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From the Guest Editors: The Legitimacy and Impact of Business Schools— Key Issues and a Research Agenda

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This is an appropriate moment to review research into the legitimacy and impact of business schools. It is more than a decade ago that Pfeffer and Fong's (2002) provocative paper challenging the perceived orthodoxy of business school success in the very first edition of the *Academy of Management Learning & Education* was published. The financial crisis, still not fully resolved, has raised significant questions about the role of management education—in particular MBAs (Tett, 2009). Business schools and their role are also central to debates raised by the question “what are universities for and what will they look like in the future?” as new modes of delivery lead to a questioning of traditional models of higher education (Collini, 2012; Barber, Rizvi, & Donnelly, 2013). Influential business school writers call for “creative destruction” without which they suggest the future will look bleak for many schools (Christensen & Eyring, 2011).

We believe that debates about the impact and legitimacy of the business schools (Aguinis, Shapiro, Antonacopoulou, & Cummings, 2014; Alajoutsijarvi, Juusola, & Siltaoja, 2015) need to be developed with reference to debates about its location in the contemporary university. The business school stands at the center of the challenges facing the modern university, and the impact issue is central to these challenges. A key task concerns the changing nature of academic community in a world that expects different outputs from those of the traditional university. Like Delanty (2001), we view the university as a key institution of modernity, albeit increasingly “challenged,” and as the site where knowledge, culture, and society interconnect. Business schools have an important and crucial role to play at this interface.

In the Call for Papers for the Special Issue we invited theoretical, methodological, and empirical papers to examine the problems facing business schools and how business schools are facing these challenges. Particularly in our current context of

global uncertainties, it is important that we explore new ways of theorizing and measuring the legitimacy and impact of business schools and that we examine the consequences of the changes taking place in business schools for business school leaders and for the various stakeholders in the business school upon whom its legitimacy ultimately depends. It is also important to put our current challenges in the context of the historical evolution of business schools and to learn the lessons history offers about the cycle of challenge, response, and consequences in business schools. Here we have in mind Augier and March's (2011) and Khurana's (2007) pioneering historical studies and what they can teach us about the problems we face today, as well as whether and how the challenges facing business schools parallel those taking place in other parts of the academy, particularly in other professional schools. The more developed the debate about how the impact and legitimacy of business schools can be defined, assessed or measured, and empirically studied the more surely we can move to define their role and their central place in the changing higher education landscape.

LEGITIMACY

The concept of legitimacy lies at the heart of much sociological thinking about the creation and maintenance of order and the place of institutions in society (Parsons, 1951). However, many social scientists from different intellectual traditions have contributed to thinking about legitimacy way beyond society as the unit of analysis. Since the 1980s, writing on legitimacy has fragmented to consider populations of organizations (Hannan & Freeman, 1989); institutional fields, (Di Maggio & Powell, 1983); institutions, (Meyer & Rowan, 1977); industries, (Aldrich & Fiol, 1994); and the development of legitimating organizations themselves (Durand & McGuire, 2005). Of course,

our present interest in the legitimacy of business schools can and should take us across levels of analysis. Business schools can be portrayed as parts of an institutional field, as members of a worldwide management education industry, as individual institutions seeking to gain and sustain legitimacy, and as professional institutions shaped by and seeking to influence society.

Clearly there is a need to consider the business school embedded in its social, economic, political, and professional contexts, but through what kind of theoretical and analytical lenses? Here we have a further challenge: An examination of available conceptual and empirical papers on organizational legitimacy indicates a host of difficulties due to variations in conceptual, operational, and measurement matters. In a much-valued and -cited paper, Suchman (1995) attempts a ground-clearing exercise on organizational legitimacy. He notes the definitional problems and the way the two main theoretical approaches to organizational legitimacy—what he calls the strategic and the institutional—operate at cross purposes and talk past one another. Suchman (1995) seeks clarity by trying to order the field through a series of analytical distinctions. Thus we are informed about three types of legitimacy: pragmatic, moral, and cognitive and three forms of legitimacy management: gaining, maintaining, and repairing legitimacy. Suchman (1995: 575) also usefully offers a generic definition of legitimacy “as a generalised perception or assumption that the actions of an entity are desirable, proper or appropriate within some socially constructed systems of norms, values, beliefs and definitions.” All this was helpful at the time, and the impact of these distinctions are regularly cited and often used in subsequent conceptual and empirical work.

But Suchman (1995) also took on a further and bigger challenge, which still remains on the table for any scholar interested in the empirical study of organizational legitimacy: How do we reconcile or offer a synthesis between strategic and institutional approaches to organizational legitimacy? How does Suchman characterize the strategic approaches to legitimacy? Drawing on the writing of Dowling and Pfeffer (1975), Pfeffer and Salancik (1978), and Ashforth and Gibbs (1990), Suchman (1995: xx) portrays the strategic approach as “purposive, calculated and frequently oppositional,” but still by actors embedded in cultural environments. Drawing on the influential work of Di Maggio and Powell (1983), Meyer and Rowan (1977), and Meyer and Scott (1983), Suchman portrays the institutional approach to legitimacy as a product

of cultural environments and symbolic systems. From this perspective there is little role for strategic intent or managerial agency in shaping legitimacy; rather, institutions acquire legitimacy through constraint and convergence from often inert and unstated norms and cultural and societal mechanisms. In fact, Suchman (1995: 577) does not offer a synthesis of the strategic and institutional approaches, but he takes a “middle course” between the two.

However, Suchman’s (1995) attempt at delineating a middle course between strategic and institutional approaches to organizational legitimacy has not lessened the controversy between the two approaches. While as Davis (2010) has argued institutional theory has become the default theory in organizational studies, its detractors cannot be silenced. Hirsch and Lounsbury (1997: 410) attempted a reconciliation of “old” and “new” institutionalism and the action—structure duality, but not before castigating new institutionalism for its “inability to address interest and the generative capacity of actors.” This leads new institutionalism “into the logical fallacy of infinite regression to higher levels of abstraction” (Hirsch & Lounsbury, 1997: 410). The desire to locate actors and action at the heart of legitimation processes while also recognizing that legitimation is an embedded process has also been picked up by Hallet and Ventresca (2006) in their ambition to rediscover “inhabited institutions” and in Fligstein’s project to place social skills at the heart of the embedded analysis of what he calls social action fields (Fligstein, 2001, 2009; Fligstein & McAdam, 2012). Other recent and notable attempts to bring embedded actors into inhabited institutions have come from Patriotta, Gond, and Schultz (2011) and Delbridge and Edwards (2013). To different degrees and in different ways, all these authors offer critiques of the oversocialized and overdeterministic views of earlier variants of institutional approaches to legitimacy.

While we are in skeptical mode, we cannot ignore by far the sharpest publicly available critique of legitimacy, legitimation, and organization. Ralph Hybels’ (1995) critique of the literature at that time was and still is profoundly apposite. We will develop further his suggestion that the way forward for the study of legitimacy should be to ground such studies in detailed observation of human activity over time in our own thoughts. Hybels’ (1995: 241) core critique is that legitimacy concepts and thinking are full of “tautologies and teleologies deriving from insufficiently grounded abstract analyses.” The “tendency for tautology stems from the way that existence tends to spawn legitimacy, which then

increases the likelihood of continued existence" (Hybels, 1995: 242). Hybels continues, "a key problem with the dual concept of legitimacy and institution lies in the fact that both are evident only from the stability of patterns of social relations, yet each is said to foster stability" (p. 242). Hybels suggests this apparent tautology can only be addressed or unraveled by studying the dynamics of legitimacy over time and by recognizing that legitimation is always liable to operate in a contested terrain where there may be parallel and interactive tendencies toward legitimation and delegitimation, again, over time. Above all Hybels (1995: 245) argues we should proceed into grounded empirical studies by recognizing that "since legitimacy is an intrinsically abstract construct, it is necessary always to refer to its existence from the behavior of the people involved." For Hybels this means identifying the critical actors in the focal organization and its various constituencies of stakeholders whose approval is necessary to gain legitimacy. To ground a study of organizational legitimacy this means locating the action in and among the various stakeholders in and outside the focal organization and analyzing the flows of resources and communications between the various parties in the processes of legitimation and delegitimation.

So the study of organizational legitimacy remains a contested terrain occupied by scholars with different theoretical starting points, different definitions, and varying methodological positions. Thankfully, this has not inhibited scholars from attempting empirical studies of legitimacy. It is not possible here to offer a comprehensive review of such studies. Instead, we mention some indicative studies that illustrate some optional pathways of development. Thereafter, we approach the scattered literature on the legitimacy of business schools. Here we find a growing literature cataloguing the contemporary challenges to business schools. Much of this writing is in the form of viewpoint papers drawing upon personal experiences rather than the systematic empirical analysis of business school legitimacy. However, a few large-scale empirical studies of the development of business schools exist, and some of these have used primary and secondary data to chronicle the contemporary legitimacy challenges faced by business schools and how those schools may be responding to them in different eras and parts of the world. Examples of these studies include Khurana (2007), Starkey and Tiratsoo (2007), Augier and March (2011), and Thomas, Lorange, and Sheth (2013).

Before we focus in on the limited number of published studies on the legitimacy of business schools,

we should mention some of the more useful indicative studies of the legitimacy challenges faced by other kinds of institutions. There appear to be few studies of public sector organizations facing legitimacy issues. A rare but perhaps limited example is the study by Hannigan and Kueneman (1977) of how a Canadian public agency lost and then regained legitimacy by refreshing its goals, activities, and profile. More recently, but this time in the private sector, Patriotta et al. (2011) offer a strategic analysis of how a Swedish energy company responded to controversy and challenge to its core purposes. In a study of banks, Deephouse (1996) asks a core question for the neoinstitutionalists: Does isomorphism legitimate? This cross-sectional study offers a stakeholder analysis to explore the negative evaluations by stakeholders of banks who appear to deviate from the norm in terms of strategy. Using the population ecology perspective, Singh, Tucker, and House (1986) offer again a limited cross-sectional analysis of whether quests for external legitimacy depress organization death rates in a large sample of voluntary service organizations in Toronto. And using later variants of neoinstitutional theory, Suddaby and Greenwood (2005) explore rhetorical strategies of legitimation in a Big-Five accounting firm in Canada acquiring a law firm. This study is avowedly temporal in character and demonstrates how and why the challenges of changing institutional logics and innovation influence the potential to legitimate strategies over time. We mention these empirical studies not to imply in some sense they are exemplary, but rather to suggest their indicative character in both methodological and theoretical terms. But our overall message is that there are far more conceptual papers on legitimacy than there are empirical ones. This limitation is also to be found in the literature on the legitimacy of business schools where three subliterations of uneven quality exist.

The three subliterations are first of all a large and proliferating body of viewpoint papers and books cataloguing the educational, research, ethical, and market challenges facing business schools throughout the world after a period of unparalleled growth. Examples of this writing include Pfeffer and Fong (2002), Mintzberg (2004), Ghoshal (2005), Ferlie, McGivern, and De Morales (2010), Khurana and Spender (2012), and Wilson and Thomas (2012). Many of these papers are written by experienced business school professors who have genuine and often credible accounts to offer of the legitimating challenges faced by business schools, but they remain personal accounts about an apparently

massively underresearched terrain. In what follows we give due emphasis to some key research themes and questions that should be examined to buttress or dismantle some of the claims made in these viewpoint papers. Above all we argue for the need in a worldwide industry for international mapping studies to examine key trends in the development, legitimacy, and impact of business schools in different parts of the world.

The second stream of research on business schools is not so much a stream as a small collection of what can be considered exemplary studies of business schools in different eras and economies: Khurana (2007), Starkey and Tiratsoo (2007), Augier and March (2011), and Thomas, Lorange, and Sheth (2013). All these studies in one way or another examine the legitimacy of business schools, but often implicitly rather than explicitly. Only Thomas et al. (2013) explicitly pick up the theme of legitimacy in the chapter headings: "The Business School: History, Evolution and the Search for Legitimacy" (Chap.1, pp. 1–51) and "Business School Identity and Legitimacy" (Chap. 2, pp. 52–89). But all four research monographs can be interpreted as developmental accounts of business school legitimacy and legitimation.

Khurana's historical treatise of the social transformation of American business schools and the professionalism of management is notable for the quality of its scholarship and critical reflection. Khurana argues that business schools have moved from higher aims to "hired hands" and that the early logic of professionalism that underlays university-based business schools was replaced by a managerial logic and then a market logic which subverts the logic of professionalism altogether (Khurana, 2007: 7). The Augier and March book, *The Roots, Rituals, and Rhetorics of Change. North American Business Schools after the Second World War* (2011) is equally scholarly and skeptical and concentrates on the development of U.S. business schools in the period of great growth from 1945 to 1970. The end point of the Augier and March treatise is another challenge to the analytical and value bias of business schools and management education. The Starkey and Tiratsoo book, *The Business School and the Bottom Line* (2007) is able to balance off the U.S. bias in the Khurana (2007) and Augier and March (2011) books by offering an analysis of the development and diffusion of the business schools drawing on a primarily U.K. database, but offering some comparative reflection of developments in the United States. Starkey and Tiratsoo's theme is "more" business and less "school," but throughout there is

much useful data and argument about the educational and research limitations of business schools and the consequential legitimacy challenges arising from those limitations.

The most recent of these four exemplary studies, the book by Thomas, Lorange, and Sheth (2013), *The Business School in the 21st Century*, may well be the most useful pivot point for future studies of the legitimacy of business schools. But why? First, their treatment of legitimacy is explicit rather implicit. Second, their analysis of the evolution of business schools and their search for identity and legitimacy are placed in the context of the modern university and of society. And third, they also provide the most international account yet of the variety of business school forms, identities, and models throughout the world. Thus Thomas et al. build up their arguments about differences between Asian, European and U.S. business schools by examining three sources of difference: institutional difference, competitive difference, and social capital differences. Using publicly available sources, they are able to tabulate various aspects of the three sources of differentiation and demonstrate plausible connections with the patterns of business school development within and across the three regions. Other approaches to assessing patterns of convergence and divergence could draw upon various critical dimensions of functionality. These include patterns of ownership; patterns of financing, scale, and structure; degrees of internationalization, product mix, and faculty mix; and rates of innovation (Pettigrew, 2014). As we shortly argue, this comparative international perspective will be particularly valuable in supporting new research on the legitimacy challenges and responses faced by business schools in different parts of the world.

As many have argued, the story of the development of business schools in the United States, Europe, and many other parts of the world is one of unrelenting growth. According to the AACSB, there are now almost 13,000 institutions offering business education in the world (AACSB, 2011). In the United States, NCES (2013) figures show the proportion of master's level degrees in business has increased from 11.2% in the academic year 1970/1971 to 25.4% in 2011/2012, while the proportion of undergraduate degrees earned in business has grown from 13.7 to 20.5% over that same period (cited in Pfeffer, 2016). A similar pattern of growth is evident within the United Kingdom. From a marginal activity at the university level in the 1950s by a few dedicated institutions, the field of business and management has now become the single largest

area of teaching and research in U.K. higher education. In the United Kingdom by 2012, 1 in 8 undergraduate students, 1 in 5 postgraduate master's students, and 1 in 4 international students were studying business and management, and 7% of the faculty at U.K. universities were employed in business and management departments. For Pfeffer (2016: 3) the scale of this growth "becomes a self fulfilling source of legitimacy because of the reality of that size and growth." Pfeffer continues, "Business schools are legitimate because there are so many of them and they become more legitimate the more they are discussed, described and written about" (Pfeffer, 2016: 22).

It seems perverse that a worldwide education industry with such apparent growth and success should also attract a minor industry of challenge and skepticism from its own professoriate. See, for example, Pfeffer and Fong (2002), Mintzberg (2004), and Bennis and O'Toole (2005). But have the facts kept up with the claims for legitimacy or illegitimacy? We suspect not . . . maybe now is the time to correct the imbalance between viewpoints and available evidence?

Given what we have argued about the character of theoretical and empirical work on organizational legitimacy, what principles might guide the formulation and execution of such needed empirical research on the legitimacy of business schools? We concur with Hybels' (1995) recommendation that legitimacy studies should be grounded, but not just in the sense of the study of action and interaction by the various stakeholders claiming and challenging legitimacy. In a worldwide industry, it is now necessary to conduct international mapping studies to map and measure similarity and variation in the development and impact of business schools (Pettigrew, 2014). Here there are crucial unanswered questions about the extent of convergence or divergence in the development of business schools and the implications of such patterns on the realized legitimacy in different national contexts and with different student groups and accreditation bodies.

Implied in the above argument about international comparisons is the crucial principle of *embeddedness*. This means examining business school legitimacy in its political, economic, cultural, and professional contexts. So context matters, but how much, and with how many levels of analysis? And what are the spatial and temporal boundaries of any contextual analysis to be? (see Pettigrew, 2012). There are, of course, no absolute answers to such important pragmatic questions. Any answers will lie in the theoretical framework guiding the

research and the specific questions being posed about legitimacy.

Some existing writing on legitimacy emphasizes the need to consider it as a process and not just as a state. Here the central questions are about the generation, maintenance, loss, and perhaps recreation of legitimacy. So we need studies of the legitimacy of business schools which not only map the terrain on an international comparative scale, but also examine the processes of creation and challenge to legitimacy where they occur. Such process studies need to marry the principles of embeddedness and temporality. The only way to reveal the interactive effects of multiple levels of analysis on a process such as legitimacy is to expose the connections between levels of context and processes in action over time. So a contextual analysis needs a reciprocal-temporal analysis, which connects a theory of context with a theory of action. In legitimation studies of this kind there will be a key role for understanding the social mechanisms at play in the creation, dissolution, and recreation of legitimacy (David & Marquis, 2005; Pettigrew, Murphy, & Denyer, 2017). These are a challenging set of principles to guide any forward-looking research agenda on the legitimacy of business schools. However, we need to take our recommendations from principles to practice. What kinds of studies are challenging but needed and feasible? We envisage two kinds: The first are indirect studies, which explore legitimacy as a consequence of other perhaps even more fundamental research questions. The second, we label direct studies of legitimacy, which examine challenges to the legitimacy of business schools through the perspective and actions of directly engaged stakeholders.

The celebrated paper by Di Maggio and Powell (1983) on neoinstitutional theory has as its opening sentence in its abstract the crucial and emblematic question of this line of theorizing: "What makes organizations so similar?" (1983: 147). Although from the outset Di Maggio and Powell had envisaged the virtues of examining the extent of homogenization of institutions in a field over time, few institutional theorists with empirical curiosity have actually tested for the extent of convergence or divergence in an institutional field. Where institutionalists have ventured into the comparative mode, the tendency has been to use the comparative case study method rather than, for example, surveying large samples of institutions in a sector or field. And yet the ready assumption of convergence persists and no more so than in writing about business schools where the constraining and homogenizing pressures of

accreditation and rankings are said to be encouraging isomorphism and mimicry (see, e.g., Wilson & McKiernan, 2011, and Wilson & Thomas, 2012). But many other writers have assumed a pattern of homogenization in strategy, identity, form, and activities of business schools. Perhaps the time is now right to carry out large-scale international comparative research to examine the extent of convergence and divergence in the purposes and identities of business schools in different parts of the world to challenge the easy assumption about convergence (Pettigrew, 2014). Having mapped the field of business school development in this way, some sharp embedded questions can then be posed about the legitimation consequences of variations in convergence and divergence in different countries, markets, and institutional contexts. If indeed a strong pattern of convergence is found, the most interesting question then becomes who dares to diverge and with what consequences for legitimation and delegitimation? Indeed, if business school legitimacy is linkable to outcomes such as impact, what are the chains of consequence linking diversity of strategy, form, and activities to variation in legitimacy with different stakeholders? So, one fruitful line for legitimacy studies is to approach legitimacy through the analysis of other crucial constructs, such as the convergence and divergence of business school development in different national and international contexts.

An alternative route is to study business schools as they are directly challenged by stakeholders such as their competitors, customers, accreditation bodies, and national governments. The most tangible and clear-cut form of legitimacy challenge is probably market failure, but we know of few, if any, examples of this. National governments tend to conceal market failure of higher education institutions by subsidy, or seeking to turn around apparent low performers, or by encouraging mergers of the weak with the stronger. Candidate examples of market failure may exist in the private sector world of higher education in, for example, the United States, India, and other parts of the Far East. One of the few published accounts of market failure is the interesting study by Alajoutsijarvi, Juusola, and Lamberg (2014) on what they call the Dubai business school mania and bubble. Collet and Vives (2013) have also used mainly secondary data to examine the rise of European and Asian business schools relative to U.S. business schools in the *Financial Times* Global MBA Rankings, but this is not strictly speaking an exploration of business school legitimacy and its consequences.

Of course, accreditation institutions such as the AACSB and EFMD/EQUIS are important players in legitimation processes, and recently a number of articles and chapters have appeared on this theme (see, e.g., Trank & Rynes, 2003; McKee, Mills, & Weatherbee, 2005; Durand & McGuire, 2005; and Trank & Washington, 2009.) Loss of accreditation by a business school provides an alternative legitimacy challenge to the more complete challenge of market failure. The pattern of accreditation offerings by EQUIS in Europe offers a rich opportunity that has already been taken up by Lejeune and Vas (2014). These authors successfully examined how seven European business schools adapted their purposes, identity, and activities after having been challenged by a failure to achieve accreditation in their first attempt. Since EQUIS offers business schools three decision outcomes in their accreditation process—no accreditation, 3-year accreditation, or 5-year accreditation, there are a number of potential research options that could be explored. These include, as Lejeune and Vas (2014) did, exploring the legitimacy consequences of an initial failure, or in other studies, the loss of a 5-year accreditation to a 3-year accreditation in subsequent cycles, or when an initial 3-year accreditation is moved up to the higher 5-year accreditation at some future point in time. All these empirical options, if negotiable, offer real grounded opportunities to examine the legitimacy challenges faced by different accreditation outcomes. Such studies would enhance our appreciations of legitimation and delegitimation by business schools and offer fresh opportunities to connect up legitimation with various impact challenges faced by business schools.

IMPACT

Impact and legitimacy are intimately related. The *Shorter Oxford English Dictionary* defines impact as “the (strong) effect of one thing, person, action, etc., on another” and “make an impact” as “having an effect.” Positive impact reinforces legitimacy, negative impact or lack of impact reduces it. Impact can be discussed in terms of the two main impacts of business school: research impact and the impact of teaching. Of course, research and teaching are related. Leading schools frequently describe their teaching as “research-led,” although what this means is open to debate as—especially in large undergraduate programs—research is frequently mediated through textbooks.

There is an extensive literature on the nature of management research, where research impact has

tended to be discussed in terms of relevance, leading to an ongoing debate about how much research really “matters.” This has been a significant concern of senior members of the Academy and addressed as a crucial issue by presidents of the Academy of Management such as Hambrick, Pearce, and others. Hambrick’s (1994) argument is that our research matters less than we like to think, that we do not “really” matter much to anyone but ourselves. He attributes this to the amount of time we spend speaking to and writing for each other—for example at the annual Academy Conference and for submission to management journals—and therefore, by implication, how little time we spend speaking to those outside the Academy and beyond the classes we teach. The profession is therefore guilty of a form of narcissism, looking at and admiring, or not, its own reflection, caring little about others’ opinions. This is a view shared by many beyond the Academy, brought into stark relief in a conversation one of the authors had about management research with a U.K. government minister responsible for higher education policy. On being shown a list of the journals we target as our preferred research publication outlets, the minister expressed surprise that he had heard of none of them, except for *Harvard Business Review*! The hidden agenda here in the conversation was why we academics spend so much time writing for journals nobody else reads or cares about.

There has been much discussion of scholarly impact and how this can be achieved (e.g., Aguinis et al., 2012; Ashford, 2013). *Impact* has been defined in terms of research productivity and measured in terms of volume of papers and citation counts. Scholarly impact in these debates means the impact we have on other faculty—in terms of publication in top journals and whether our papers make a difference in terms of being read and cited. Many papers fail the latter test and fade into oblivion. A strong critical theme in this literature is that the most common approach to scholarly impact relies exclusively on impact on one group of stakeholders; namely, other academics and that this is unsustainable. Aguinis et al. (2014) argue focusing exclusively in academic impact is short-sighted and, in the long run, may well even threaten the credibility of our scholarly community. As an alternative, Aguinis and colleagues argue for a proactive strategy based on a pluralist conception of scholarly impact that can be found in engaged scholarship (Van de Ven, 2007), or design science (Boland & Collopy, 2004; Romme, Avenier, Denyer, Hodgkinson, Pandza, Starkey, & Worren, 2015). By definition this

will involve greater engagement with a set of stakeholders that stretches beyond the academic and also changes in our career, publication, and training norms. Accrediting bodies such as AACSB have made a similar point, arguing, for example, for changes in doctoral education.

The question of impact raises important questions about the nature of research. One way of framing these questions is in terms of exchange relationships. Here Georg Simmel’s work is useful. Simmel develops a processual view of reality in which his central concept is exchange. In his classic text, *The Philosophy of Money* (1978) he poses such questions as: How should we conceive of money’s nature and of its essential functions? What other institutions must exist for money to come into being and develop? What have been the major stages in the institutional development of money? Similar questions can be applied to the nature of research, for example, “How should we conceive of the nature of research?” “What relationships sustain good research?” “What happens if we conceive of research as a process of exchange?”

For Simmel, exchange is the fundamental economic phenomenon, not production. This is an insight taken up by economic sociologists who argue that we need to seek explanations of economic activity in terms of exchange in social networks (Zelizer, 2013). If we become obsessed with production to the exclusion of exchange value we run the risk of irrelevance. Our sense of value depends upon “sociation” (Scott, 2010) and value itself, according to Simmel, depends upon something being exchangeable. Exchange functions as a mediator of values (Kanter & Khurana, 2010). It involves sacrifice and reciprocity. In exchange, individuals surrender what they control to gain access to what is under the control of others (Poggi, 1993). Simmel frames individual experience the human “fate” as an ongoing and inexorable dynamic of constraint (“bondage”) and choice, a dialectic of obligation and freedom. In this context, exchange depends upon trust. We have to earn trust by convincing the skeptical of our value, and if we conceive of impact as impact beyond the scholarly, then this depends upon engaging with others in a process of co-production. Co-production is a feature of a number of science disciplines (Nowotny, Scott, & Gibbons, 2001). For example, it has been crucial to developments in microphysics (Galison, 1997), where stakeholders interact in what the author calls “trading zones” (Romme et al., 2015). “The modern university lives amid multiple networks” (Barnett, 2011: 4) and it thrives or declines according to the quality of exchange in these networks.

How impact is to be defined beyond scholarly impact remains relatively undefined and contested. Davis (2010: 180) bemoans a trend in research papers obsessed with novelty, not cumulative knowledge development, and driven by increasingly sophisticated econometrics, so presumably not particularly relevant to any but academic stakeholders. He also suggests that proxy measures of impact are "easily gamed," although he does espouse the importance of the public benefit of research while leaving the vexed question of its definition unexplored. Collini (2012) angrily dismisses the confusion of quality and quantity in measuring impact—"how ludicrous it is to propose that the quality of scholarship can be partly judged in terms of the number of 'external research users' or the range of 'impact indicators'"—demonstrating yet again how difficult it is to define impact in ways that will convince different parties.

Debates about how to demonstrate impact are likely to continue for some time. Those who support the idea of impact would accept that "intellectuals have a responsibility to indicate the implications of their ideas . . . for actual practices" (Barnett, 2011: 3). Bourdieu (2000: 15) is critical of academic thinking ("scholastic reason") that ignores economic and social issues in its claim for autonomy and academic freedom, arguing that such a position "threatens to confine scholastic thought within the limits of . . . the withdrawal from the world." At the very least, the growing emphasis upon the need for impact is evidence of the need for contextualization and a shift from "a culture of scientific autonomy to a culture of accountability," although we would agree with Nowotny et al. (2001: 119) that the latter "is still too reactive and in danger of being interpreted in a formalistic and bureaucratized way." The design challenge for business schools is to develop "cultural and organizational practices that sustain a business school's full practice/science synthesis" and "knowledge products that are unique to business schools" (Rousseau, 2012: 604). If this can be achieved, then we can also achieve Herbert Simon's (1967) ideal of the business school rooted in both science and practice and getting the balance right between the two, meeting the double hurdle of rigor and relevance (Pettigrew, 1997, 2001).

Although there is much discussion of impact and relevance, there is little explicit evidence that management research has had a strong impact on practice. Teaching impact is more easily definable, if only in the exponential rise over the last half century of those receiving a formal management education in business schools. Business schools

have become big business and a leading element in terms of size of most universities' teaching portfolios. Management education has had a major impact on the finances of its university hosts, frequently discussed in terms of its "cash cow" role (e.g., Kirp, 2004; Starkey & Tiratsoo, 2007). There have been a variety of criticisms of the impact of management education, particularly in relation to the MBA, which has been accused of turning/churning out graduates with little insight into the complexities of management practice, leadership in particular (Mintzberg, 2004), and obsessed with finance, analysts, and clever "restructurers" (destroyers, some say) of companies, rather than creators of companies that are built to last. Entrepreneurship teachers counter this claim, and there has been a healthy interest in developing new business ideas into start-up companies, although too often the main motivation here is not to build an enduring business but to get to IPO as quickly as possible. We are well and truly implicated in a world that is, in Davis's (2009) phrase, "managed by finance."

The impact of our teaching has been talked about in a variety of ways, and its effects have been demonstrated in various papers in *AMLE*. For example, business schools have been identified with "responsible" impact (Christensen, Peirce, Hartman, Hoffman, & Carrier, 2007; Knights, 2008) with some suggesting that some MBA degrees might help promote environmental awareness (Slater & Dixon-Fowler, 2010). However, there has been significant criticism of the effects of our teaching by authors such as Pfeffer and Fong (2002) who argue that it has far less impact than we would like in preparing our students for successful careers, and by Khurana (2007) who argues that we have failed to become proper professional schools and have essentially become servants of capital, a criticism echoed by Starkey and Tiratsoo (2007). The MBA has come in for particular criticism, with damning comments made about the skills and attitudes it develops in our students. Khurana (2007) and Ghoshal (2005) argue that it is tainted by a one-sided view of management and humanity, emphasizing economic and financial impact at the expense of the social and cultural.

Not everyone shares this criticism. Indeed some leading figures in the business school world argue that it is precisely this mind-set that the business school mission should emphasize. In perhaps the most extreme example of this defense of the business school, a leading dean (Hubbard, 2006) argues that the main skill the MBA inculcates in students is

the ability to value companies and apply new creative finance instruments to extract financial value from them. In the period after the financial crisis, this defense perhaps rings rather hollow. MBAs and MBA education were implicated in the financial crisis (Tett, 2009; Starkey, 2015). It has been argued, for example at Harvard Business School, that one of main lessons for business schools from the crisis is the need to develop better leaders (Snook, Nohria, & Khurana, 2013; Starkey & Hall, 2013). This debate is ongoing.

Former Harvard President Bok, now at the Kennedy School of Government (Bok, 2013: 4), criticizes both the impact of management theory and of management education, suggesting that it is "arguable the business schools' emphasis on the uses of social science techniques in decision-making and the paucity of faculty members with actual business experience" have had a negative effect on what business schools teach, as has the preoccupation with maximizing profits and shareholder value. Worth pointing out here is that finance research has had a powerful effect on both what is taught in MBA programs and on corporate practice. Wall Street has long been a preferred destination for graduates of top MBA programs. In the era of austerity that has succeeded the financial crisis of 2008 and with which we are still wrestling, it is not clear that this impact has necessarily been a positive or benign one. It is also interesting that the financial crisis led to a searching analysis of the assumptions of disciplines such as economics. Management research has been less engaged (Starkey, 2015). Policy makers have looked to new variants of economic thinking such as behavioral economics as a response to the crisis, but not to management theory (Zelizer, 2013).

Crossan, Mazutis, Seijts, and Gandtz (2013) quote *The Economist* (September 24, 2009) on the fact that leading figures in the financial crisis who were associated with the collapses of leading banks, such as Dick Fuld of Lehman Brothers, Andy Hornby of HBOS, and John Thain at Merrill Lynch were MBA alumni from leading schools: "You cannot claim that your mission is 'to educate the leaders who make a difference in the world' [a core mission shared by leading business schools] and then wash your hands of your alumni when the difference [impact] they make is malign."

Crossan et al. (2013) argue for a different kind of impact, a change to existing education practices focused on teaching functional content, so that education concentrates on developing "leadership character." A leading social scientist, Sennett (1999), implicated

management in what he describes as *The Corrosion of Character* as a consequence of the new financialized version of capitalism that now defines the corporate world. Crossan and colleagues (2013: 286) argue that the antidote to this is an education that develops leaders who want to make "a positive difference in the world tomorrow" by focusing on morals, values and universal virtues (wisdom, courage, humanity, justice, temperance, transcendence). This is necessary, they argue, to counter the aspects of management education that promote individualism and disregard for others with a negative impact on values (Krishnan, 2008). This will require a rebalancing of the curriculum away from finance and economics, which tend to promote self-interested behaviors such as greed (Wang, Mahotra, & Murnighan, 2011; Ferraro, Pfeffer, & Sutton, 2005).

Criticisms of the impact of business school education can also be construed in the light of two main areas in which they are failing to make a positive impact: in developing leaders with character, as is implicit in Crossan et al.'s (2013) argument, and also in that they are failing to enhance experiential learning (Kolb & Kolb, 2005), which can help make students more willing and able to reflect upon the reasons that underpin their attitudes and behaviors and to be more self-critical. Kolb and Kolb (2005: 209) point out that any moves in this direction would require significant rethinking, institutional development by business schools, and different faculty groups to create a more benign "learning space."

a holistic program of institutional development that includes curriculum development, faculty development, student development, administrative and staff development, and resource development . . . coordinated around an institutional vision and mission to promote learning.

One of the most interesting approaches to changing the way we think about business school education, and thus, how to promote a more satisfying vision of impact in scholarship and teaching is *design thinking*. The philosophy of design fits well with the vision of impact developed by Augier and March (2011), who discuss the evolving purpose of the business school through the lens of business school relevance, linking this to the broader debate about the purpose of professional schools: medicine, engineering, education, public policy, and administration. Impact, contribution, and legitimacy go

together, and a key challenge facing the business school is twofold: the development of an educated professional management cadre to meet the technical needs of a technology-driven economy and the development of professional managers with a sense of management's social role beyond technology alone, for example, in terms of a sustainable and humane society and the social integration that this requires. There are strong echoes here of Simon's (1967) ideal of the business school rooted in both science and practice. Dunne and Martin (2006) argue that a design approach requires an open-minded approach to problem definition and to the definition of the role of management. Design philosophy is based on thinking broadly about challenges and specific problems, recognizing the vital importance of developing a deep understanding of users in addressing management challenges, and recognizing the potential contribution of the ideas and the knowledge of others. Dunne and Martin (2006: 514) spell out the implication for the MBA:

MBA's have to learn to listen to other people and understand their reasoning process. Not spend their time saying "Their reasoning process is different than mine; therefore it is wrong; therefore, I must stomp it out." That would be the traditional MBA approach.

The crisis of management education, Dunne and Martin (2006) argue, is that we have allowed the field of management to disintegrate into a complex of narrow disciplines that in their own ways oversimplify the complexity of the management task. The impact of this, from a design point of view, and from the perspective of leadership character (Crossan et al., 2013), is education not fully fit for purpose, and that runs the risk of being perceived as increasingly lacking in legitimacy.

Impact can also be looked at more generally. One important strategy for demonstrating the impact of business schools is to look at how they deliver value to local and regional economies (Chartered Association of Business Schools, 2016). Here value is defined in terms of economic development, such as Lancaster University Management School's contribution to job creation. The benefits identified include the direct benefits of employment and investment and the contribution of students to the local economy. University leaders also stress the economic benefits of business schools in terms of the influx of capital from international students. More generally, universities justify their role in

terms of the benefits from research collaborations with business and the commercialization of ideas generated in universities. In this regard business schools claim a unique intermediary role at the interface between business and universities, particularly in terms of improving productivity and innovation and in supporting start-up enterprises. It is also suggested that business schools have a particularly important role to play in supporting social enterprise. Indeed, one might even suggest that they will come to define the social enterprise role as central to their nonprofit mission and their role in a public university.

Finally there is the question of what the growing concern with impact means. Pettigrew (2011: 348) sees this as a major and necessary response to a changing context, a major shift in the "recognition of the complex interactions between multiple stakeholders in the research process and a more contested landscape for evaluating the quality and relevance of research processes, outputs and outcomes." Impact needs to be considered from multiple perspectives, and Pettigrew (2011: 350) quotes work done for the U.K. Economic and Social Research Council (Meagher, 2009) that identified five categories of impact: instrumental, conceptual, capacity building, cultural change, and enduring connectivity. The research agenda that flows from this is to better understand how policy and practice impacts occur and how they are driven by research. Building on the ESRC work, Pettigrew (2011: 351) identifies a number of key factors that appear vital for impact generation:

- Established relationships and networks with user communities;
- Involving users at all stages of the research;
- Well-planned user engagement and knowledge exchange strategies;
- Portfolios of sustained research activities that build reputation with research users;
- Good research infrastructure and management support for user and knowledge exchange;
- The involvement of intermediaries and knowledge brokers as translators, amplifiers, and network providers.

Of these, and supporting the previous exposition of the importance of exchange relationships, the most important factor was the existence of networks built upon strong relationships with research users. This view is supported in the AACSB (2008) report on impact, which emphasizes the potential importance of better engagement between business school faculty and their stakeholders, actual and, more important, potential. The argument is not just about shifting the

emphasis from the ultra-academic to the more applied. This would reduce the debate to an unhelpful and competitive either/or. As Pettigrew (2011: 353) concludes, the real issue concerns the quality and the functionality of the knowledge production process and "extending [the] boundaries of knowledge production processes will create [better] conditions for greater scholarly and policy/practice impact."

Given what we have argued and the nature of existing theoretical and empirical work on impact, what issues and principles might best inform future research? One way of focusing this is by asking the question: What would happen if business schools were to disappear? What differences would be noticed beyond the business school itself? These questions are posed by Kalika, Shenton, and Dubois (2016) in justifying the "Business School Impact System" developed by the European Foundation for Management Development. Their starting point is the perception that business schools face growing legitimacy challenges and possibly major restructuring to cope with a changing context. The "crisis" facing business schools is, they argue, both financial and ideological. Their analysis focuses mainly on the regional level, and they identify four main categories of impact: financial, economic, intellectual, and corporate social responsibility (CSR). This definition of impact provides an interesting and useful starting point for future research. We need empirical studies which are national, regional, and international to map the impact effects that construe impact as a multidimensional phenomenon using constructs such as: financial impact; economic impact; CSR impact; impact on local, regional, and international levels; societal impact; and reputational impact. What we have in mind here is large-scale studies that treat impact as a variable, the theorizing of which requires further development and whose empirical effects need to be charted, not least in terms of bolstering the legitimacy and reputations of business schools.

The research we have in mind is both macro and micro. We tend to discuss business schools as a relatively unitary phenomenon with debates framed by a notion of what a top business school comprises that reflect the practices of the current elite schools. This is understandable, as the league tables (ranking lists) of business schools tend to be relatively stable at the top end and dominated by this elite group. Elite schools are atypical in their ability to control their own destinies. Large endowments give them far more degrees of freedom than the average school and far less resource dependence upon fickle

markets and fluctuating higher education policy. We need more research into the diversity of business schools and their local and regional impacts, as well as the resource constraints within which they operate to gain a more complete picture of the diversity of the business school world, what is working well, at what level and which aspects, and, most important what is sustainable in our complex and ever-changing environment. We need macro international comparative studies to map the terrain of impact and to demonstrate what we assume are the diverse accomplishments of business schools as institutions. What dimensions of impact are being discussed and used in different institutions in different parts of the world that have varying market positions, reputations, sizes, and even geographical strengths and weaknesses? What are the consequences of impact dimensional choices on the image and performance of the business school?

Microlevel studies are required of the ways in which the management philosophy of schools and local factors affect the orientations and identities of faculty in shaping their choices about whom they engage with and why. In our experience, these are key variables, impacting both the capacity for having impact beyond academia and the willingness to make this a core element of school strategies. Such microstudies would illuminate the key choices of orientation and identity made by individuals and groups of faculty, as well as the pivotal role of deans and the different leadership orientations in senior faculty that are embedded and enacted (or not) in school cultures. We have emphasized the context as one in which there is an increasing demand for impact from within and without the business school and the university. We need to understand better what faculty attitudes to impact are and how they affect their working routines. What importance do faculty attribute to doing impact-related work? We suspect that this is only a minority concern which, if our sense of the changing context is correct, can only create a greater divide between faculty attitudes, a preoccupation with publication, and the expectations of external stakeholders. Where are the models of best practice and the diversity of practices that generate impact? These are likely, we propose, to be based on working relationships and networks with user communities, based on notions of exchange beneficial to all parties, and adopting a process of co-production between these parties.

Is it possible to conceive of an engaged and impactful professoriate? Presumably such individuals would demonstrate a certain level of

engagement with stakeholders in the private and public sectors as a necessary prerequisite for the co-production of knowledge. This co-production of knowledge hypothesis also needs testing: Do those who are well engaged have a higher probability of co-producing and those who co-produce have a greater chance of delivering policy and practice impact? Co-production depends upon accepting engagement as a necessary driver of research and raises questions about other research drivers: Is the work theory driven, method driven, issue driven, or phenomenon driven, and what are the implications again for engagement and subsequent impact of these different drivers? We are presuming that engagement is the necessary stepping stone to—and the most important enabler of—impact along the various dimensions that can be mentioned, but this needs to be subjected to critical data-driven analysis.

An important issue here is the unresolved question of whether business schools are to be considered as professional or social science schools. The impact agenda implies professional schools, but the extent to which faculty espouse this identity or enact it in their practices is not clear. We suspect that a majority of faculty might even subscribe to a social science rather than a professional ethic: knowledge about business and management rather than knowledge for business and management. This is an important question that merits examination in the context of large-scale empirical study and in micro contexts.

As authors from the United Kingdom, we take the liberty of making our final point with reference to our own national context. The United Kingdom leads the way in terms of making the impact of universities and business schools more publicly accountable, and in therefore raising the profile of impact as an increasingly important strategic issue. This process has been driven by government-initiated periodic (every 5–6 years) Research Excellence Assessments and the Research Excellence Framework (REF), which is key focal point of university research activity. The recent Stern (2016) review of the REF is an important stock taking of the benefits and the future of this process, and while supporting the principle of the need for accountability, argues for a fundamental change in attitudes to research, which Stern argues has become increasingly dominated by a drive toward safe, “publishable” topics and short-termism, with a consequent reluctance to engage in risky or multidisciplinary topics. Stern (2016) argues that we need to find ways to encourage researchers to explore big or fundamental problems as a priority,

and that it is in all academic faculty’s interest to be able to demonstrate the impact of their work. Impact is broadly defined as socioeconomic; impact on government policy; on public engagement and understanding cultural life; impacts outside the field and impacts on and through teaching.

Again, we need to have a better picture of the business school landscape to discover where and how aspects of this research agenda are being best conducted. Where are the exemplary cases of the long-term complex interdisciplinary management research projects, and how is this kind of research to be nurtured and valued in a context where the short term and the quickly publishable tends to dominate? How are user-engagement and knowledge-exchange strategies best developed, and what constitutes the best infrastructure to research infrastructure and management support for user and knowledge exchange? Where are the champions of this approach, and how can they have more impact on what we do? Who are the key intermediaries and knowledge brokers to support this process? Which firms and other organizations beyond the business school and the university are key players in developing impactful relationships and networks? What and where are the kinds of trading zones that exist to bring researchers and practitioners together?

We wonder, in finishing our review essay, if engagement and impact will lead to a new image and scholarly identity for business school faculty: that of the engaged and impactful scholar actively pursuing research and teaching, with a multidimensional orientation focused on phenomenon-driven research as distinct from theory- and method-driven research, committed to both scholarly and policy or practice impact, rigor, and relevance.

STRUCTURE OF THE SPECIAL ISSUE

This Special Issue brings together research and essays on legitimacy and impact. In the first article, Benson Honig and Ying Hong investigate the vexed question of business school faculty salaries as a focus for discussing issues of legitimacy. They embed their analysis in human capital theory with reference to knowledge and value impacts to discuss important drivers of how business schools compete to attract and retain the best faculty. This is one of the most important challenges faced by business school deans. Academic achievement via publication is a key driver of reputation and salary and is a critical factor in rewarding academic prowess. The authors note a shift in faculty constitution

toward a mix of research "stars" and other faculty responsible for day-to-day operations and instruction. How human capital, reputation, and legitimacy operate in the marketplace is a key consideration in business school strategy. Publications rather than industry/business experience impact individual compensation. The authors cite Bourdieu to emphasize the institutional factors that tend to maintain the status quo and also decrease incentives for innovation.

In the second article, Julian Birkinshaw, Ramon Lecuona, and Patrick Barwise examine the impact of academic research in the context of debates about a relevance gap between management research and management practice. The question of impact is a major concern and also important for legitimacy in the view of the end users of business school services, such as students, employers, and funding bodies. The authors discuss the important role of bridge journals as a major intermediating factor in making management research more visible and accessible. They identify key characteristics of the papers that appear in these journals that serve to enhance their role in overcoming the "lost in translation" problem that besets management research, focusing on the citation trail that marks the trajectory of knowledge from academic paper to bridge journal. They suggest three types of paper that fulfill the bridging role: beacons, walking sticks, and brooms. These require a particular skill in composition while retaining their academic quality. They also tend to be inductive or theory-based rather than deductive, so their arguments resonate more with managers than the normal academic paper.

In the third article, Annie Powell-Snelson, Johanne Grosvold, and Andrew Millington examine business school legitimacy through the lens of education for sustainability. Using fuzzy set analysis, they examine how sustainability becomes coupled with or decoupled from mainstream business school activity. The context for their argument is a growing demand for business and business leaders to move beyond only focusing on the profit motive and issues, including issues of management education, relating to the financial crisis of 2008. The authors examine the strategic and organizational factors associated with whether and how business schools have embraced a sustainability agenda and whether this reflects an intrinsically motivated commitment to sustainability in practice or is merely symbolic and more rhetorical than substantive. Factors involved include history, size, prestige, competitiveness, resources, and expertise. The overarching question here is do schools

pursue sustainability in pursuit of legitimacy at a time of great change in the business context as a genuine heartfelt commitment or is it merely ceremonial, and therefore, unlikely to maintain legitimacy in the long term? Knowledge and expertise emerge as key factors governing success of a sustainability strategy.

In the first of the essays in the Special Issue, Todd Bridgman, Stephen Cummings, and Colm McLaughlin address issues of legitimacy and impact by revisiting the history of the development of the case method in management education. Their starting point is the view that if we do not understand our history, we are unlikely to escape its constraints. They provide a counterhistory of the case method as a challenge to make us think differently about how it might be used more innovatively. As with the previous paper, the assumption here is that business education stands at a crossroads after the global financial crisis. The authors demonstrate how such crises are not new, referring to debates and developments in the case method in the inter-war period with particular reference to the role of a business school dean (Dean Donham at Harvard) and a leading philosopher (A. N. Whitehead). They examine differing perspectives on how to respond to perceived legitimacy challenges with reference to a pragmatism-innovation paradox. They develop a perspective on the case method as a contribution to reinvigorating legitimacy through encouraging greater reflexivity about what and how we teach in search of innovative business school responses to pressing global challenges.

In the final essay Graeme Currie, Julie Davies, and Ewan Ferlie present a "call" to business schools to lower their walls and engage more deeply and meaningfully with other faculties and departments as a way of building business school impact and legitimacy. Again the context they depict is one of more complex challenges than our current structures and mind-sets might prepare us for. They highlight interdisciplinary research and collaboration, in theory and in practice, as a way of tackling complex multifaceted issues and "grand challenges." The authors identify three phases in the development of the business school involving a move away from a concern with professional knowledge to an internally focused preoccupation with academic impact and legitimacy which eventually became complicit in particular philosophies of research and teaching for particular business interests, reinforced by accreditation and ranking concerns. The call to lower our walls is seen as

a necessary precondition for mitigating the isolationist and restricted position that business schools currently occupy. They suggest that a public interest model, focused on the creation of social value broadly defined, is the best way for business schools prepare for the future.

The research and essays included in this Special Issue demonstrate the range of high-quality thought being put into the consideration of the challenges of legitimacy and impact. They also indicate that there is still much work to be done and many other topics to explore.

The research papers and essays in this Special Issue provide us with a rich repository of thought and possibilities for future research. We very much appreciate our authors' contributions and the opportunity and the support we enjoyed in preparing this Special Issue of *AMLE*. We hope the Issue generates an ongoing conversation about the topics of legitimacy and impact which are, in our opinion, among the most vital facing business schools and our profession.

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