

~~Life and fate: a new perspective on the Free-Standing Company in international
business history~~

The Free-Standing Company: a 'zombie' theory of international business history?

Introduction

Our basic sociological concepts are becoming what I call 'zombie categories'. Zombie categories are 'living dead' categories which govern our thinking but are not really able to capture the contemporary milieu.

In this situation I don't think it's very helpful only to criticize normal sociology, and to deconstruct it. What we really need is to redefine, reconstruct, [and] restructure our concepts.

- Ulrich Beck (Slater & Ritzer, 2001, p. 262)

The Free-Standing Company (FSC) is a theory of international business that comes from the field of British business history. FSCs were—at least superficially—single unit corporations that operated without economies of scale based on vertical or horizontal integration (Wilkins, 1988, 1998). In line with the intention of this special issue to explore how 'change points', chronologies and periodizations in history are problematized (Tennent & Bowden, 2016), this article explores the relationship between historiography and theory development. The purpose of this article is to critique and then redefine, reconstruct, and restructure the understanding of the FSC as a theory. The

central argument made is that the FSC is—following Ulrich Beck—a 'zombie' category that masks and misdirects knowledge of a phenomenon of considerable importance to international business history. Only through critical reconsideration of the theoretical assumptions that lie behind the existing history can further historiographical progress be made. This illustrates a broader point that the theories that paradigmatically frame research design, chronology, and periodization in business and management history, can themselves be substantial barriers to historiographical development. As I demonstrate here, there are substantial gaps in the existing historiography of FSCs (not least considerable uncertainty about the decline of the FSC) that stem—at least in part—from the ways in which the organizational form has been theorized.

The development of research into the FSC has foundered two fundamental problems: first, as I will argue, the “FSC” is a “zombie category” (Slater & Ritzer, 2001) which does not capture the organizational dynamics of the firms it has been used to categorise, but has nevertheless become a paradigmatic frame which has both directed and limited research; and second, perhaps surprisingly given that the FSC concept comes from historical research, it is also an ahistorical theoretical object, with little or no ability to explain change over time.

The paper proceeds as follows. First, I review the historical context in which the FSC is thought to have existed. I then examine the historical context from which the theory itself emerged, and explore how these two historical contexts, one of history, the other of historical writing, has confused the clarity and power of the theory. I explain how the authorial origins are bound up in the writing of

American international business history in the 1950s and 1960s (an intellectual project intimately tied to the performative needs of American business education in the Cold War) and the development of the field of mainstream International Business, and how dominant ideas from that discourse misdirected investigation into the FSC. This analysis is used as a means of conceptual and theoretical critique. In the third section, I outline an ontogenetic (life course) framework for explaining the life-cycle of such firms that aims to revive the FSC as a temporally sensitive theory. I conclude by identifying the implications for the field of international business history.

The importance of being British: the historical context

Between 1860 and 1914, a period of intensifying economic globalization (Kenwood, Graff, & Loughheed, 2013), Britain was the world's main foreign direct investor. It has been calculated that in 1900 seventy-five percent of the international movement of capital was British in origin and that between 1904 and 1914 annual outward capital flows were in the region of £173 million each year (Davis & Huttenback, 1986). By 1913 around thirty per cent of British national wealth consisted of overseas assets, leading one leading economic historian (Edelstein, 1994, p. 173) to comment that 'never before or since has one nation committed so much of its national income and savings to capital formation abroad', and another to note that whether 'these were higher proportions that any country before is less important than the sheer weight of British investment in the world economy' (Pollard, 1985, p. 491). It has been argued the main corporate vehicle for this unprecedented volume of

international investment was the “Free-Standing Company” (FSC), a concept first articulated by the American business historian Mira Wilkins (Miller, 1998; Wilkins, 1988, 1998; Wilkins & Schröter, 1998). The FSC concept is somewhat similar to “born global” firms or “international new ventures” (INVs) as contemporary single unit international businesses have subsequently come to be known (Gabrielsson, Kirpalani, Dimitratos, Solberg, & Zucchella, 2008; Oviatt & McDougall, 2005). The FSC was then a single unit “free-standing” entity that was “born-global” in the international business activities that it undertook.

Though there are conceptual similarities between the categories of FSC and INV, they are (it seems) temporally disconnected phenomena, and largely engaged in very different sectors and business activities. So while INVs are associated with service firms (Rialp, Rialp, & Knight, 2005), research into FSCs indicates they could be found in every sector of the economy—primary, secondary, and tertiary—and were engaged in activities as diverse as mining (Harvey & Press, 1990; Harvey & Taylor, 1987; Mollan, 2009), plantations, forestry and livestock farming (Mollan, 2008; Tennent, 2013), public utilities (Platt, 1977), banking (C. Jones, 1977; Geoffrey Jones, 1998), transport (Boughey, 2009), trade (Geoffrey Jones, 2000) as well as in wholesale and retail (Mollan, 2010). There were many thousands of FSCs (Houston & Dunning, 1976; Wilkins & Schröter, 1998) and yet though they are thought to have largely disappeared from view by the mid-20th century, both the reasons for their decline, and their fate, remain largely unknown (Miller, 1998; Mollan & Tennent, 2015; Wilkins, 1998; Wilkins & Schröter, 1998). The importance of these British firms to the international economy and its development cannot be overstated. Prior to the dominance of

American, European and Japanese multinationals from mid-century onwards (A. Chandler, 1990; G. Jones & Khanna, 2006), it was British firms that dominated the international business environment.

The origins of the FSC theory

Alongside Alfred Chandler, Mira Wilkins was one of the founders the modern discipline of business history. Wilkins early work (Wilkins, 1970, 1974; Wilkins and Hill, 1964) provided a international companion to the work of Chandler, who himself was the series editor for two of Wilkins books published with Harvard University Press (Wilkins, 1970; Wilkins & Hill, 1964). The importance of the ("Chandlerian") paradigm these scholars created is undeniable. In business history it is the orthodox theoretical base of a great deal of scholarship (Fligstein, 2008; Iversen, 2008; Mccraw, 2008; Whittington, 2008). Its influence on Oliver Williamson indicates its value to economics and the field of Transaction Cost Economics, and on the field of Strategic Management is also seen, and is significant and enduring (Iversen, 2008; S. R. H. Jones, 1997; Mccraw, 2008; Whittington, 2008; Williamson, 1981). Remarkably, Wilkins was also one of the scholars whose research into American overseas business was also foundational for the field of International Business (G. Jones & Khanna, 2006).

The intellectual context for Wilkins's earliest research was the American academic milieu of the Cold War. There is a growing body of work which indicates how the way in which management practices were interpreted and

portrayed, and how management ideas were taught and diffused, were part of the ideological conflict of the Cold War itself (Cooke, 2005, 2006; Genoe McLaren & Mills, 2008; Kelley, Mills, & Cooke, 2006; B. Spector, 2006; Bert Spector, 2008). Wilkins's research in the 1960s and 1970s was funded by the Ford Foundation, who commissioned her first book, a history of the Ford motor company (Wilkins & Hill, 1964). This was, of course, the Ford of Robert McNamara, the Planning School, of rational calculation, and the exploding Ford Pinto (Dowie, 1977; Gabor, 2000), an act of corporate malfeasance that Wilkins appears never to have written about. Though not hagiographical or unscholarly, Wilkins research—like that of Chandler—nevertheless lionizes the superiority of American business methods and management structures. This reflects the political and social currents of the time, and the emerging genre of business history associated with the Harvard Business School.

Though Wilkins' work is less well known than that of Chandler, and her book titles (e.g. *American Business Abroad: Ford on Five Continents*) lack the pithy titles and lexical power of Chandler's main works (*Scale and Scope; Strategy and Structure; The Visible Hand*) her work is no less important. It established a dominant narrative for the nature of the internationalization process of American business, which in turn provided a cognitive imprint of considerable importance to the development of various theories of the international business, including those which deal with the modalities of internationalization. It is therefore interesting, and a little ironic, that Wilkins greatest contribution to *British* business history would be to identify a type of firm for which none of the insights drawn from the American experience of internationalization proved

useful in explaining. And, in exploring these firms further here, I hope to advance international management theory in a way that escapes the dominant theories of internationalization which rest on the experience of Cold War American business for their empirical succor and intellectual origination.

In 1988 Wilkins published an article in the *Economic History Review* titled 'The free-standing company, 1870-1914: an important type of British foreign direct investment' (Wilkins, 1988). This article employed what Wilkins described as a 'new typology' to theorize a field of research that was largely atheoretical. Her primary purpose was to explore a gap in the historical literature. In the preceding few years there had been extensive research into British investment overseas which had focused on financial flows, FDI stock, and portfolio investment (for example, Stone, 1977; Pollard, 1985; Platt, 1986).

Supplementing this was a growing literature on the formation and operation of British overseas firms (for example, Jones, 1980; Michie, 1981; Michie, 1983; Turrell and Van Helten, 1986). What this literature lacked, however, was a theoretical explanation that related the historical record to existing and emerging IB theory, notably to the research the stemmed from the path-breaking work of John Dunning (Dunning, 2000), and that of Peter Buckley and Mark Casson (Buckley & Casson, 1976, 2009). And, in addition, to find a theory which could explain the organizational form that British investment appeared to be held within.

The simple beauty of Wilkins's central insight was that the companies into which British investors poured capital generated by the burgeoning industrial economy

in the period between 1860 and 1914 were unlike the large-scale American multinational corporations that Wilkins had spent her career thus far writing about. Instead, there were many of them, they were typically small (or substantially smaller than American multinationals at any rate), they did not operate through multidivisional structures, many of them failed (often soon after creation), they left few archival traces behind (unhelpfully for historians), and—critically—unlike their American counterparts, they appeared to have few if any domestic capabilities on which their overseas activities were based.

Reflecting some years later on the genesis of the FSC concept Wilkins wrote:

My own research had uncovered the historical pattern of US businesses that began at home and then expanded abroad. At the same time, in the 1960s, students of contemporary multinational corporations including Raymond Vernon, Charles Kindleberger, Richard Caves, and John Dunning, for example, were all taking the pattern of domestic first then foreign [expansion] for granted. ... Research on multinational enterprise, historical and contemporary, initially focused on American companies. When historians of multinational enterprise turned to study the path of British multinationals, they at first applied the 'American model' (derived from the prior research) and they realized that many British industrial enterprises conformed nearly to the American pattern: J & P Coats, Lever, Courtaulds, and others began with operations at home, and then went overseas based on their domestic business expertise. From my own research (which had shifted from the history of American business

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3 abroad to the history of foreign investment in the USA), I had become
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5 aware that many British overseas investments carried management and
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7 potential for control over borders. Often, however these British firms
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9 investing abroad did not fit with the traditional multinational enterprise
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11 'model' that I had previously encountered: frequently they did not start
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13 with domestic business operations and then expand abroad based on the
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15 core competencies they had developed at home. The free-standing
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17 company – the company that inaugurated foreign business afresh – could
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19 not have *at origin* [emphasis in original] advantages within the firm, since
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21 it had no domestic operations on which to base these advantages
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25 (Wilkins, 1998, 5-6).
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29 This passage reveals a great deal about how the concept of the FSC emerged. As
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31 Wilkins intuits, the model of *American* business was sometimes found in British
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33 overseas business, but these similarities of form were coincidental, reflective of
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35 that fact that when manufacturing firms internationalize they are rarely if ever
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37 "born global" and are much more likely to have followed a stage approach to
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39 internationalization (Johanson & Vahlne, 1977). Yet Wilkins was evidently
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41 intrigued by the presence of considerable investment that did not follow the
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43 model of American business in mid-century. Wilkins' (1988) article began to
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45 explore the British domiciled firms into which capital had been invested, not
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47 from the perspective of investor behavior, or aggregated capital markets, but at
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49 the level of the firm—where the function of the firms themselves was of critical
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51 analytical importance.
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Wilkins's historical article was certainly inspired by a little known piece by Houston and Dunning (Houston & Dunning, 1976) who identified the basic features of the FSC as being that they typically only operated in one sector of the economy and were 'administratively as well as legally independent – that is, its management strategy was not subordinated to, nor coordinated with, that of a British parent company operating at home in the same industry (Wilkins, 1988, p.262).' Wilkins also drew on the work of Chapman (Chapman, 1985) to explain the ways in which FSCs were embedded in nexuses of social and commercial relationships formed by, variously, company promoters, mining engineers, company directors, trading companies, merchant banks, and the geographical locations of the head offices of the companies. FSCs were domiciled in clusters found mainly in Edinburgh, Glasgow, Dundee and—especially—the City of London (Mollan and Michie, 2012). In these locations they accessed the inputs they required, from raising capital via securities markets, to obtaining financial, commercial, and legal services which were not internalized within the firms themselves. Indeed, Wilkins makes quite clear that in her view most FSCs 'had no company specific experience on which to rely' and therefore 'had to engage existing service sector individuals and firms that both identified the opportunities and furnished the initial supervision (Wilkins, 1988, p.278).' While role of entrepreneurs was critical in establishing these firms, there is considerable causal ambiguity to the nature of their success. But Wilkins also made a number of missteps, which reflect the origins of inquiry.

First, despite the identification of investment groups associated with FSCs, it was assumed that the relatively small size of the individual companies indicated that

they could not enjoy the competitive advantages of scale and scope which American business possessed. In fact, as Wilkins was to partially admit in 1998 (Wilkins, 1998), there were frequently horizontal linkages that connected firms in different but complimentary sectors, and that coordination could occur between companies rather than within companies. In turn this raises an interesting question of divergence between the firm as an economic unit of analysis and the firm as an incorporated company. In the scholarship on the development of the modern corporation (A. Chandler, 1966; Williamson, 1981) it is generally assumed that the boundaries of the firm are coterminous with the corporate boundaries established by legal incorporation. This assumption, however, rests rather with the Big Businesses seen in the US in mid-century and the optimality of that form, which is again another legacy of a body of scholarship formed by and in the Cold War. As Wilson and Thomson hint at, there are other forms of firm where the governance structures may be rather looser, at least in terms of legal entities (Wilson & Thomson, 2009). These 'network-form' corporations (N-Form) may have distributed (rather than centralized) management capabilities and organizational structures and so are less obviously visible (the *less visible hand*, if you will). Here, the boundaries are less clear from a conceptual and legal point of view, but might nonetheless be very clear from an insiders (that is to say, *managerial*) perspective. It is clear that the Investment Groups operated in this way (Chapman, 1985, 1998), but so did networks of mining companies, tied to mining finance houses or firms of mining engineers (Frankel, 1967; Harvey & Press, 1990; Harvey & Taylor, 1987; Mollan, 2009; Phimister & Mouat, 2003). Under this model, then, one of the managerial

competencies of the firm must have been “networked strategic management”, which differs from “portfolio investment management”.

Second, related to the first, Wilkins did not consider to any great degree that the different needs of different sectors would have different coordinative requirements. For example, in the provision of public utilities (something that classical MNEs have never undertaken to any great degree), inter-firm coordination was not necessary because the business model of a single-unit utility provider was based on local monopoly rent extraction rather than economics of scale and/or scope, or the production of price sensitive goods in a competitive market. Similarly, in cattle-ranching or plantation management, there might be downstream processing and refinement which could be integrated into a larger firm, but there was no automatic gain to be had from a larger scale or from greater scope at the level of production.

The issue of business models (Teece, 2010) is therefore also important and does not figure to any great extent in the existing literature on FSCs. Different FSCs developed different business models, which were sector specific and depended also on the stage in the life-cycle of the firm. Indeed, the power of the FSC as a theoretical construct breaks down when, apart from identifying that a firm was not a classical MNE, it is unable to reasonably predict (or, more precisely, retrodict) anything about its operations, structure, relationship to other firms, business model, or how it might proceed in terms of its life course. This stems from Wilkins, whose original insight, though powerful, was a kind of negative categorization—“the FSC is not the American MNE”—rather than trying to explain

the FSC in its own terms. Indeed, from the initial claim that these supposedly small and short-lived firms had no managerial capability stems a raft of scholarship which made this a theoretical assumption when attempting to explain the behavior of such firms in general terms. Here there is also a distinctly American twist on the importance of ownership and control and the shift from founder's capital to managerial capital modes of corporate governance.

In contrast to this transition in American business history, FSCs were controlled without heavy or majority capital ownership, a feat accomplished by the nature and powers of the Articles of Association (which placed management and control in specific hands), the nature of the companies as legally independent entities which meant that if hypothetically they were taken over against the wishes of the management they could would be excluded from the wider network linkages that made the system work (for which they were would be little benefit), and that that the investing public that had stumped up capital were primarily interested in *their own* investment portfolio, and did not buy stock to have a stake in the management. Charles Jones has made the point that addressing the FSC through the theory lens of American Big Business (a la Chandler) makes little sense because the nature and conception of control were very different (and were British) (C. Jones, 1997). This is important because it is an element of the mis-direction. Because the ownership structures of the firms did not conform to what would convey control in an American context in mid-20th century, it was therefore assumed that the FSCs lacked adequate managerial control. That is, there was an anachronistic importation of one historically located construct into another era when it did not apply.

There have, however, been attempts to theorise why FSCs were successful, most notably by Casson (Casson, 1998) and Hennart (Hennart, 1998). Casson in particular has done much to challenge the supposed 'paradox' of the existence of FSCs by arguing that their competitive advantage did not originate in the domestic sphere, so their lack of a domestic presence was not a competitive disadvantage. Casson further discusses the nature of control, arguing that many FSCs were located in equity-raising centers (such as the City of London) for the purpose of capital issuance and legal registration, while the effective control was not there but in the location of the operation of the firm. This issue has long confounded historians. The opaque nature of FSC has left largely unanswered whether the metropolitan headquarters – including the board of directors – had any effective corporate governance or strategic role to play and, indeed, whether it was one of the bundles of resources which led to competitive advantage. Casson examines the role of the headquarters in providing (therefore exporting) resources such as 'technological advice or [some] other intangible service' (Casson, 1998, p.107) as well as information ('embodied in the exercise of control').

The role of the headquarters among firms that exported both technology and information was, therefore, to take an initial entrepreneurial idea – to sink a mine or to build a railway, for example – and refine it. The headquarters teamed up with specialists on engineering, marketing, property law, and procurement to generate a detailed specification which enabled the project to be completed on time and within budget (Casson, 1998, p.108).

Casson was right to emphasize the role of technological change in creating the FSC phenomenon. Although a few FSCs were created before the nineteenth century, the FSC flourished in the aftermath of the First Industrial Revolution. Moreover, this organization structure was especially popular in those industries (e.g., mining, railways), that were created or transformed by the First Industrial Revolution. Casson's insight also has the virtue of explaining why FSCs were closely associated with agglomeration economies, especially the City of London, where entrepreneurs could access both commercial and financial services of the kinds described above (Mollan and Michie, 2012). Casson (1998) analogises the capabilities of the FSC to those required for project management, which explains why, he argues, FSCs tended to be located only in one host overseas country. Furthermore, it also explains why FSCs were often relatively short-lived, because once the project was complete the firm no longer had any purpose, whereas firms which engaged in research and development – for example those in manufacturing – had the capacity to continue to innovate over time. In this interpretation the operations of FSCs were usually tailored to the unique geography and resource endowment of the overseas location which had no exact parallel at home. The home based skills used by the free-standing firms were the general skills possessed by the scientific community and exploited through consulting firms, rather than the specific skills found amongst employees in the domestic industry. This explains why R&D intensive firms expand by replicating domestic operations overseas while free-standing firms did not (Casson, 1998).

However, and this is a considerable issue, the use of International Business theory to explore and explain the FSC left it shorn of the important consideration

of both strategic and operational management and, critically for my argument, long-term management (and by extension notions of continuity and change; i.e., history). Though Casson's contributions to the theory of the FSC are considerable, the *relative* weakness of his contributions are the paucity of empirical evidence and the assumption—in line with mainstream IB theory—that firms are boundedly rational-acting black-boxes iteratively interacting with their environment. The assumption is that the FSC was an empty vessel, with limited managerial capabilities internalized within the firm, and so was unable as a matter of logic to demonstrate any strategic capabilities stemming from its management, let alone anything approaching Teece's *dynamic* capabilities (Teece, 2007), and still further from a theory that could explain or account for the history of these firms in their own terms. Marchildon argues that FSCs were capable of surviving only through isomorphism towards multinational form: 'only those FSCs which adopted some of the strategies and structures of Chandlerian MNEs were capable of competing with them. (Marchildon, 1998, p. 392)'. Yet this imposes further on the history of the firms that were FSCs a theoretical framing which is rooted in the ontology of conventional micro-economics and multinational theories of the firm; theories that were created after the existence of the FSC to explain firms that existed later than the FSC. So, even where it is accepted that the concept goes beyond being a heuristic device, there is a deterministic teleology which projects the FSCs towards MNE form over time.

Sector variations are implicitly incorporated and expected in all historical research into FSCs, as historians tend to deal in *ceteris paribus* conditions

silently as each unique case demands study in its own right. Nevertheless, these sector variations are one of the greatest problems in theorizing the FSC, as life-ways, business models, entrepreneurial origin, and network embeddedness are all likely to be generally different depending (and dependent) on the sector the firm operates in. Chapman's Investment Groups (Chapman, 1985, 1998) and Frankel's Group Systems (Frankel, 1967) are the most obvious examples of this, but the relationship of individual cases to a trajectory is only partially accounted for in the literature and not theoretically elaborated at the temporal level (i.e., it does not move beyond idiographic historical accounts towards a nomothetic theoretical account). Thus the sinews of actual management – causally ambiguous, but actually strangely visible – the technical expertise, risk management, social capital, basic business model, managers and management, strategies and demographics of this structure are not theoretically articulated in any systematic way, nor in a way that aids temporal generalization.

Towards a new perspective

What is peculiar is that the theoretical model of the FSC offers no process of historical change for the organizational unit. In the histories of FSCs, processes of organizational change are narrated; they are not theorised. That is, they form part of the history of the specific firms; but the history of the firms does not feedback towards an understanding of the theoretical object (i.e., the FSC) over time and in time. In this sense, then, the "FSC" as a theoretical object is a "zombie". It categorises but does not explain. This forms one of the tensions for theorisation from history, that categorical investigation does not lead to dynamic

(i.e., temporal and processual) theorisation. The literature on the FSC is empirically rich and theoretically anchored, but it does not lead to further theoretical insight even as the empirical basis becomes deeper and wider, simply because of the atemporal nature of the basic axioms of the theoretical object. So, though there is narrative explanation, there is no generalised sense of how the FSC-structure was replicated in time or how it evolved. Thus the histories are historical, but the theory is atemporal. That is the chief challenge of the theorist who wants to draw on history (Maclean, Harvey, & Clegg, 2016). Historical working must be apart of the theorists repertoire. How, then, to adumbrate a theoretical schema that allows both the history and the theory-from-history to be revealed?

One option would be to dispense with the FSC as a theoretical object altogether, recalibrate the boundaries of organizational populations and periods more tightly, and move on. Such a research agenda would fit to a more general trend in historical work, described as 'the current penchant for a narrower, more archival brand of historicism ... where books [that] focus on, say, fifteen or twenty-year chronological slices are now the norm'. This is a tendency 'to concentrate monographically on individual [cases] ... or on chronologically circumscribed movements (Israel, 2015, p. 4).' This might, for example, indicate that historical enquiry into Latin American public utilities between 1919-1939 should be completely separated from studies into, say, African mining companies between 1880-1914. However, this would be to ignore some of the essentiality and insights generated by the FSC concept; that it does bear to [a] historical experience, especially that of the organization of British international business in

the period 1850-1950. That is, the FSC concepts represents, albeit with limitations, a sense that the historical organization of British international business did not conform to Chandlerian notions of organizational modernity. In the writing of business history (and organization history in general) it is easy to be led to a determinist teleology that our current and historically experienced version of modernity was the only one available, and perhaps (even) the only one to have occurred. However, the FSC as a concept speaks to this theoretical dissonance, where a break with hierarchy, scale, and scope, and the stationary orthodoxies of Cold War era business.

This paper therefore begins to outline and problematise a framework to enable categorisation (and by extension historiographic problematization) of British international business, in order to work towards more comprehensive theoretical accounts. This approach builds on the elements of the FSC-concept that are useful, but develops a much greater sense of temporality and greater weight on polymorphic tendencies in the population as a whole. This model is along the following three dimensions: (1) Organization; (2) Management and Strategy; (3) Time, where (1) and (2) are what I refer to as "Static Conditions" and (3) elaborate the "Temporal Dynamics" of the case(s).

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10 In the tabular outline above, "Organization", and "Management and Strategy", are
11 those characteristics that can be present in any non-temporal account of any
12 business/firm, and can apply to any organizational analysis. That is, at any point
13 in time it is possible to analyse a firm using those categories. The "temporal
14 dynamics" are those aspects of unique cases that change over time, intersecting
15 with the "statics" at any moment in time, which when compounded (i.e., taken as
16 serial) can become a historical analysis by incorporating the temporal elements.
17 To a certain extent, the empirical facts in the box marked "Organization", above,
18 are present in the existent literature, though hardly systematically rendered, and
19 not in a serial form. Nevertheless, in terms of the FSC-literature, we know most
20 of all about the "Organization" of these firms as I have defined it above.
21 Following that, a number of pieces address the issue of Management and
22 Strategy, often coming closer to understanding the strategy of the firms than
23 understanding how they were managed, though there are hints at this
24 throughout the literature, but particularly in the work of Tennent (Tennent,
25 2009, 2013).

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47 Implicitly, then, in the writing of history the authors who have looked at FSC
48 have a temporal dimension, but generally not one that explains the historical
49 trajectory in ways that can be made theoretically applicable more widely. Indeed,
50 a great deal of the literature on FSCs concentrates on the formation of such firms,
51 concentrating on the promotion, financing (Casson, 1998; Hennart, 1998),
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entrepreneurship (Casson, 1994, 1998), and external formation (Stanciu, 2000).

By and large there is no sense of isomorphism (the tendency to become more alike over time) or polymorphism (the tendency to become more different over time) in the population of these firms over time. In what follows I begin to elaborate the temporal dynamics element of the model.

The epoch of formation is most easily identified, and locates the formation of an FSC (or FSCs) in a historical context which might (and often does) indicate external conditions which shaped the nascence of the firm(s). The Life and Fate of the firms (ontogeny) is more problematical, but there are several histories that trace firms over longer periods of time, and do not simply assume that FSCs were short-lived (Chapman, 1998; Gerriets, 1992; Greenhill, 1995; Miller, 1998; Tennent, 2013). However, as viewed from a theoretical perspective, understanding the life and fate of an individual firm in a case-study does not enable a full transition from idiographic (individual cases) to nomothetic (generalizable) modes of enquiry and so acts as a barrier to theorisation (Bryant, 2000; Lyman & O'Brien, 2004). Here, we can observe that in the field of organizational demography /ecology temporal studies are widespread that trace organizational populations over time but no such study has been undertaken which has achieved the same for FSCs. One recent article relating to large numbers of FSCs (Mollan & Tennent, 2015) consciously situates itself in a middle-ground between idiographic and nomothetic studies of organizations in order to explore the impact that taxation had on FSCs. While this article might point to the possibility of serial investigation into the population of FSCs that

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3 *might* in turn lead to nomothetic theorisation, the article itself does not attempt
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5 such a project, instead confining itself to historiographical refinement.
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9 To try to break this problem, I suggest that we need to look much more closely at
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11 the ontogeny (life course) of individual firms and then, as a serial, to gradually
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13 build a empirically grounded organizational phylogeny—a history of the category
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15 or categories (i.e., species and sub-species) as a whole. To do this we need an
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17 expanded conceptual repertoire that that can better incorporate temporal
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19 processes and flows, continuity and change, similarity and difference, and how
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21 within every unique context for an event (that is, "event" as a moment of
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23 historical specificity; for example, both the whole life-course of an organization,
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25 as well as a specific juncture in time) there is a history, both before and after,
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27 prelude and aftermath, which shape the event and our understanding of the
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29 event. In respect of this we should embrace two perspectives: the temporal
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31 notion of 'decay', and the demographic study of organizations (FSCs, in this case)
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33 over time.
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39 As time passes being decays into new being. Here I mean decay not in the sense
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41 or rotting or decrepitude (though, of course, failure is an organizational reality),
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43 but as the passing of ephemeral being. Something like this notion of decay is
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45 described thus:
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51 We know that organizations and institutions exist only in actual people's
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53 doings and that these are necessarily particular, local and ephemeral. We
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55 can, of course, recognize specific social forms, a soccer match, a university
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class, or a family dinner, for example, but each event is produced in time and locality, and decays into the past over the course of its accomplishment. There is no moment when it is decisively there and no place in which it can be found again as the same as it was before (Smith, 2001, p. 163).

Conceptualising historical change as the search for the dynamics of growth (and its naturalistic inverse concomitant, the process of decay) takes us away from the time-static models of organizational change that are based in the *ceteris paribus* and presentist assumptions of micro-economics. These are the foundation for the theories of international business that lie behind the current conceptualization of the FSC. Instead, then, we may see a fluid process of cyclicity and the forward flow of time: organizations brought into being, developing, maturing, and failing. They may be in some essential way similar, but each iteration unique. As each organization fails its capital (tangible and intangible) either obsolesces or is reconstituted. Reconstitution and decay are, then, a dual a process of both continuity and change, and a sinew through which the past experience is connected to the present. By presenting this history *en masse* at the level of the population by incorporating demographic and ecological perspectives, comparable patterns of (dis)similitude can be identified (and therefore reveal at least the potential for generalizability) and how they are tethered to the past and the present, and so exhibit continuity and change.

Insights into these deep structural processes can—perhaps only in tantalising outline—be glimpsed in the existing historical accounts. The process of

organizational reconstitution leading to demographic changes, for example, is partially visibly in the history of the international mining sector in the 1890s when, during an investment boom, hundreds of FSCs spiralled into and out of being through an seemingly endless reconstitutive process through which companies were formed, acquired assets, developed the assets, in some cases (but not all) mined, were broken-up, reconstituted and the process began again. Capital would flow from one company to the next, with one share generated new shares in the new companies (Harvey & Press, 1989; Harvey & Taylor, 1987; Mollan, 2009; Phimister & Mouat, 2003). Given the embeddedness of FSCs into wider organizational contexts, the meaning of this both managerially and in terms of the life-course of individual firms, the wider life-course of networks—and the morphology of the population as whole—is not clear, and more empirical work would be useful. Though it is widespread within research into the FSC to imagine that the companies were short-lived—and many did have a short duration—there were also many others that endured for long periods of time (Miller, 1998; Tennent, 2009). Second, the spiralling cascade of firms in sectors where reconstitution and networked embeddedness was present calls into question where the boundaries of the firm as an economic unit lie, and how duration and periodization might or ought to be applied to such firms. Ian Phimister's work on the role of Edmund Davis in coordinating a network of FSCs, largely but not exclusively in the mining sector, is a case in point. Davis's stratagem was to control dozens of companies via inter-locking directorates for the purpose of creating global cartels (Phimister, 1996). Davis's substantial empire was built over time, and endured for around four decades before his death in 1939. Without understanding the longitude and extensity of the

network as a whole its significance cannot be fully understood. Elsewhere in the mining history literature there are numerous further example of similar N-form organized 'corporations' (in the economic rather than the legal sense of the word) which endured for long periods of time. Mining historians have explored how phenomeon was related to market position, technological factors of extraction, risk-distribution, and the influence of finance (Harvey & Press, 1989; Harvey & Taylor, 1987; Mollan, 2004, 2009; Phimister & Mouat, 2003)–but this has not been aligned to the theoretical development of the FSC, or how FSCs as organizations changed over time, or, indeed, to better understand how 'N-form' corporations were governed or managed. Instead, the focus has been with the distinct historiography of that sector, which though completely understandable, is of lesser use in developing the theory and historiography of FSCs.

Similarly, one explanation of the decline of the FSC in the 20th Century is associated with a transformation from N-form towards M-form modes of organization (Mollan & Tennent, 2015). For example, take the case of Lonrho. Founded in 1909 as the London and Rhodesian Mining Company, it is portrayed as a kind of portfolio investment vehicle for a range of assets largely in Rhodesia (Geoffrey Jones, 2000). Whether it was, in fact, an N-form type firm is not discussed. However, from the early 1960s until c.1980 under the leadership of Tiny Rowland, it expanded via acquisition to become a substantial multinational conglomerate. Many of its acquisitions are identifiably FSCs (Swainson, 1980). This provokes fundamental questions to explore the historical transition ('change points'), the organizational dynamics and management implications of the integration of FSCs into wider structures, and whether what might be

thought of as a 'post-FSC' based MNE encountered different strategic and managerial challenges to the more usually discussed American and European MNEs. Intriguingly, despite only controlling around 17 per cent of the stock, Rowland was able to 'exercise virtual personal control over the firm' because of 'an absence of large institutional investors and thousands of small private shareholders (Geoffrey Jones, 2000, p. 123).' This was most probably a direct legacy of the FSC origins of Lonrho, where this pattern of shareholding was common. Whether this was a general pattern—or merely a unique one—again depends on further work to align nomothetic research at the level of the population with idiographic research at the level of the firm. Nevertheless, the example of Lonrho provides evidence of FSCs decaying into a new organizational structure, that as multiple organizational life-courses terminated, they led to growth in another organization.

Conclusion: the implications for international business history

Mira Wilkins's initial observation that the Chandlerian multinational M-form did not always apply to British international business in the period before 1914 was a significant insight. Following her elaboration of the FSC to define and categorize these firms there has emerged a substantial historiography made up of an empirically rich and well researched literature. Yet, as argued here, the FSC is in many ways a 'zombie' theory—created from an ahistorical negative categorization, and with limited explanatory power, especially to explain the historical trajectories of the firms so defined *after* the period of foundation.

This article has also explored the paradigm from which the FSC was developed as a theory. The foundational work on the FSC was embedded in a world-view that in turn stemmed from historical accounts of American multinational enterprises in the 20th century, and in the context of undertaking that historical research in the Cold War. This has subsequently shaped research into the FSC in subtle ways. In focusing attention on unit based boundaries of the firm, the importance of wider networks has been underplayed. Similarly, entrepreneurship and inception have been emphasized, while management and strategy, and the long-run success and failure of such firms, are under-explored.

The organizational form that was categorized as the FSC appears to have disappeared by the mid 20th century only for a similar single-unit organizational form to (re)appear in the 1980s and 1990s in the form of the International New Venture (Oviatt, B., & McDougall, P., 2005). There are then, perhaps, contemporary lessons that might be learned from the historical experience of the FSC, and an opportunity for international business historians to engage in both historiographical and theoretical development, particularly in light of the burgeoning call for theoretical insights stemming from historical research (G. Jones & Khanna, 2006; Maclean et al., 2016; Suddaby, 2016; Suddaby & Foster, 2017).

Following this, future research should focus on three areas. The first is to establish the full life-course (ontogeny) of FSCs both individually and in serial, in a way that articulates the isomorphic and polymorphic forces at work on the

population as a whole, across countries, across sectors, and over time. Second, there is a need to determine the fate of these largely British organizations in the course of the 20th Century. Once there were thousands FSCs; what happened to them all? Demographic historical research will enable population-level changes to be seen, and will direct researchers to events and periods of change. At level of the firm (idiographically) this will provide problematization for research using the business historian's usual tools: archives and case-studies. And third, more should be done to think temporally about the relationship of idiographic cases and the nomothetic power of the theoretical schema used to explore and explain them.

The final point to make is with respect to the primary role that history can play in developing theory. The theoretical limits of the FSC have been reached. When theories become exhausted in this way—when they are 'not really able to capture the [historical] milieu' (to paraphrase from the quotation at the outset of this article)—they also cease to drive historiographical development. To make progress, then, there needs to be a patient accrual of additional empirical evidence. Only from such painstaking and rigorous historical research can new, potentially competing, theories emerge that better explain the phenomena about which a generalizable proposition (i.e., 'theory') is possible. This article can, and should, be read as a call for more empirical work on the FSC (or whatever name we apply in future to that category or categories of organizations), but also as an affirmation of the importance of empirical historical research as a foundation for international business theory.

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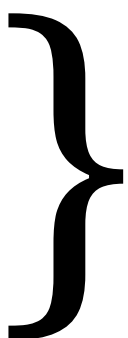
Static conditions	Temporal dynamics
Organization 1. Location of operational activity ¹ 2. Location of HQ ² 3. Location of ownership ³ 4. Location of strategic management ⁴ 5. Network embeddedness ⁵ 6. Corporate governance arrangements ⁶	 1. Epoch and process formation ⁷ 2. Life and Fate (ontogeny): genesis (birth); development (growth); maturation; transformation or decline; death and after-life / aftermath ⁸
Management and Strategy 1. Nature of strategic and operational management ⁹ 2. Unit business model ¹⁰	

Table 1. Towards a model of the FSC: static and temporal factors

¹ Typically for an FSC this would be in one country only. See (Wilkins, 1988, 1998).

² FSCs were mostly headquartered in financial and commercial centers, such as the City of London. This enabled access to agglomeration economies where inputs could be obtained. See (Mollan and Michie, 2012), (Mollan and Tennent, 2015), and (Casson, 1994).

³ The location of ownership is potentially important in establishing the regime of corporate governance.

⁴ (Tennent, 2009; 2013) has challenged the assumption by (Wilkins, 1988; and Casson, 1994, 1998) that FSCs were largely devoid of management. Strategic management might be exercised in either headquarter or operational location.

⁵ There is a substantial literature on network embeddedness and its importance to governance, strategy and management. For an introduction see (Mizruchi, 1996); with reference to British business history see (Wilson, Buchnea, and Tilba, 2017). For the historical implications with reference to FSC governance, see (Brayshay, Cleary, and Selwood, 2006).

⁶ Corporate governance is addressed in the foundational literature on FSCs (see Wilkins, 1998), and for the period of greatest creation of FSCs (pre 1914) see (Hannah, 2007). See (C. Jones, 1997) for a more elaborated account of different governance models of FSCs.

⁷ See (Casson, 1994, 1998) relating to formation of FSCs.

⁸ See (Hannan and Carroll, 2000) for a summary of demographic approaches to temporal organization studies, and (Garnett, Mollan and Bentley 2015, 2017) for use in business history.

⁹ Studies of FSCs that explore strategic and operational management are comparatively rare; see (Tennent, 2013).

¹⁰ See (Teece, 2010) for the standard text on business models.