the Canadian firm Tim Hortons, based largely on secondary sources (Foster, Suddaby, Minkus, & Wiebe, 2011). This is an ongoing debate, which might benefit from additional historical research to further explore and delineate the (positive or negative) role attributed to history in the underlying theoretical models. Strategy research, in general, as has been argued forcefully by Farjoun (2002b), could benefit from a deeper engagement with history (see also Kahl et al., 2012b).

Within the research programs discussed in this section, history has featured as an explanatory factor. Although considering historical variation as an element of theory, such treatments of history have inevitably constituted an oversimplification. The imprinting and ecological studies have typically been based on quantitative data and have operationalized “history” as a very specific, single—occasionally composite—variable, such as the background of founders or cumulative experience in certain activities like mergers or, more generically “changes”. The few historical case studies that can be found in the research programs on path dependence and organizational capabilities have generally been based on interviews or secondary sources and have rarely attended to the specificities of

the particular context. Even the few studies that have been somewhat more oriented toward historical research and narrative and have attempted to be more attentive to temporality and the underlying mechanisms affecting outcomes have tended to shy away from considering particularities within long-run developments—not surprising given predominant generalizability concerns. Here is where the kind of studies discussed in the subsequent section stand out.

Historical Cognizance: Taking Context Seriously

As noted at the outset, in addition to identifying research programs that used history either to build/test theory or as a variable within the theoretical models themselves, we also looked for studies that “took history seriously”. These studies could come out of either of the two approaches that we used to structure our overview. Thus coming from “history *to* theory”, they would develop new or modify extant theories not in a timeless and universal manner, but instead try to explicitly identify and conceptualize the influence of specific historical time periods—as a kind of boundary condition. And when it comes to history as a determinant *within* theory, they would not just consider a kind of generalized past, but would be concerned more explicitly with, as Isaac and Griffin (1989, p. 886) put it, “a theorized understanding of the historical particularities and contingencies of the series and relationships under analysis”.

When reviewing the literature, we managed to identify a number of publications that met these criteria. We consider this as an emergent and growing trend toward what we chose to call “historical cognizance”. And while we do refer to the previous sub-division of history *to* and *in* theory to denote their origins, it needs to be stressed here that these publications are occupying a space in between the two, since they ultimately lead to theories that are more historically sensitive. What can be said more generally is that,

- (1) in early examples most of these publications were not from the top U.S.-based organization and management theory journals, but rather books—which should not come as a surprise, both because books do not have the same constraints as journal articles in terms of length, which allows their authors to present more contextualized and nuanced views, and because these studies were addressing inter-disciplinary and novel issues, less amenable to journal publication. Notably, however, what we have also been able to identify is that historically cognizant research has more recently and increasingly been extending initially to sociology journals and then to the ones in organization and management studies;
- (2) there were significantly more publications of this kind emanating from the history *to* theory approach rather than from history *in* theory. This does come somewhat as a surprise, because there is no inherent reason, for

- instance, why quite a few papers with (neo-)institutional approaches can be found in this category, whereas research on “imprinting”, for instance, generated close to none, despite Stinchcombe’s (1965, 2005) own strong support for historical research—but hence the understandable exhortation by Marquis and Tilcsik’s (2013) survey for “taking history seriously”; and,
- (3) while they emanate from various of the research programs discussed, they do not dominate any of them—“institutional logics” probably coming closest. Instead, they are clustered around a number of issues/topics where it would seem to make sense to take a more historically cognizant approach—topics, about which, in many cases and not surprisingly, historians have also conducted quite abundant research.

In the following, we provide what needs to be considered a tentative overview of this trend, organized around some of the most important issues/topics examined by these publications.

Accounting for Big Business (in the U.S.A.)

When it comes to develop or modify theory from history, one of the major issues concerns the origins and transformation of big business in the U.S.A. (and to a lesser extent elsewhere). This has also been one of the central concerns of business historical research from its inceptions in the 1920s and even more so with the work of Chandler since the 1950s (McCraw, 1988). The respective research emanating from organization and management theory often criticized, modified, or theorized findings of the purely historical research. Thus, (business) historians had drawn attention to the early railroads, not only as providing the necessary infrastructure for the emergence of large-scale businesses benefitting from economies of scale (and speed), but also as a model for how to manage these kinds of organizations. The early history of the railroads has also attracted some interest from management scholars, with the work of Dobbin, in particular, displaying a significant degree of historical cognizance.

A book that examined policy-making toward the railroads in the U.S.A., the UK, and France demonstrates the power of both an historical and comparative approach (Dobbin, 1994). In essence, it argued that railroad policy in each country was shaped by the extant political culture, and in turn shaped the subsequent “industrial ideology”—with the U.S.A. promoting the market mechanism even if detrimental to small, entrepreneurial firms, the UK displaying the opposite preferences and France seeing centralized, bureaucratic state control as superior. While this is fundamentally an institutional argument, identifying isomorphism between the political and economic spheres, hence history *to* theory, it is also historically sensitive in that it shows sequences of the varied past in the three countries conditioning their

future policies, an insight one can find in the history *in theory* approach. This once again highlights how “historical cognizance” is situated between these two approaches—something that also offers a possibility for moving forward (see below). And in a historically cognizant article, Dobbin and Dowd (2000) examined, based on historical evidence, how the different ways, in which competition was managed within the Massachusetts railroad industry, was impacted by a kind of period effect, namely due to changes in anti-trust legislation. Thus, prior to the enforcement of anti-trust law in 1897, they identified a “cooperative” model as dominant, which involved price fixing or cartels. Under the anti-trust regime, by contrast, the finance model, which was based on friendly mergers, became prevalent—a fact that Dobbin and Dowd (2000) tied to the influence exerted by the banking industry. The banks had significant stakes in the smaller railroads and wanted to avoid bankruptcies, which would have been the result of the alternative “predatory” model, where competitors were deliberately driven into bankruptcy and then acquired. This paper also shows the extent to which making theorizing more historically cognizant is a matter of degree, since the authors had drawn some more general theoretical insights based on similar data in an earlier paper (Dobbin & Dowd, 1997; see above).

Even more squarely targeting Chandler (1977, 1990) and his notions that technological innovation, economic efficiency, and the “visible hand” of management drove the rise of big business in the U.S.A.—and its more tardy appearance elsewhere—are the books by Roy (1997) and Perrow (2002). Broadly and simplistically put, their alternative accounts focus instead on the role of a “power logic”. Thus, Roy (1997) rejects what he refers to as “efficiency theory” for the emergence of large corporations, first by conducting statistical tests based on stock market and census data, showing that high capital intensity and productivity not necessarily led to higher growth and profitability. But what makes his book an example for “historical cognizance” is his subsequent analysis (based on secondary sources), which shows that the corporate form was created by governments to organize the construction and operation of canals, bridges, etc. due to the absence or unwillingness of private capital for doing so. And similarly, the financial institutions, which later funded private enterprise, originally traded government securities. And it was both the availability of the corporate organizational form and the institutionalized power relations, in particular, at “Wall Street”, which eventually led to the creation of private corporations. While theoretically underpinned, this is also a deeply historical account—in many ways, more historical than Chandler’s, and certainly more contextual since it goes beyond big business as the unit of analysis.

Perrow (2002) goes even further in pointing at power as the major transformative driver for the creation of big business in the U.S.A. in the late nineteenth and early twentieth centuries—the power of corporate shareholders

and managers as well as investment bankers. What he argues is that the U.S.A. was special in that, unlike in Europe, these groups did not face any countervailing interests and power—there was no nobility, for instance, and that the state was weak, and that these powerful groups also managed to have the Supreme Court and lawmakers adopt rulings and laws in their favor. While this is a history *to* theory account built around a power logic, it becomes historically cognizant by adding a history *in* theory element, showing how this logic first played out when the U.S. railroads became dominated by private interests at the national level, leading to path dependencies in terms of ownership (from public to private), of regulation (or rather a lack thereof), and of scope (from regional to national): “The railroads’ twisting historical path [...] made the modern multidivisional multiproduct corporation possible” (Perrow, 2002, p. 183).

While both Roy’s (1997) and Perrow’s (2002) critiques were largely based on secondary sources, Freeland (1996, 2001) conducted an in-depth, archive-based study of General Motors, in which he challenged the original findings by Chandler (1962) and its further theorization by Williamson (1971), who had identified economic efficiency as a major driver for M-form adoption. Freeland (1996, 2001) instead highlighted the role of the continuous power struggles between owners and various levels of management, suggesting that “governance by consent” rather than strict divisionalization tended to lead to superior performance. And there is, in particular, the work by Fligstein that deserves a special mention here. After having used historical data to empirically test various theories for their explanatory power regarding the M-form (see above), he investigated large firms in the U.S.A. and their organization and leadership during the course of the twentieth century more broadly (Fligstein 1987, 1990). Based on both primary as well as secondary data and combining qualitative with some statistical analysis, he elaborated a very nuanced account and periodization, taking into consideration the changing political, legal, economic, business, and even educational contexts. Theoretically, he developed the notion of “conceptions of control” (Fligstein, 1990, p. 10)—totalizing world views that cause actors to interpret every situation from a given perspective, which he linked, among others, to the functional backgrounds of the top managers of large U.S. firms (see also Fligstein, 1987). In its complexity and the way the varying context is taken seriously, this is in many ways a more “historical” account than the one provided by Chandler (1962, 1977, 1990).

Spreading Management Models and Ideas

Another issue that clusters historically cognizant work among scholars in organization and management theory are the above mentioned diffusion studies, which look at how organizational models have spread over time and space. Somewhat of a path-breaking role was played by Westney’s (1987)

book, which examines the transfer of three organizational models from the West to modernizing Japan during the Meiji period (1868–1912). Based mainly on secondary sources, the book focuses primarily on three empirical case studies: the police (imported from France), the postal service (based on the English model), and newspapers (emulated from the Western example more generally). But Westney (1987) also derives more general theoretical insights, dismissing some of the predominant explanations at the time, namely cultural idiosyncrasies, on the one extreme, and the universal imperatives of a latecomer nation or of industrialization, on the other. Instead, she sketches a more dynamic model, which links organizational and societal change (with the new organizations shaping traditions at least as much as vice versa) and with both the models and their emulations continuing to evolve—with the imitation sometimes advancing beyond the original, as is evident, for instance, in the earlier professionalization of the police service in Japan. While therefore belonging to the “history to theory” category, the study is exemplary in the way it is cognizant of the effects of various contexts: (i) organizations and their environment; (ii) Japan and the Western countries; and (iii) historical time periods stretching from before to after the Meiji era.

The same is true for a book-length study by Guillén (1994). Covering much of the twentieth century, he examined how scientific management, human relations, and what he calls the “structural analysis model”, which also relates to Chandler’s M-form, were discussed and applied in the U.S.A., Great Britain, Germany, and Spain. Based largely on secondary sources, he highlighted, in particular, the role of what he called “management intellectuals” in these processes. Again, what makes this study exemplary, is that while aiming “to draw some theoretically meaningful causal conclusions about the ways in which organizational change takes place that could be applicable beyond the four countries included in this study”, Guillén (1994) identified and stressed the particular “conditions” and “configurations of institutional factors” that explained developments in each country (pp. 266–267). What should once again be highlighted here are the benefits derived from taking both a comparative and historical approach (see also Greif, 2006).

These benefits are also apparent in a book that looks at how the American model of corporate organization spread to France, Germany, and Italy after World War II (Djelic, 1998). Based on selected primary as well as secondary sources, and drawing on the neo-institutional framework of coercive, normative, and mimetic forces, the study identifies the crucial role of U.S. pressures—applied to varying degrees in the different countries, and, even more importantly, of national “modernizing elites”. In a subsequent article, Djelic (2008) has also examined and discussed how notions of time and history have been used in diffusion studies more generally, ultimately concluding that, moving forward, historical approaches should be complemented with genealogical and/or archaeological ones. The topics of both books had also

been addressed quite extensively by historical research—with lesser or larger degrees of theorization (see, e.g. Kipping & Bjarnar, 1998; Zeitlin & Herrigel, 2000). This highlights both a challenge and, as we will argue below, an opportunity in terms of the direct interaction between research and researchers in history and in organization and management theory.

Finally, an interesting extension of these studies on the diffusion of organizational forms and management ideas is recent work that has historicized the various schools of management thought themselves, highlighting, in particular, how subsequent scholarship had “demonized” scientific management, while the human relations school and its founders, namely Elton Mayo were “deified” (Bruce & Nyland 2011; Hassard, 2012). Based on actor-network theory, Bruce and Nyland (2011) actually show how this served John D. Rockefeller Jr. and other powerful business leaders to legitimize their authority in the workplace and society at large. In a related paper, based on secondary sources, Hassard (2012) questions the established textbook accounts of the origins of the human relations approach at Western Electric’s Hawthorne works. Examining the broader social and political contexts, he shows that Hawthorne’s welfare capitalism not only preceded the arrival of the Harvard researchers and of Elton Mayo, but was also based on a mixture of paternalism and anti-unionism—with his study ultimately offering an alternative narrative for the emergence of human relations.

Historicizing Institutional Logics

Among the various research programs, the institutional logics approach has also generated a number of historically cognizant publications, mostly in journals. As we discussed above in the section on “history to theory” at the macro-level, the institutional logics literature has made use of historical analyses in addressing temporal shifts in dominant logics or sources of plurality within organizational fields—though mainly with a view to developing generalizable models (e.g. Thornton, Jones, & Kury, 2005). However, especially during its early development, when the focus was on identifying historical shifts and their organizational level consequences, particular attention was paid to history as a moderator of causal processes shaping organizational characteristics, actions, and outcomes. Indeed, although there has also been a companion search for history-independent effects (see e.g. Thornton, 2004), historical contingency has been identified as a “meta-theoretical” principle of the institutional logics approach (Thornton & Ocasio, 2008; Thornton, Ocasio, & Lounsbury, 2012). This assumption implies that both institutions and their organizational effects are historically contingent.

Thornton and Ocasio (1999) demonstrate this idea by documenting the shift from an editorial to a market logic in the higher education publishing industry in the U.S.A. Based on historical analysis as well as interviews, they

first divided the time span of their study (1958–1990) into two periods, and then showed through quantitative analyses that the antecedents of executive succession differed across the two periods, organizational structure and size being more salient under the editorial logic (i.e. before the mid-1970s) and acquisition strategies and competition under the subsequent market logic. Using the same research design, data set and methods, Thornton (2001) later showed that institutional logics—or the time periods in which they prevailed—moderated the relationships between organizational and market antecedents and the risks of acquisition. She obtained similar results for the associations between professional and market variables and the change to divisionalization in the organizational structure (Thornton, 2002).

Joining these studies, but published in book format, was the project by Scott, Ruef, Mendel, and Caronna (2000), which examined institutional change in the U.S. health care field by tracing alterations in dominant logics over a period of five decades. They identified three historical periods, namely the eras of “professional dominance” (1945–1965), “federal involvement” (1966–1982), and “managerial control and market mechanisms” (1983–1990s), in which different logics prevailed. Focusing on the San Francisco Bay Area, change was studied at the level of the field and of separate organizational populations as well as at the organizational level through selected case studies, which also made use of primary archival data. While, overall, this was a study that exemplifies what we refer to as history *to* theory, the authors also recognized—though perhaps less strongly than the research we discussed above—that not only the meanings attributed to practices varied across the historical eras, but also the relationships among the variables that were examined.

Along the same lines—albeit in a purely quantitative study—Zajac and Westphal (2004) also obtained empirical support, in the context of stock market reactions to stock repurchase plans, for the moderating effects of historical periods, in which different institutional logics prevailed. They showed that, as an “agency logic” increasingly replaced the “corporate logic” of governance from the mid-1980s onwards, market reactions to stock repurchases of large American firms turned from negative to positive. Although again not involving any particular historical research, a similar idea is in part taken up in Greenwood, Díaz, Li, and Lorente’s (2010) study of downsizing in Spanish manufacturing firms. In developing the theme that organizations often operate under multiple institutional pressures and therefore examining the effects of state and family logics in addition to the market logic, they show the significant role of regions, a key element in Spanish history, in shaping firm action. The authors forcefully argue that these results demonstrate that “the intensity of community pressures, the form that they take, and the receptivity of particular organizations are contingent on history” (Greenwood et al., 2010, p. 535).

And the Rest . . .

In addition, there are a number of more isolated studies that demonstrated greater historical cognizance in developing theory from history. As a scholar, Haveman has played an important role in this respect. Thus, the research by Haveman, Russo, and Meyer (2001) on the impact on hospitals and thrifts of the introduction of a “managed-competition program” in California in 1982 provides a relatively early example at the organizational level of analysis. This study essentially involved the testing of general hypotheses pertaining to the influence of a radical regulatory shift on domain change and CEO succession as well as the effects of the latter changes on financial performance. Yet, from our perspective what sets this study apart is the particular emphasis on the role of history and time. Following the observation that “longitudinal studies of organizations are typically ahistorical in that theorized relationships are assumed to be time-invariant”, the authors conclude by stating that “environmental punctuations partition the history of an industry into periods during which different causal processes operate” (p. 270).

Another, more recent and kind of perfect example for what we consider historical cognizance is a study of entrepreneurship by Haveman, Habinek, and Goodman (2012). These authors examined entrepreneurship within the context of the U.S. magazine industry between 1741 and 1860, focusing on two periods, namely 1741–1800 and 1841–1860. They showed that the “social position” of the founders with respect to “occupation, education and geographic location” varied in the two periods (p. 587). Relative to founders in the eighteenth century, those starting new magazines in the mid-nineteenth century were likely to come from outside the industry and from more modest backgrounds. Haveman et al. (2012) have combined a relatively detailed historical account with quantitative analyses based on data constructed from secondary as well as some published primary sources. Notable for us is again the particular emphasis on the effects of the specific historical periods and the call that the authors make for “grounding studies of entrepreneurship in historical context” (p. 585), which may “set important scope conditions on any theory of entrepreneurship” (p. 617). This is a study, which should also be welcomed by (business) historians, who have clamored for more context-based as compared to the predominant cross-sectional, characteristics-based research on entrepreneurship (Wadhvani & Jones, 2014).

Similar in its main theme to Haveman et al. (2012) is a study by Arndt and Bigelow (2005) on the masculinization of the previously female-dominated hospital administration in the U.S.A. in the early twentieth century. Although the authors do not specifically describe their study as historical—even if they use primary (published) historical sources, analyzing them using qualitative methodologies (pp. 237–239). Based on their analysis, Arndt and Bigelow (2005) point out that studying change in the gender composition of hospital

administrators in that particular historical period enabled them to research the rare case of the masculinization of an occupation. This is notable because existing theories on change in gender boundaries have focused on feminization. Moreover, with this study, they have been able to show that, different from current theoretical thinking on gender-based occupational boundaries, masculinization was a process internal to the occupation.

Our review revealed fewer cases of what we would consider examples of historical cognizance, where history featured with its specificities *within* theory—the few that we could locate being at the macro-level. Although not devoid of concerns with generalization, the studies we did identify would couch their hypotheses in the historical context that they were examining. An exemplary recent study in these respects came from the social movement literature, where King and Haveman (2008) examined the founding of anti-slavery societies in the U.S.A. in the period 1790–1840. The authors point to the significance of this particular historical period in the birth of social reform organizations in the U.S. The focus on this period enabled the authors not only to attend to the conditions contributing to the genesis of the anti-slavery movement in the U.S., but also to address more generally the antecedents of social movement formation. Their study showed that the mass media of the time had a major role to play in anti-slavery organization foundings, whereas the influence of religious organizations varied according to their theological orientations.

Another notable example is Mezias and Boyle's (2005) work on the emergence of the film industry in the U.S.A. in 1893–1920, which, they suggest, provides an appropriate “historical context . . . to study the institutional ecology of competitive intensity” (p. 24), indicating what we would refer to as a history *to* theory approach. Indeed, the study was driven by an overriding theoretical concern, which involved the relationship between legal environments and population dynamics. Yet, they examined context-specific hypotheses that were derived from a historical review of a trust that had come to dominate the industry—the Motion Pictures Patents Corporation (MPPC). Results showed that MPPC members suffered lower mortality rates. On the other hand, lawsuits filed by the MPPC and higher market shares of its membership increased mortality in the entire population. However, with the advent of anti-trust policies not only did litigation against the MPPC reduce mortality rates in the population, but also MPPC members had greater difficulty in adapting to the turn toward making feature films. Mezias and Boyle (2005) are careful to note that “the specific historical events that occurred during the emergence of the American film industry are unique”. Nevertheless, as is typical of a theory-oriented paper, this point is also accompanied by the statement that this “representative” setting was chosen “to ensure that the results would be applicable outside the setting of the emerging U.S. film industry” (pp. 29–30).

Finally, as noted at the outset, what is somewhat surprising is the almost complete absence of what we consider historically cognizant studies among the large “imprinting” literature, which covers both the micro- and macro-levels—and this despite the significant interest in and support of history and historical methods expressed by Stinchcombe (1965, 2005) himself. What might explain this is that these studies take only a view back from the present to a kind of stylized past as a driver for the former and have little interest in understanding the historic context of the founding conditions per se or, for that matter, in the developments occurring between that founding moment/period and the present. Nowhere is that perhaps more obvious than in the study of the establishment of the Paris Opera by Johnson (2007). While using primary sources from the period, the author kind of imposes modern notions of “cultural entrepreneurship”, “isomorphic” processes and stakeholder power. In stark contrast to the entrepreneurship study by Haveman et al. (2012), discussed above, Johnson (2007), largely disregarding the specific historical context, offers, for instance, the suggestion that Louis XIV as an important stakeholder had his “modern (albeit significantly less powerful) counterparts [. . .] in the persons of venture capitalists, philanthropists, legislators, and corporate lawyers” (p. 100). In their extensive review of the imprinting literature in this journal, Marquis and Tilcsik (2013) seem to have recognized this shortcoming, since they elaborate a revised theory of imprinting that looks at “how specific phases of the past (rather than the vague totality of historical conditions) matter” (p. 230) and subsequently provide a number of exemplary topics such as institutional complexity and networks where an imprinting perspective could contribute to examine how history matters in organizations.

As this overview has shown, there have been quite a number of studies in organization and management theory that do take history seriously. These could be found clustered around a number of important research questions, such as the rise and transformation of big business or the diffusion of management models and ideas—questions that are shared by both management scholars and (business) historians. There were also historically cognizant publications across some of the research programs we discussed above—and, in particular, within the institutional logics literature, while they were largely—and surprisingly—absent from others, namely “imprinting”. Both the growing presence and the unexplained absence of such publications opens opportunities for further strengthening and possibly re-shaping the role of history in organization and management theory. We will discuss these opportunities and suggest how they can and should be exploited in the following, final section after briefly summarizing our findings regarding history *to* and *in* theory.

Summary, Discussion, and Suggestions

In summary, there is quite a bit more history than meets the eye in organization and management theory. We come to this perhaps somewhat surprising conclusion based on a broad definition of history as an empirical and/or theoretical concern with the past and/or the use of historical evidence, both quantitative and qualitative, and with a sampling approach that went beyond the “top” journals used by previous surveys—even if they retained a prominent position—and also included the strategy literature. Our survey, summarized in Table 2 shows that “history” can be found in a wide swath of research programs within organization and management theory—as well as strategy, at both the macro- and micro-levels of analysis.

What we also confirmed are two very distinct uses of history in organization and management theory: one, where historical evidence serves to develop, modify, and—less frequently—test theories, an approach we refer to as “history *to* theory”; the other, where history, i.e. events or conditions in the past determine—directly or as a moderating factor—the present, which we call “history *in* theory”. In both cases, the use of history seems to be dictated often not by a conscious choice but by need, since certain theories require evidence that covers longer time periods in terms of longitudinal data or a sequence of events, while others incorporate the past as an explanatory (or moderating) variable. In some of these cases, for instance, path dependence, “history” is the only choice, since the theory would not work without a sequence stretching back into the past. In most others, say for studying changes in institutional logics or identifying founding conditions, researchers have to make a trade-off between the benefits resulting from the use of history and the difficulties inherent in collecting the required historical evidence—evidence, which is often less comprehensive and consistent than cross-sectional data. It is quite telling that despite these obvious challenges history has been used relatively extensively—definitely more frequently than previous surveys and ongoing discussions about the apparent need for a “historic turn” suggest.

So, does this mean, there is no longer any need for that “historic turn”? Not quite. First of all, the uses of history we have surveyed and summarized in this paper are still grounded within the dominant science paradigm. So, if one believes that this paradigm should be replaced or complemented by approaches from other disciplines, including history, then indeed there is such a need. But, as others have realized and suggested (Greenwood & Bernardi, 2014; Leblebici, 2014), this would require management publications to accept papers from history, literary studies or other liberal arts on their own terms, rather than asking them to conform to the evaluation criteria of organization and management theory, or it would require management scholars to think and write like historians—which seems little realistic, given the strong emphasis on theory in

Table 2 History and Historical Cognizance in Organization and Management Theory: Selected Studies

	History to theory	Historical Cognizance	History in theory
Macro/ecological	<i>Organizational ecology</i> Langton (1984) Carroll and Hannan (2000) Dobrev (2001) McKendrick and Carroll (2001) <i>Institutional Theory</i> <i>Institutional change</i> Brint and Karabel (1991) DiMaggio (1991) Leblebici et al. (1991) Holm (1995) Greenwood and Suddaby (2006) Wright and Zammuto (2013) <i>Institutional entrepreneurship</i> Hargadon and Douglas (2001) Maguire et al (2004) Lounsbury and Crumley (2007) David et al. (2013) <i>Logic change/plurality</i> Lounsbury (2002) Sine and David (2003) Rao et al. (2003) Marquis and Lounsbury (2007) Dunn and Jones (2010) <i>Diffusion</i> Fligstein (1985) Barley and Kunda (1992) Abrahamson and Fairchild (1999) <i>Institutional ecology</i> Dobbin and Dowd (1997) Haveman and Rao (1997) Lounsbury (2002) Haveman et al. (2007) Hiatt et al. (2009)	<i>Big business</i> Fligstein (1990) Dobbin (1994) Roy (1997) Dobbin and Dowd (2000) Perrow (2002) <i>Transfer of management models</i> Westney (1987) Guillén (1994) Djelic (1998, 2008) <i>Institutional logics</i> Thornton and Ocasio (1999) Scott et al. (2000) Thornton (2001, 2002) Zajac and Westphal (2004) Greenwood et al. (2010) <i>Other</i> Mezias and Boyle (2005) King and Haveman (2008)	<i>Imprinting</i> Baron et al. (1986) Baron et al. (1988) Marquis (2003) Greve and Rao (2012) Marquis et al. (2013) <i>Organizational ecology</i> Carroll and Hannan, (1989b) Hannan and Carroll (1992) Dobrev and Gotsopoulos (2010) <i>Co-evolution</i> Djelic and Ainamo (1999) Murmman (2003, 2013) Lamberg et al. (2006)

Table 2 (Continued)

	History to theory	Historical Cognizance	History in theory
Micro/organizational	<i>Process theorizing</i> Pettigrew (1985, 1987) Van de Ven and Huber (1990) Mintzberg (2007) Burgelman (2011)	<i>Big Business/M-form</i> Freeland (1996, 2001) <i>Other</i> Arndt and Bigelow (2005) Haveman et al. (2001) Haveman et al. (2012)	<i>Imprinting</i> Kimberly (1975) Boeker (1988, 1989a, 1989b) Baron et al. (1996) Baron et al. (1999) Phillips (2002, 2005) Beckman and Burton (2008) Marquis and Huang (2010) <i>Organizational histories</i> Kelley and Amburgey (1991) Amburgey and Miner (1992) Amburgey et al. (1993) Phillips and Kim (2009) Roberts and Amit (2003) Shipilov et al. (2010) <i>Path dependence</i> Farjoun (2002a) Koch (2011) Schreyögg et al. (2011) <i>Dynamic capabilities/RBV</i> Barney (1991) Langlois (1997) Oliver (1997) Teece et al. (1997) Suddaby et al. (2010)

all the publications we surveyed regardless of the category they belonged to. But, as we have also shown, there is a way, in which management and organization theory can “take history seriously”—without having to renege on social science methodology and the overall concern with theorizing. As Greif (2006, p. 388) has also pointed out with respect to studying institutions and institutional trajectories, for example, “emphasizing the context-specificity of institutions does not imply aborting the social-scientific tradition of seeking generalizations”.

These are the studies that we have identified as displaying “historical cognizance”. They represent a “third way”—situated between the almost “pure” science approaches based on cross-sectional data and the narrative, post-modern “historic turn”—and point to an opportunity for organization and management scholars to take on board some of the unique strengths of historical research—namely its attention to the peculiarities of a specific historical context. It is these peculiarities, which the historically cognizant researchers have not only examined in detail, but also incorporated in their theorizing. What needs to be stressed here is that this should not be considered in anyway as a “superior” approach, but is only intended to complement the others, which remain entirely valid. Hence, it is not meant to challenge the positivist approach, where the majority of publications—in particular, in the top journals—are still situated, nor does it minimize the value of studies that take a more narrative stance or those that apply Foucault’s conceptions of history to management (see above). And neither does it want to turn management scholars into historians—even if some of the “historically cognizant” studies discussed above do make a contribution to historiographical debates. At the same time, however, these studies have demonstrated the validity of looking into making many of the extant organization and management theories more historically contingent. Moreover, the accumulation of such studies has the additional potential for developing a more incisive understanding of organizational and management phenomena (Greif, 2006).

The question then becomes how to move from “history *to* theory” and “history *in* theory” to “historical cognizance”. Based on our detailed analysis above, three specific suggestions can be made:

- (1) *Be more explicit and reflective about how history is being used:* As seen above, history has made its way into quite a few research programs in organization and management theory. In many of them, it is used in a purely functional way, because it provides variation in terms of the data or because the past is needed as a predictor for the present. This could be done with more consideration and explanation of how this data or the stylized past is derived and employed. This is similar to the suggestion made by Coraiola et al. (in press), who point to the somewhat uncritical acceptance of historical knowledge by many social scientists, while many

historians actually have less ontological certainty about the “reality” of their accounts. Hence, there is a need for a more explicit and critical discussion of (i) the origins of their historical evidence, in terms of the primacy and originality of the underlying sources; (ii) the use it is being put to, namely in terms of our categories (history *to* or *in* theory), and (iii) their own ontological and epistemological position toward this evidence (Rowlinson et al., 2014). This would help to assess the validity of that evidence and increase confidence in (a) the theory that is being tested, modified or developed, on the one hand, or (b) the theoretical model, of which it is made a part, on the other.

- (2) *Recognize, even look for period effects and historical contingency*: Discussing “history”, its origins and uses more extensively is a first, and important step. As the various studies that we have categorized under “historical cognizance” have demonstrated, it is possible to go further, namely by (a) placing and understanding the historical evidence/past in its own peculiar context; and (b) making that peculiarity an explicit part of theorizing itself, through the introduction of period effects or the development of historically contingent theories. As seen above, studies that had both a historical and comparative dimension seemed particularly well suited to produce such an outcome, since they increased the variation in context and made its influence very apparent, prompting even those looking for general causal mechanisms to realize their conditionality on context-specific factors. This suggests that future studies aiming to further historically cognizant theorizing should be set up in ways that allow to specifically examining this conditionality.
- (3) *Interact with theory-conscious historians and their research*: As noted, many of the studies that we characterized as historically cognizant addressed topics that had also been researched quite extensively by historians. Sometimes, these studies by management scholars directly challenged the extant historical literature, while at other times they used some or part of it as evidence. This suggests that a more systematic and consequential interaction between management scholars and (business) historians might be of benefit. This can range from mutual recognition of the respective research results, to cross-fertilization between the different approaches and even direct collaboration between researchers. The benefits of such a collaboration can actually be seen in a quite extensive and still growing body of research on what have been called “authorities on management”, i.e. management education, consulting and the media (see above). While work on these started earlier, a large-scale, EU-funded project on the Creation of European Management Practice during the late 1990s helped combine and significantly expand the extant research by bringing together management scholars with historians. Together they studied the development of these “authorities” and their effect in different countries and over

time—loosely grounded in neo-institutional theory (for an overview, see Engwall & Kipping, 2006). Their research generated a large number of publications and, more importantly, served as a catalyst for subsequent and ongoing efforts resulting in many workshops and thematic streams/sub-themes at conferences leading to additional publications.

Our paper has brought into view a quite extensive base of research programs in organization and management theory that employ history in a variety of ways—without necessarily using the term itself. It has also identified a growing number of studies that display what we call “historical cognizance” by considering period effects or historical contingencies. Heeding the above suggestions will, we believe, strengthen, and expand both of these and, ultimately, turn history from what appeared like an outsider status into an integral part of (empirical) research and theorizing in organization and management studies.

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