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Abstract

We argue that the field of international business should evolve its rhetoric from the relatively uncontroversial idea that 'history matters' to exploring *how* it matters. We discuss four conceptual channels through which history matters, illustrating each with a major example. First, historical variation is at least a worthy complement to contemporary cross-sectional variation in illuminating conceptual issues. As an example, we show that conclusions reached by the literature on contemporary emerging market business groups are remarkably similar to independently reached conclusions about a very similar organizational form that was ubiquitous in the age of empire. Second, historical evidence avoids spurious labeling of some phenomena as 'new', and by so doing may challenge current explanations of their determinants. Whereas some firm types today were also present earlier, some types have disappeared, some have appeared, and some have disappeared and reappeared later. Third, history can allow us to move beyond the oft-recognized importance of issues of path dependence to explore the roots of Penrosian resources. We argue that the choices made by Jardine's and Swire's in Asia today, for example, are an outgrowth of strategic choices first in evidence more than a century ago. These would remain obscured absent an historical analysis. Fourth, there are certain issues that are unaddressable, except in the really long (that is, historical) run. Exploring the causal relationship (if any) between foreign direct investment, a staple of the international business literature, and long-run economic development provides one important example. Throughout, we advocate embracing rigorous methods for analyzing small-sample and qualitative data when conventional regression techniques do not apply. That is, we suggest that re-embracing history in the mainstream is not tantamount to sacrificing methodological rigor.

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Introduction

International business scholars know that 'history matters', AIB meetings have had a 'business history' track, or else included business history as a subcategory in a thematic track, since 1998. A simple search showed that the word 'history' was mentioned in 119 articles and notes – or at least one-third the total – published in the *JIBS* since 1990. Yet not a single article was either explicitly devoted to the history of IB, or employed historical data to explore an issue. Only a handful of articles contained longitudinal data covering more than a decade. Although there is widespread acknowledgement that history matters, there is still a search for *how* it matters.

This was not always the case. The first generations of IB scholars appear more engaged with historical data. Among the pioneers of the discipline, Raymond Vernon's product cycle model provided an evolutionary approach to explain the wave of US manufacturing investment in postwar Europe (Vernon, 1966). Subsequently Vernon undertook a vast research project at the Harvard Business School to establish the historical origins of the largest US multinationals. This remains the largest longitudinal source of data on the evolution of historical firms (Vaupel and Curhan, 1969, 1974; Curhan *et al.*, 1977). Vernon produced a cohort of graduate students, including John Stopford, Larry Franko and Lou Wells, whose early work contained a strong historical dimension (Stopford and Wells, 1972; Stopford, 1974; Franko, 1974). Vernon was familiar with Mira Wilkins, the business historian who pioneered historical structures of the multinational enterprise (Wilkins and Hill, 1964; Wilkins, 1970, 1974), facilitated the publication of her early work, and put her books on his course reading lists (personal communication from Wilkins to the authors, 9 September 2004).

John Dunning's first major book, published in 1958, which examined US multinational investment in Great Britain, traced the evolution of those firms back to the nineteenth century. Many of the key concepts of the OLI paradigm were first developed in that book, as the result of empirical research and historical observation, before being formally stated in later work (Dunning, 1958, 2001). Subsequently, Dunning published the first – and to date only – historical estimates of the size of world FDI in 1914 and 1938, which showed its enormous importance in the world economy even then (Dunning, 1983). This research was conducted nearly two decades before the 'first globalization' of the world economy before 1914 became a fashionable research area for economists and economic historians (O'Rourke and Williamson, 1999).

Subsequently there has been important progress in building on these early achievements. IB scholars including Mark Casson and Jean-Francois Hennart demonstrated how transaction costs theory could be employed to explain historical patterns of horizontal and vertical integration in natural resource and manufacturing industries, as well the organizational forms adopted historically to exploit entrepreneurial opportunities across borders (Hennart, 1982, 1986, 1991, 1994; Casson, 1986, 1994). A parallel stream of research by business historians used transaction costs theory to explain the changing modes used by multinationals in foreign markets before World War 2 (Nicholas, 1983). More recently, Kogut has engaged in both qualitative and quantitative historical work in a range of studies, including the international diffusion of management principles and country competitiveness (Kogut, 1993) and the impact of different economic systems on corporate innovation (Kogut and Zander, 2000). O'Sullivan (2000) brought detailed historical research to bear in her major study of the impact of corporate governance on innovation and economic development. Cantwell (1989, 1995) has used patent data to map historical shifts in innovation between countries and industries.

Yet, as a broad generalization, we believe that is fair to assert that systematic investigation of historical evidence has disappeared from the research agenda of most IB scholars, in parallel with a decline in the teaching of history in US business schools (Van Fleet and Wren, 2005). This may reflect the growing strength of the disciplines, especially in US institutions, at the expense of multidisciplinary, topic-based departments such as IB (personal communication of Jean-Francois Hennart to the authors, 13 April 2005). Business historians have continued to work extensively on the history of international business (Jones, 1996, 2003, 2005a; Wilkins, 2001). Business historians have pursued some topics before they rose to prominence in IB research agendas, including the relationship between FDI and portfolio capital flows (Wilkins, 1989, 2004), and multinational strategies in services (Jones, 1993, 2000). Although not the focus of his research, Alfred D. Chandler included multinational strategies in his seminal historical studies of the growth of large corporations (Chandler, 1962, 1977, 1990). His more recent longitudinal studies of the electronics and chemicals industries emphasize the role of multinational strategies in shifting patterns of competitive advantage (Chandler, 2001, 2005).

In IB, there emerged major professional and methodological roadblocks to further interaction with historical evidence. As the discipline matured, there was a growing pressure for a standardized social science methodology, especially multiple regressions, which appeared almost *de rigueur* for an article to be published in *JIBS*. The general pressure for quantification felt in all the social sciences did not encourage deeper engagement with the often patchy or partial data available historically. Qualitative research, including history, came to be regarded as non-rigorous. (In the course of the rest of this essay, we point to several rigorous empirical techniques suited to small-sample and qualitative data whose adoption would facilitate history moving back into the mainstream of IB.)

It was as hard to cross disciplinary boundaries in this field as elsewhere. Business history research was often contained in large monographs, either analyzing the growth of single firms, or pursuing more wide-ranging topics. Such studies represent rich sources of data for understanding the evolution of firm-specific competences in international business. Yet their sheer size and approach make access to non-specialists difficult, especially as many IB scholars – along with other management scholars – were increasingly disinclined to read book-length studies.

We turn now to considering four categories of reasons why history can illuminate conceptual issues of interest to scholars of contemporary IB. The first of these provides a 'base case', as it were. That is, historical variation is at least as good as contemporary cross-sectional variation in illuminating conceptual issues. The second use of history is in avoiding the spurious labeling of some phenomenon as 'new' and thereby understanding it only erroneously. The third suggests that history can allow us to move beyond the oft-recognized importance of issues of path dependence. The fourth suggests that history can help us expand the domain of inquiry of IB. That is, there are certain issues that are unaddressable, except in the really long (that is, historical) run.

For this essay's didactic purposes only, we will refer to historical work very broadly as that which employs data in excess of a decade. We recognize that this working definition is basic, given the vast literature generated by historians about the scope, methodology and purpose of their discipline (Bloch, 1953; Carr, 1961; Gaddis, 2002). Nor do we wish to enter debates among business historians as to the boundaries and methodologies of their subject, whether it is a separate discipline, or a subdiscipline of something else. We opt for a broad definition of business history as an area of academic inquiry concerned with 'the study of the growth and development of business as an institution' (Wilkins, 1988a). We refer to other work in IB as mainstream, without implying anything pejorative to either the mainstream or the historical work thus defined.

Argument 1: augmenting sources of variation: history as a source of time-series variation in the study of business groups

Consider an example that illustrates the manner in which studies of phenomena exploiting cross-sectional (contemporary) variation can be complemented by studies exploiting the much under-used time-series variation afforded by history. The example concerns the study of business groups – collections of legally independent businesses, often extensively diversified, and interconnected by a medley of economic and social ties – that one sees in virtually all emerging markets (and some developed markets).

The work of prominent economists such as Bhagwati (1982) and Krueger (1974), although not directly about business groups, nonetheless shone a light in the 1980s on rent-seeking activities in developing countries. A consensus emerged that business groups, often controlled by families, existed to rent-seek, and therefore did not serve any socially useful purpose. More recently, in the late 1990s, this view of groups was amplified by attention that financial economists focused on a different type of dysfunction – the idea that the families that were the controlling shareholders used the structure of groups to siphon off funds from the minority shareholders (La Porta *et al.*, 1999). Implicitly these were the dominant characteristics of groups on which to focus (see the survey by Khanna, 2000).

Two streams of work, entirely separately conceived, challenge this one-dimensional view of groups and suggest that a much more nuanced view of groups is a better description of reality. The nuanced view includes conceiving of groups as socially useful, rather than purely socially unproductive, entities. They build on early descriptive essays by Leff (1976, 1978) and on Harry Strachan's dissertation at Harvard Business School (Strachan, 1976). Here, we will show that they reached strikingly similar conclusions, one study by exploiting contemporary cross-sectional variation and the other by exploiting time-series variation through the age of empire. And the conclusions are undoubtedly more robust for having been reached in two parallel ways.

Khanna and Palepu (see, for example, 1997, 2000, 2005) suggest that there is, in the jargon of economists, a welfare-enhancing view of groups as compensating for the poorly functioning markets within which they typically operate. For example, when it is hard to allocate talent to its best use, the internal talent markets that operate among the diverse businesses perform a useful function. This stream is based on contemporary (mostly late 1980s through late 1990s) data collected from 15 emerging markets in Asia, Latin America, the Middle East and Africa.

Meanwhile an entirely different stream of research on this issue was undertaken in parallel by business history researchers concerned to explain the historical development and resilience of European trading companies. There was a large literature on Japanese trading companies, especially the *sogo shosha*, which had tended to assume that they were primarily a Japanese form of business organization (Yoshino and Lifson, 1986). Closer consideration, however, led to the identification of the historical importance of trading companies in many European countries, including Britain, the Netherlands and Switzerland (Jones, 1998). Before 1914 they accounted for a high percentage of trade flows between Europe and developing countries. Intriguingly, their importance did not cease after 1914. They continued to flourish as large-scale trade intermediaries, taking the form of diversified business groups, up to the present day in some cases. This historical experience had long been forgotten, or at least de-emphasized. Guillen (2000), for example, presents a view of diversified business groups as being the consequence of the interaction of specific inward and outward investment policies pursued in the postwar era. Although this may be a factor during the time period of his study, the fact remains that such groups far predate this time period and such public policies.

To investigate this phenomenon, Jones undertook a large-scale research project on the growth and strategies of UK-based trading companies from their nineteenth-century origins until the present day. Initially a large number of multinational trading firms were engaged in trade intermediation between Britain and host economies in (mostly) developing markets. The study included firms such as Jardine Matheson and Swire, which remain important components of the Asia Pacific economy until the present day, and other firms such as Balfour Williamson, Anthony Gibbs, Inchcape, and the United Africa Company (UAC), which were once major regional players – UAC employed around 70,000 in West Africa in the 1960s, and was the largest modern business enterprise in the region – but which for one reason or another no longer exist, at least in their current form.

A striking conclusion from this study was the importance of business groups. A general pattern was diversification from trade to related services, and then to FDI in resources, and processing. Business groups turn out to be more important than one would estimate simply by examining contemporary settings. A classic pattern can be seen in the case of Harrisons & Crosfield. Founded as a Liverpool-based partnership engaged in tea trading, buying tea in India and China and selling it in Britain, from the 1890s the firm opened branches outside Britain in Sri Lanka, India, Malaya, the Dutch East Indies (Indonesia), the United States, Canada, Australia and New Zealand. These branches were usually established to trade in tea, but soon acquired a wider range of import and export business, and began acting as agents for insurance and shipping companies. Tea trading led to the purchase of tea estates in South Asia from 1899 onwards, and then the development of distribution facilities in tea-consuming countries in Britain and North America. After 1903 the firm diversified into rubber plantations. During the interwar years, Harrisons & Crosfield deepened its involvement in South-east Asia through investment in logging in Sabah, while in Malaya it diversified from rubber plantations into rubber manufacture. These tea and rubber plantations were all placed in publicly quoted companies in which Harrisons & Crosfield retained some equity.

The motives for such diversification, and the way it was organized, have many parallels with the emerging market business groups investigated by Khanna andco-workers. The systematic influences included strong internalization incentives arising from asset specificity, uncertainty, frequency of transactions and opportunism. These, in turn, arose from information and contracting problems that underlie transaction costs. For example, Chang *et al.* (2001), in a study of analysts' behavior around the world using contemporary data, show the difficulty of gaining access to accurate, unbiased information on corporate activity around the world. That information was inaccessible in nineteenth-century Britain is also evident from Jones's (2000) description of the evolution of Britain's Companies Act. Under the liberal Companies Acts of 1856 and 1862, public limited companies had no statutory obligation to reveal information, even though it was considered advisable to supply a balance sheet before the annual general meeting (AGM). Only in 1929 was sending a balance sheet to shareholders prior to the AGM made mandatory for public limited companies, and in 1948 the same was required of profit and loss statements. The requirements for private companies were, unsurprisingly, even less onerous.

Evidence of contracting difficulty also abounds. Extreme examples perhaps illustrate this best. Jardine's and Swire's were subjected to forced divestment of their assets by the Communist regime in China after 1949. In only slightly less draconian fashion, India's post-independence socialist government confiscated some prized assets of the Tata Group, India's oldest and most celebrated business house (including airlines and insurance companies). Thus the grabbing hand (Shleifer and Vishny, 1998) of government was evident cross-sectionally today and in history.

British trading companies historically and contemporary business groups in emerging markets operate in environments of scarcity of talent. The responses of the groups are remarkably parallel. Samsung runs a de facto business school, a training center, where they attempt to capture expertise from their various businesses and channel it to other (often very different) businesses. Tata runs the Tata Administrative Services, which seeks to develop an elite cadre of managers who are rotated across, again very diverse, businesses. This de facto business school function is valuable in an environment where the nurturing of commercial talent is in short supply, relative to the demand for it (Khanna and Palepu, 1997). A similar elite corps of managers was evident in Jardine and Swire, recruited initially from particular communities and educational backgrounds. For several generations Jardine Matheson recruited most of its managers not only from Scotland, but from a discrete region of Scotland: the county of Dumfriesshire (Jardine Matheson, 1947). When it began to experiment with recruiting university graduates during the 1930s, it much preferred them to have attended Scottish universities. Swire's, in contrast, recruited from the leading English universities of Oxford and Cambridge. Today's HSBC – formerly the Hong Kong and Shanghai Banking Corporation, founded in the 1860s – traditionally ran its business using a small number of expatriate managers – again heavily recruited from Scotland (King, 1984–1991). That bank continues to run an expatriate corps of highly skilled British graduates who can help manage the worldwide businesses of one of the world's largest banks.

The organization of the diversified businesses of these British trading firms had even more parallels with that seen in contemporary emerging markets. Whereas trading operations and certain agency businesses were wholly owned, diversified activities in ownership of plantations and mines were placed in partially owned firms, which were often floated on the equity market. Harrisons & Crosfields had floated around 40 plantation companies by 1914, with shares in the equity of between 1 and 70%. Equity provides only one link within these 'business groups', and rarely the most important ones. Hennart and Kryda (1998) assert that the establishment and maintenance of trade relationships was the goal, and equity, debt and cross-directorships were the means, in these British business groups. Similar ties hold business groups together today. Indeed, it is not even clear that the equity ties are the most salient. In a contemporary study of Chilean business groups, Khanna and Rivkin (2000) argue that equity ties are not the most salient delineators of business group boundaries.

In much of the older literature the organizational forms employed by the British trading firms in South and Southeast Asia were looked upon with the greatest suspicion. The complexity and costs of interest within such groups appeared costly and inefficient. Outside shareholders looked vulnerable to exploitation compared with the owners of the core trading firms, which were often families (Bauer, 1948). However, the thrust of this historical research was to suggest that there were real benefits from these groups also. They functioned as venture capitalists in countries where capital markets were highly undeveloped. They could recruit far better management than lots of atomistic small firms as they could offer far better career prospects. They facilitated the international marketing of products, and provided a mechanism for spreading information and knowledge between firms. In both cases, thinking of these business groups purely as investment groups drastically understates their true role and function – they perform a wide range of market intermediation functions in the face of an equally wide range of market imperfections. The study of contemporary business groups documents the same patterns (Khanna, 2000) and similar skepticism regarding minority shareholder exploitation (La Porta *et al.*, 1999). The extreme view of the skeptic's school was mistaken then, and is mistaken now. A nuanced approach, emanating from an understanding of the contextual environment, remains the more sensible interpretation.

Consider also the longevity of the business group organizational form. Again, the two parallel studies yield the same conclusion. Jones and others have documented that the European trading companies lasted well beyond the initial circumstances in the nineteenth century that encouraged and facilitated their roles as trade intermediaries, especially the poor state of transport and communications, which gave rise to a high degree of information asymmetry, and the spread of colonialism, which provided a favorable political context for direct investment.

In practice, European trading companies and their business groups proved robust. They survived radical improvements in the information environment – which occurred in stages with the progressive introduction of the telegraph (1860s), telephones, faxes and the Internet. They also survived momentous shifts in the political environment – associated with the end of Empire and widespread nationalizations as in China – and in technological paradigms. The companies frequently 'reinvented' themselves to suit the evolution of context, but always outperforming sensible comparable companies. As sea transportation gave way to air travel after World War II, Swire – which had owned a large commercial shipping fleet since the late nineteenth century – established Cathay Pacific, which remains one of Asia's leading airlines today, still controlled by Swire's. British trading companies such as Harrisons & Crosfield and Inchcape – the product of amalgamations of a number of nineteenth-century trading firms – functioned as highly diversified general trading companies with striking resemblances to Japan's sogo shosha. In the early 1980s Inchcape operated in 44 countries and marketed the products of 2750 manufacturers. Its business group included general merchandising, shipping, port operators, tea producers and manufacturing (Jones, 2000).

It was only during the 1980s that capital market pressures, arising from the growing preference among investment analysts and consultants for focused rather than diversified businesses, resulted in the restructuring of Harrisons and Crosfield and Inchcape into 'core businesses' (Butler and Keary, 2000). Nor was this evidence of a peculiar British aberration. A similar tale could be told about Dutch and other European trading companies (Sluyterman, 1998; Jonker and Sluyterman, 2000). Similarly, Khanna and Palepu (1999) demonstrate that contemporary Chilean and Indian groups respond to dramatic improvements in ambient information and dramatically higher levels of competition, not by disbanding but by reinventing themselves and seeking out newer areas of business. See Figure 1 for a 100-year-plus evolution of India's leading Tata Group, wherein the reinvention of the group over

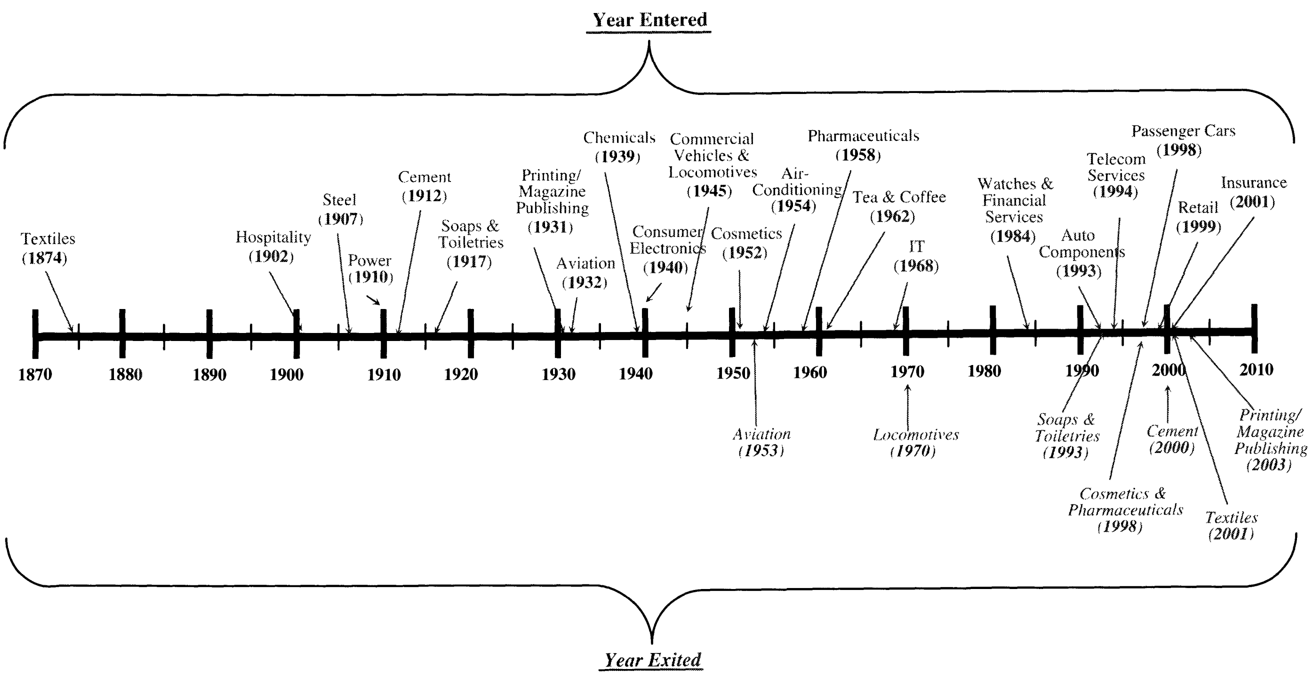

Figure 1 Evolution of the Tata Group since 1870 (Reproduced from Khanna and Palepu, 2005). *Source*: Bombay House, Tata Group.

time is clearly evident from the pattern of exits and (mostly) entries into new lines of business (Khanna and Palepu, 2005).

Thus we see that *intra*-country market failures in an array of contemporary contexts result in certain patterns regarding the structure and effects of business groups. The historical context, focused on *inter*-country market imperfections, also yields the same patterns. Our intention here was to present a 'base case', as it were, where historical variation does as well as does contemporary cross-sectional variation.

In the remainder of the essay, we suggest that history can allow us to go where contemporary sources cannot.

Argument 2: dynamics matter: things change

We are not simply arguing that the primary use of historical analysis is to confirm that cross-sectional analyses are correct. If that were the only role for history, a skeptic could be forgiven for ignoring the subject. We maintain that dynamics and not statics are central. Things change. Firm strategies and organizations are shaped by the economic, social and political environment. Environments change, often and sometimes radically. Relations between variables change. Understanding these changes matters for the issues that concern IB scholars.

Consider the shifts in the types of business institution that have engaged in international business activities. There are types of enterprise today, such as business groups, that were also present historically. However, an important outcome of historical research has been to show that there are some types of business enterprise that have disappeared over time, some have appeared, and some have disappeared and reappeared later. Such historical evidence challenges IB scholars to understand whether and how the variables affecting the correlation between the organization of firms and their environment change.

Take the issue of 'born global' firms, an area of growing concern to IB scholars (Knight and Cavusgil, 2004). The phenomenon of firms that initiate a process of internationalization almost immediately after they have been established, and grow their global operations very rapidly thereafter, is generally assumed to be a new one. In fact, thousands of companies with compelling resemblances to such 'born global' firms were created in the nineteenth century. In the business history literature they are known as 'free-standing firms', a term coined by Wilkins (1988b) to describe the numerous European firms (in particular) that were established with the primary intention of pursuing international investment opportunities. The creation of such firms slowed after 1914, and stopped almost entirely after 1929.

This literature not only warns against the fallacy of believing contemporary phenomena are 'new'. Historical evidence allows the subsequent fate of such firms to be examined. This research is ongoing, but it is already evident that multiple pathways existed. Many nineteenth-century 'born globals' had insufficiently strong governance structures to survive the risks of cross-border business; others grew internationally and then diversified back to their domestic economies; others coalesced into business groups of various sorts; yet others grew into present-day global giants, especially in services and resources. At the current state of research the industrial sector in which such firms were engaged and the nature of the host economy have been identified as important variables in these outcomes (Wilkins and Schroter, 1998; Jones, 2000). Hennart (1994) argues that the relative efficiency of domestic versus international capital markets was the most important factor. 'Free-standing' or 'born global' firms became domestic firms when it became more efficient to seek local finance than to find finance in Paris, London or New York.

There were other types of business enterprise that existed in the past, but have since disappeared. These include government-charted corporations, such as the European East Indian companies of the seventeenth and eighteenth centuries. These not only grew as giant international trading firms, but in South Asia and elsewhere diversified into running countries (Carlos and Nicholas, 1988). Another now almost extinct form was the giant international cartel, ubiquitous from the early twentieth century. By the 1930s such cartels controlled at least 40% of world trade, and also – as business historians have discovered – accounted for significant cross-border flows of knowledge. After World War II such international cartels were largely wound up, although they remained vibrant in numerous services, including air traffic and communications, as well as in diamonds, gold and minerals (Spar, 1994). Understanding the dynamics and performance of such firm types is of more than antiquarian interest for IB researchers. As for the born global firms, who knows when similar forms may reappear?

Argument 3: illuminating path dependence: the roots of Penrosian 'resources'

The history of a firm is now widely regarded as a crucial determinant of its distinctive competences. In the words of Edith Penrose in the third edition of her classic *The Theory of the Growth of the Firm* (Penrose, 1959, 1995), 'one of the primary assumptions of the theory of the growth of firms is that history matters: growth is essentially an evolutionary process and based on the cumulative growth of collective knowledge, in the context of a purposeful firm.' Penrose herself engaged deeply in empirical historical research, notably on the growth of the international petroleum industry (Penrose, 1968). *The Theory of the Growth of the Firm* should have included a chapter containing a historical case study of Hercules Powder Company, a chemical company that emerged from Du Pont, but it was dropped by the publisher to save space, and later published in *Business History Review*, published at Harvard Business School (Penrose, 1960).

Penrose is acknowledged as a major influence on resource-based and path-dependent theories of the firm (Wernerfelt, 1984; Foss, 1997). Yet the 'history' in most of this literature is treated in a stylized fashion with little concern for the complexities of particular historical situations. Nelson and Winter (1982) were explicitly motivated by the inability of the standard neoclassical approach to explain the dynamics of the economic system, although Nelson (1991) subsequently acknowledged that their work had been 'handicapped by insufficient study of the writings of Chandler'. In particular, Nelson emphasized the importance of the 'halting, trial and feedback, often reactive rather than thought-through, process that led to the ways of organizing that Chandler describes'. Subsequently Nelson himself, Murmann (2003), and Raff (2000) among others, have engaged with the Chandler-style complexities of real historical evidence, but they remain a minority of evolutionary economists prepared to go beyond identifying the importance of 'history' to engage with the research of professional business historians.

Understanding the roots of Penrosian resources can help shed light on many conceptual issues. We consider two illustrative examples here. First, we discuss the *raison d'être* of multinationals, an extensively studied issue in the pages of *JIBS*, and demonstrate how historical evidence might contribute to the scholarship on this issue. We then turn to the issue of the longevity of resources. Through an example comparing two British firms in the Far East – Jardine and Swire – we argue that their initial competences, manifest in the choices they made a century ago, still matter in interpreting their actions today. Without a historical analysis one would be hard-pressed to identify the roots of their contemporary strategies.

Consider, then, the scholarly quest for the *raison d'être* for multinationals. One oft-debated issue is the extent to which multinationals exist because they are more efficient than are cross-border arm's length transactions involving knowledge transfer (see, among others, Hennart, 1982; Morck and Yeung, 1991; Caves, 1996; Gupta and Govindarajan, 2000; Foley, 2002). For example, Kogut and Zander (1993), building on earlier work that demonstrated the benefits that firms have over markets when it comes to the transfer of tacit knowledge (Hennart, 1982), maintain that multinationals specialize in the transfer of knowledge that is difficult to understand and codify. Firms are seen as defining a community in which there exists a body of knowledge regarding how to cooperate and communicate. Through repeated interactions, individuals and groups within firms can develop a common understanding by which to transfer knowledge from ideas into production and markets.

Some recent econometric evidence suggests that, indeed, multinationals do seem to exchange information regarding patentable know-how more within the firm than do otherwise comparable arm's length actors, and that it is personal networks of researchers within the multinational that appear to facilitate this intra-firm knowledge interchange (Singh, 2004).

On the other hand, IB theorists such as Solvell and Zander (1998) have shed suspicion on this front with their assertion that multinationals are 'not particularly well equipped to continuously transfer technological knowledge across national borders' and that their 'contribution to the international diffusion of knowledge transfers has been overestimated.' As the transfer and diffusion of knowledge is a process that needs to be observed over a period of time, the longevity of experiences offered by historical materials is evidently important in identifying the proximate determinants of when intra-firm cross-border knowledge flows are, in fact, realized. There are specific benefits here derived from the methodology employed by historians in providing a reality check against easy assumptions of linearity. Social scientists often start with the present moment and work backwards. For example, the Vernon/Harvard Business School database on the growth of large US multinationals mentioned earlier took the largest existing multinationals at the time of the project, and then derived a picture of the growth of multinational enterprise by tracing their histories backwards. This methodology was unable to identify the numerous firms that were once large, but 'fell by the wayside' for exogenous or endogenous reasons. In contrast, professional historians, accustomed to study data sources generated at the time, can identify phenomena that had existed or had been important in the past, but which were no longer significant when the research is being undertaken. This methodology reveals discontinuities, if they exist. It also raises the possibility of identifying specific causal mechanisms that can never be reliably inferred from cross-sectional regression studies.

It turns out that business historians have uncovered good evidence on major multinationals with long track records that have often done quite poorly on this front. A new historical study of Ford in Europe shows that for at least three decades after World War II Ford's research and development centers in that region operated with almost no coordination between them, reflecting their embeddedness in their respective national organizations (Bonin *et al.*, 2003).

Historical studies highlight the nature of path dependence in knowledge transfer, or lack thereof, that Penrose discussed. It certainly does not appear to be the case that firms 'automatically' get monotonically better at cross-border knowledge transfer over time. As an example, an archival, multi-decade, study on the relationship between Unilever and its US affiliates shows that the stickiness of knowledge even within such a highly internationalized corporation was not only huge, but for some decades deteriorated through repeated interactions, getting 'stickier' over time. The smooth flow of knowledge that characterized the 1930s declined sharply between the 1950s and 1980s, before improving again. However, there were marked variations between product categories within this overall pattern, and knowledge transfers worked better from the US to Europe than the other way round. Jones (2002, 2005b) shows that an interaction of organizational and external influences helps explain this situation. In particular, the vested interests and psychological attitudes of US managers encouraged them to seek autonomy from the European head office, which felt itself constrained by US anti-trust laws from enforcing more efficient knowledge flows by coercion.

The roots of Penrosian resources sometimes stretch back over a time horizon that can only be uncovered by historical analysis. Absent such an analysis, as our detailed comparison below of Jardine and Swire shows, one would be hard pressed to understand the contemporary choices made by some firms. The issue of understanding why firms might make very different, and long-lasting, choices when faced with broadly similar market conditions is a staple of strategy scholars. Eminences going back at least to Ansoff (1965) and Andrews (1971) have highlighted the understanding of firm heterogeneity as the defining feature of strategy.

Both Swire and Jardine had ostensibly similar backgrounds as British traders in the Far East in the nineteenth century, and remain to the present day ultimately owned and controlled by families resident in Great Britain. They also shared similar experiences, experiencing the chronic political instability of interwar China, and the loss of much of their business in World War II followed by the Communist Revolution. Both rebuilt their businesses from the British colony of Hong Kong after 1949.

Yet the two firms have diverged substantially. Whereas the origins of the families behind Jardine lay in Scotland, the Swires came from the county of Yorkshire in the north of England. Whereas the Swires pioneered appointing Oxbridge university graduates to management positions in 1920s – stressing academic ability and Chinese language skills and an ability to 'thoroughly understand the Chinese' – Jardine was sceptical of graduates, and preferred so-called practical Scottish recruits. The two companies developed quite distinct corporate cultures. Over a long period Jardine sought to recruit 'risk-taking' entrepreneurial types, whereas Swire preferred low-profile 'modest' types. Swire had a long-term aversion to 'making money out of money' – a view of the early Swire family – that Jardine did not mind at all (Jones, 2000) During the 1970s Jardine launched a successful joint venture with the London merchant bank Robert Fleming. Swire pioneered the recruitment of Chinese into management rather than use of *comprador* intermediaries in the 1930s. Jardine followed this route after a considerable lag. In fact, even before 1914 Swire moved to more modern distribution methods in China using sole agency agreements with independent Chinese merchants (Cox *et al.*, 2003).

These differences played out in quite different strategies. Jardine appeared more entrepreneurial, acting as de facto venture capitalist, from the late nineteenth century to the present. Swire took a more evolutionary pattern, avoiding financial activity and moving in an evolutionary fashion from shipping to airlines. The ownership of dockyards and the like became the basis for a real estate business, and distribution and sugar-refining activities led into Coca-Cola bottling, first in Hong Kong and subsequently in the United States. Swire also demonstrated a long-term interest in working with the Chinese. 'As far as she is able China intends to become a manufacturer, first for her own needs and ultimately for export,' a senior executive of Swire advised the management of a British-owned shipping company with which it had long-term collaborative arrangements.

> This development may take two or three decades during which – if Britain is to increase her trade to China – we must go all out in equipping Chinese industry and participating therein. When China becomes a successful industry economy we shall reap our future reward in her higher standard of life, and our increased sales to her of our quality goods. (Jones, 2000)

The roots of Jardine's and Swire's strategies emerged early in their corporate histories. These early choices powerfully conditioned their subsequent evolution. To understand a particular firm strategy it is necessary to understand the specific historical conditions in which fundamental decisions about how to run a firm were made, and to identify the reasons why the firm will find it difficult to change these decisions. Pettigrew (1992) observes this in his advocacy for an understanding of temporal interconnectedness. 'The past is alive in the present and may shape the emerging future' (p: 10)

There are several possible approaches to making the use of such historical evidence consistent with current IB scholars' (appropriate) sensibilities concerning methodological rigor. Ragin (1987) pioneered techniques from Boolean algebra to facilitate systematic comparison of small numbers of cases, such as the two firms Jardine and Swire. Suppose a researcher wishes to attempt to explain the relative success of one of these firms over the other at a particular point in time. She is faced with the issue that there are literally dozens of meaningful dimensions along which Jardine and Swire are different, and dozens along which they are similar. How is one to determine the causal connections between the multiplicity of causes and the performance differences? Ragin's answer, very roughly, is that there is only a certain degree of precision with which an answer can be obtained from a particular set of cases. To some extent, judicious selection of a cross-section of cases, or perhaps time-series variation of the cases (that is, Jardine and Swire at different points in time, in the example above), can improve the precision of the answer, but often not entirely. Thus an answer might be as imprecise as: we can conclude that the differences are not due to 'Scottish origins', or that we know that the presence of 'Scottish origins' positively affects relative performance in the presence of 'China location', but not otherwise. The answers are typically reported in what Boolean algebra refers to as a 'truth table'. (For other examples, see Kogut's methodological essay, 2001.)

Without going into further detail, various other approaches have been proposed in recent years to rigorously investigate small number situations not employing regression analysis (Ghemawat, 1997; Khanna *et al.*, 2000). In short, there are already some methodological toolkits available to employ historical data.

Argument 4: expanding the domain of inquiry: FDI and development in the really long run

Besides using history to uncover the essence of a firm, historical analysis can add value by uncovering the long-run effects of particular choices. Consider one important example – the impact of international business on development. Given the serious and apparently growing inequality in world incomes, and the heavy emphasis placed by policymakers on the role of foreign direct investment (FDI) in stimulating growth, it is remarkable that the literature on FDI and development remains so inconclusive (Fortanier, 2004). Among the many reasons for this situation, the restricted methodologies of IB researchers and the disinclination to engage with accumulated historical evidence on the economic – and political – impact of multinationals must rank as a significant contributory factor.

There are major benefits to be achieved from incorporating historical evidence into debates about the long-run effects of countries opening up to foreign investment. This is an extensively studied topic today. For example, Sachs and Warner (1995) catalogued a series of countries that have 'opened up', as evidenced by a series of objective economic indicators. Several authors have investigated the causes of opening up and its various effects on industrial organization and the operation of markets for capital and labor (see, for example, Ghemawat *et al.*, 1998). However, these studies take a 'recent history' view of the world, in most cases investigating changes over a few years or, at most, a decade. Several of the longer-run effects simply cannot be studied using such research designs.

Yet many of the effects associated with multinational activity are most likely to manifest themselves over the longer haul. Consider the raging debate today about the effects on the contextual environment of multinationality (Broad, 2002) – for example, to what extent are multinationals harmful to the environment versus helpful to it? *The Economist* (29 Jan – 4 Feb 2000 issue), a popular London-based newsweekly, published a cartoon demonstrating the shifting opinions on this issue over the past four decades. The cartoon suggested that society's attitude to multinationals had evolved from fear (1970s) to nonchalance (1980s) to exuberance (1990s) and back to fear (2000). This time-series variation in societal attitudes is arguably due to inappropriate extrapolation of the immediately accessible empirical evidence as a generalizable truth. A closer approximation to the real effects of multinationality could come from a longer-run historical study (over several decades, or even centuries).

Any such long-run historical evidence on the impact of multinational investment is likely to show multiple and changing factors and processes. IB scholars are likely to feel frustrated at such a world, where 'everything appears linked to everything else'. However, there are several suggestions of ways to address such complexity. For example, the sociologist Abbott (2001) has explored means of understanding networks of intertwined processes. Khanna and Palepu (2004) experiment with examining sequences of actions and events over long time periods to search for patterns in them, not discernible to casual introspection. They undertake a quantitative analysis of 'histories' of 134 companies, over the past five decades, in several emerging markets. The analysis rests on coding the histories into 'event time' strings. That is, significant events in the histories of each of the companies are coded with particular labels.[1] The spirit of the analysis was to look for statistical patterns in the sequence of events. As a simple example, do domestic events (say, an organizational change in the home market) precede international events (e.g., raising capital overseas)? Each event in an event sequence was represented by a single letter, and then 'distances' between the event sequences were computed by computing a 'string edit' distance for each pair of strings.

There are more fundamental methodological issues here. As the historian Gaddis (2002) has argued, historians typically do not approach problems by seeking to distinguish independent from dependent variables, but ensure the interdependence of variables as their interconnections are traced over time. This has a greater methodological similarity to natural sciences such as astronomy and geology than to social sciences. Whereas the latter usually assume a world of linear and therefore predictable phenomena, historians – like natural scientists – have methodologies that accept and explore chaos and complexity.

From this perspective, historical evidence is well positioned to explore an issue such as the long-run developmental impact of FDI. As an example of an historical study that sheds light on the channels through which FDI affects development, consider how China and India responded to their forced, at British gunpoint, opening up of their economies in the mid 1800s. Rhoads Murphy (1977), in *The Outsiders*, points out that, whereas the British sought to use the treaty port model in both China and India, the outcomes were drastically different in the two countries. 'Treaty ports' (or their equivalent, as this is a term usually applied to Chinese ports) in India – Bombay, Calcutta, and Madras – became commercial centers of the country and radically affected the nation's development. Treaty ports built in China – constructed after the Indian ports and after others in Colombo, Rangoon, Bangkok, and Singapore – had a more circumscribed effect. The Chinese managed to isolate the effect of the ports to their immediate geographic vicinity.

Murphy goes on to argue that this differential effect is because China had a functioning, vibrant market economy whereas India was in economic and political shambles in the twilight of the Mughal era. Taking this interpretation at face value, this is already interesting because it validates the viewpoint of multinationals as arbitrageurs (Foley, 2002), a viewpoint that has become somewhat discredited today as capital mostly flows among rich countries rather than from rich to poor (Easterly, 2001).

But the example is more interesting, because it sheds light on the channel through which the FDI had the alleged effect. In particular, an elite developed in India that was willing to embrace the British ways – if not the British – and use them to try to develop India. There was no 'demand' for such an elite in China, satisfied as the latter society was with its indigenous trading system and extensive intra-country commerce.

The Indian pattern reveals further regional and ethnic complexities. The first elite group to respond to the British was the tiny Parsee community around Bombay. They were extremely active in developing a modern cotton textile industry by the second half of the nineteenth century. Their entrepreneurial success has been variously described as the result of close relations with the Colonial authorities, 'outsider' minority status, and a 'Protestant' style work ethic (Timberg, 1978). However, during the interwar years, Marwaris – originally a trading community from Rajasthan – began building powerful business groups in Calcutta that began to erode the British commercial presence in the interwar years, well before the end of colonialism in 1947. Marwaris entrepreneurs were more competitors than collaborators with British interests, and whereas the Bengalis might have considered them 'outsiders', the British considered them 'insiders', Meanwhile, the cotton textile industry of Ahmedabad was built by 'mainstream' Hindus who had lived in the region for generations, and who had little relationship at all with the British (Oonk, 2004).

The mechanism of the development of an elite to facilitate widespread foreign influence could not be documented in any of the current attempts to investigate effects of FDI, confined as these are to measure short-run productivity differentials. It was evidently influenced by multiple factors. The European presence in India dated from the fifteenth century, far earlier than their substantive impact on China, which began in the 1840s. India's long tradition of 'absorbing' external influences can be contrasted with China's long tradition of relative isolation.

However, it is difficult to attribute causality to a historical story in the absence of a sensible counterfactual. What would have happened if the phenomenon being investigated had not occurred? What would have happened if the French rather than the British had replaced the Mughals as imperial rulers in India (a very plausible scenario)?

Again the issues are challenging, but the shape of solutions is evident. To some extent comparative historical studies get around this. So too do ingenious general equilibrium models built by economic historians to simulate counterfactuals under some underlying plausible behavioral model of the historical actors. O'Rourke and Williamson 1999 (Chapter 8) specify a computable general equilibrium (CGE) model to study the effect of mass migration from Ireland and Sweden to the US. This is in the context of the roughly 60 million people who migrated from Europe to the New World starting in 1820 (and spurred by the Irish famine of 1845).

What does such a model allow one to do that one could not do otherwise? A historian confining herself to the way history actually played out would not be able to accurately answer the question of the net effects of the migration on either the source or the host countries. The issue to confront is the benchmark against which the realized outcomes should be compared. The option of comparing with the pre-migration outcome does not seem appealing. That would be sensible only if no labor market outcome would have changed in the countries in question in the absence of this mass migration. But plausibly what might have happened is that, if labor did not move, capital would have flooded then-labor-abundant Sweden and Ireland to take advantage of lower wage rates. A CGE model, calibrated to the realized outcomes, would allow one to simulate this alternative possibility, and then use this as a benchmark against which to compare the realized outcome. The catch, of course, is that calibration of the model depends on making some assumptions about the way the world the economy in this example works, typically some assumptions about optimizing behavior of providers of capital or talent in this story. Thus one way to view this is to see the acceptance of simple assumptions buying the possibility of a clear specification of historical counterfactuals.

A comparative approach also gets at the spirit of specifying counterfactuals, although not literally. For example, consider Huang and Khanna's (2003) articulation of the development trajectories of modern China and India. Succinctly, China ultimately embraced foreign direct investment, at the expense of the indigenous private entrepreneur; India made the mirror-image choice. Each country's evolution suggests a counterfactual for the other. Thus the value of China's foreign direct investment should not be compared with a base case of zero, but with what might have been if it had embraced private property rights and nurtured indigenous entrepreneurship.

Discussion and limitations

IB has a glorious tradition of engaging with historical evidence, going back to Dunning, Vernon, Wilkins, Casson, Hennart, Kogut and others. Although IB has developed in other senses – emphasizing quantitative research of the sort that one of us has contributed to actively – it has lost sight of the value of this historical tradition. We aver also that the complementarities between the historical and the mainstream tradition are not now, and never really were, exploited. This essay is an attempt to shine light on these hypothesized complementarities.

Our base case says that historical variation is a suitable complement. In the now-burgeoning literature on business groups, a major study exploiting contemporary cross-sectional variation yielded conclusions similar to a study conceived separately and implemented contemporaneously and in parallel that exploited time-series, historical variation.

To us, this is sufficient reason to treat historical variation seriously as a crucible within which to develop and test theories and ideas. But history offers much more.

History allows us to go far enough back in time both to find historical examples of modern phenomena that we might otherwise misleadingly label 'new' and to uncover the roots of Penrosian resources. In the time-spans considered by most modern studies, a few years or at most a couple of decades, we are forced to take things as exogenously given. What history allows us to do is figure out where these 'givens' come from. Our examples of Jardine and Swire, among others, illustrate this.

Further, history allows us to examine long-run effects of phenomena of interest. If we confine ourselves to researching events in the recent past, we are ruling out the possibility of uncovering effects that only manifest themselves over the longer haul. A legitimate criticism of studying things in the really long run, of course, is that such experiments are inevitably contaminated because the 'treatments' being studied are not the only influence on the outcomes of interest. In the context of our earlier example, one might study the effect of FDI on indigenous entrepreneurship, but the wider the time horizon of interest, the greater the room for other factors affecting entrepreneurship to also change and affect things. The only sensible response to this critique, beyond acknowledging it, is that it suggests that the pros and cons of really long-run research be weighed judiciously.

It is important not to underestimate the scale of the challenge in using historical evidence to inform IB. There is the issue of specifying the counterfactual. Allowance must also be made for differences in the methodologies of researchers. Gaddis (2002), like all professional historians, stresses that historical 'facts' are strongly embedded in interpretation. IB scholars do not need to become professional historians to use historical evidence, but sensitivity to and awareness of the nature of historical evidence are important to avoid naïve mistakes (a crude example might be using a study published in 1980 when an author 20 years later has significantly revised the data or the interpretation).

The British India example is illustrative of this latter point. Some might look at the legacy of British India and say that the civil service, railways and legal code were gifts of Britannia and ultimately outweighed the negative aspects of losing independence. Nehru, in his *Discovery of India* (1946), has a scathing rejoinder to the same 'facts'. Historians interpret the past for the purposes of the present generation. Currently, historians are reinterpreting the nineteenth-century era of imperialism in terms of the building of a global economy (Bayley, 2004). The building of cooperative networks between Western capitalists and resilient indigenous business networks is frequently emphasized (Cain and Hopkins, 2002). It looked different to the earlier generation of nationalists who strove to remove European imperialists from Asia and Africa. The potential point of discomfort, for our present purpose, is that the current generation of IB researchers, trained as they are in quantitative techniques, are likely to be more comfortable with quantitative measures that they do interpret, literally, as 'facts'.

Current IB scholars are generally not trained to use rigorous methods suited to small-sample and qualitative data. But there are techniques – we mention some, such as Ragin's Boolean algebra, Abbot's string analyses, and O'Rourke and Williamson's computational models, to name a few – that can allow one to rigorously extract information from historical data.

We conclude that the costs and benefits of historical analysis in IB should be carefully weighed. Our feeling, however, is that the costs are overly emphasized currently, and the benefits largely obscured. This deserves scholarly scrutiny.

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Notes

[1]Key events that were coded were: diversification, divestiture, domestic acquisition, domestic capital raising, executive change, geographic expansion, international capital raising, joint venture (domestic), organizational structure change, origin, ownership change, regulatory closing, regulatory opening, joint venture (international), vertical integration.

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