Why study emerging-market multinationals?

RAVI RAMAMURTI

As developing and transition economies opened up to the global economy in recent years, a number of local firms not only survived the battle for markets at home, they expanded internationally through exports and foreign direct investment (FDI) to become fledgling multinational enterprises (MNEs) in their own right. By 2007, the more prominent emerging-market MNEs (hereafter referred to as EMNEs) included firms such as China’s Huawei in telecommunications equipment, Mexico’s Cemex in cement, Russia’s Gazprom in energy, India’s Tata Consultancy Services in information technology (IT) services, and Brazil’s Embraer in regional jets. Many more firms in emerging economies were preparing to go down the same path in the future. Business magazines, such as BusinessWeek (2006) and the Economist (2007), trumpeted this trend with cover stories on “emerging giants” or “globalization’s offspring” and illustrated the disruptive effects EMNEs were having on established Western multinationals. Consulting companies, such as McKinsey & Co. and the Boston Consulting Group (BCG), also took notice of these potential clients. There was a parallel increase in studies on EMNEs by international business (IB) scholars, although no consensus emerged on whether and how EMNEs differed from multinationals that had come before.

Why have EMNEs come into prominence in the past decade? What competitive advantages did they leverage as they internationalized? Were they distinctive in any way because they originated in emerging economies?

1 I would like to thank Jitendra V. Singh and other participants in the NU-Wharton conference for useful conversations leading up to this chapter.


3 Aulakh 2007; Buckley et al., 2007; Child and Rodrigues, 2005; Dunning, 2006; Goldstein, 2007; Khanna and Palepu, 2006; Luo and Tung, 2007; Mathews, 2002; Narula, 2006; Ramamurti, 2004; and Zeng and Williamson, 2007.
economies? What internationalization strategies did they pursue, and why? What impact were they having on their respective global industries? How consistent or inconsistent is the rise of EMNEs with mainstream IB theory? These are some of the questions we explore in the pages that follow.

This project was based on three premises. First, we view the rise of EMNEs starting in the early 2000s as a long-term trend with important consequences for the global economy, rather than a flash in the pan. Like Korean and Japanese companies that came before, emerging-market firms were seen as capable of becoming global giants in a number of industries in due course. By some accounts, EMNEs were already among the world’s top twenty firms, in such industries as container shipping (eight firms), petroleum refining (six firms), steel (five firms), mining (three firms), electronics (three firms), and telecommunications (two firms) (UNCTAD [United Nations Conference on Trade and Development], 2007: 123). There was no assurance, of course, that EMNEs would grow steadily in the future as they had in the early 2000s, when, arguably, all the stars were aligned for their ascendance. On the other hand, if the twenty-first century really belonged to emerging economies, as some have claimed (e.g., Wilson and Purushothaman, 2003; van Agtmael, 2007), then these countries could reasonably be expected to spawn many more EMNEs.

A second premise of the project was that IB theory could explain a lot about EMNEs, but not everything of interest to managers and policy makers. Studying EMNEs was therefore seen as a way to enrich existing IB theory, particularly about the process by which firms internationalize and become multinational enterprises. However, to ensure that insights from extant IB theory were taken fully into account in our research, prominent IB scholars participated in the Northeastern University-Wharton School conference at which authors presented their preliminary chapters.

The third premise was that a collaborative research effort would be the most productive way forward, given that EMNEs were relatively new actors on the global stage and hailed from a heterogeneous set of countries – even if those countries were often lumped together under the label “emerging economies.” Accordingly, we invited a team of scholars to write papers on EMNEs specifically for this volume. They were leading IB scholars deeply familiar with the countries about which they were writing. All but two of the country studies (Mexico
and South Africa) had at least one co-author from the country involved. Our sample covered the famous “BRIC” economies – Brazil, Russia, India, and China – plus four other emerging economies. We hoped that juxtaposing the country experiences and company studies would allow us to discern more clearly the country-level and industry-level variables that shaped the competitive advantage of EMNEs. To be sure, including so many countries and researchers could make it harder to reach conclusions, but given the topic’s novelty and complexity we felt alternative approaches would amount to oversimplification. We believed that you had to understand the lay of the land and the facts on the ground before rushing to conclusions about EMNEs or zeroing in on very specific research questions and hypotheses, as some prior studies have done.5 We expect follow-up studies to be focused more narrowly on particular industries, countries, or issues.

Multinationals from rich and poor countries
In the post-WWII period, most of the world’s FDI flowed from one advanced economy to another (see Cell 1 in Figure 1.1). Therefore, even as most of the world’s largest MNEs were based in the advanced economies, most of the research on MNEs was about Cell 1 cases – for example, American companies investing in Europe, or European companies investing in the US.

Cell 2 is probably the next most widely researched case by IB scholars, because even in the 1970s, more than 20 percent of global FDI flows went to developing countries, especially after the commodity-price boom of the mid-1970s (Weigel et al., 1998: Figure 2.4, p. 16). The strategies of Western MNEs in developing countries and their stormy relations with host governments, sometimes resulting in outright expropriations by host countries, caught the attention of IB scholars and development economists (e.g., Kobrin, 1977). Cell 2 assumed renewed importance in the 1990s, when many developing

4 This is in keeping with the plea by Tsui (2007: 1358) for developing “context-specific indigenous theories.”
5 For instance, one detects a rush to judgment in consulting company studies such as those by Sinha (2005) and the Boston Consulting Group (2006), and other studies, such as Mathews (2002) or van Agtmael (2007).
countries set aside their hostility to MNEs and welcomed them instead with open arms. At the same time, sweeping reforms in China and other transition economies created vast new opportunities for MNEs. By 2004, FDI from advanced countries to emerging economies accounted for almost 30 percent of global FDI flows. Concomitantly, research on Cell 2 situations grew significantly.

To date, most of the research on FDI has been about Cells 1 and 2, that is, about investment originating in advanced countries. The fact that most research on MNEs was conducted by Western scholars, particularly in the United States, further skewed the research on MNEs towards Cells 1 and 2.

In the late 1970s and early 1980s, however, outward investment from developing countries received attention from IB scholars for the

![Figure 1.1 Source and destination of FDI.](image)

*Note:* Down-market FDI refers to investment from a more developed country to a less developed one, and up-market FDI refers to the opposite.

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6 This was estimated from data in *World Investment Report 2006*. Total outward FDI in 2004 from developing and transition economies was $60 billion, excluding outflows from offshore financial centers, almost all of which went that year to other developing and transition economies (p. 118). Subtracting this amount from the total inward FDI into developing and transition economies that year ($275 billion, p. 299), suggests that $215 billion came from advanced countries (Cell 2). Given that worldwide FDI outflows that year (excluding $66 billion from offshore financial centers) was $747 billion, Cell 2’s share is 29 percent.
first time, at the same time as the first significant wave of outward FDI from developing countries took place (Wells, 1977). At least two-thirds of that outward investment went to other developing countries (Wells, 1983: 4), that is, they were predominantly of the Cell-3 type, or what is sometimes referred to as South–South investment. Studies from this period shed light on the distinctive aspects of South–South investment (e.g., Wells, 1983; Lall, 1983; Kumar, 1982; Lecraw, 1977), but work on Cell 3 situations petered out as South–South FDI failed to keep pace with overall growth in FDI, partly because the leading source of outward FDI from developing countries – Latin America – got mired in debt crises during the 1980s. Even at its peak, though, outward FDI from developing countries in the 1970s represented only a small percentage of global FDI flows (UNCTAD, 2007). Moreover, since Cell 3 cases did not affect advanced countries, they did not receive much attention in the West.

Of the four cells in Figure 1.1, the least studied was Cell 4, which represented FDI originating in developing countries and destined to advanced countries. To be sure, this neglect was largely justified by the facts. At best, such flows represented one-third or less of the outward FDI flows from developing countries, which itself in past years represented one-tenth or less of overall global FDI flows. And even when it occurred in the 1960s and 1970s, Cell 4 cases were probably seen as aberrations, originating in atypical developing countries, such as Hong Kong, which at the time was an unusually open economy (Lall, 1983, ch. 3). In most other developing countries, Cell 4 investments were a rarity, although they did occur from time to time – for example, when the Indian firm Kiroloskar bought up 48 percent of a German engineering company in 1965 (Lall, 1983: 22).

The second wave of outward FDI from developing countries began in the 1980s in countries such as Hong Kong, Singapore, and Taiwan, but spread to many more countries in the early 1990s. Annual FDI outflow from developing and transition economies peaked at $133 billion in 2000, then fell to one-fourth of the peak, followed by a rally that took it to $174 billion in 2006 (UNCTAD, 2007: 251). Outward FDI from emerging economies (i.e., developing and transition economies) could no longer be ignored. By 2006, the outward FDI stock of emerging economies exceeded $1,600 billion, compared to $149 billion in 1990 (UNCTAD, 2007: 255). In this second wave, the outward FDI from emerging markets represented 14 percent of global
FDI flows, which was substantially higher than in the 1970s. This was all the more impressive because FDI outflows from advanced countries also surged in this period, from $50 billion in 1980 to $218 billion in 1990 and $1,023 billion in 2006.

The share of outward FDI from developing countries going to advanced countries averaged 20% between 1985 and 2004, reaching a high of about 35% in 2000 (UNCTAD, 2007: 118). These Cell 4 investments made headlines in the West, because they belonged to the “man-bites-dog” category of news stories: you had firms from poor, underdeveloped countries investing in rich, developed countries, which puzzled many observers, including FDI scholars. Among the recent headline-grabbing Cell 4 cases were China National Offshore Oil Corporation’s (CNOOC) failed bid for Unocal, Lenovo’s acquisition of IBM’s personal computers business, Mittal Steel’s merger with Arcelor of France, Russian Lukoil’s acquisition of Getty Oil, and Tata Steel’s takeover of Anglo-Dutch Corus Steel, to name just a few examples. There were many more examples of Cell 4 investments, as the country studies in this volume show.

Cell 4 cases are interesting theoretically, because they go against the grain of conventional wisdom about the direction in which capital, technology, and knowledge should flow in the global economy – that is, from advanced economies to emerging economies. Cell 4 is a good example of a situation that extant IB theory fails to explain well.

The focus of this book is on Cells 3 and 4, both of which deserve more attention than they have traditionally received from IB scholars, not because they account for the lion’s share of global FDI flows – which they do not – but because they are important to the home countries involved and because of the disruptive effect that EMNEs seem to have on their global industries. The rise of Cemex, Embraer, Huawei, or Tata Consultancy Services (TCS), for instance, caused considerable turmoil for Western MNEs. In the case of Cemex, a tranquil, regional industry was turned into a dynamic, global one, forcing established cement firms such as Holcim of Switzerland and Lafarge of France to quickly bolster their global presence (Ghemawat and Matthews, 2000; Lessard and Lucea, Chapter 10 in this volume). Brazil’s Embraer was a real thorn in the side of Canada’s Bombardier, which had earlier been the global market leader in regional jets (Goldstein, 2007; and Fleury and Fleury, Chapter 8 in this volume). Huawei’s aggressive internationalization was at least one important
reason why Siemens and Ericsson pooled their telecom equipment businesses and Alcatel merged with Lucent. And Indian IT firms such as TCS, Infosys, and Wipro forced giants such as IBM and Accenture to rethink their core business models (Palmisano, 2006; Ramamurti and Singh, Chapter 6 in this volume).

EMNEs are also important because they are potentially the Samsungs and Toyotas of the future. In the 1960s, about thirty Japanese companies, and no Korean companies, appeared on the Fortune Global 500 list, but in 2007 companies from these two countries and Singapore held ninety-two spots on the list (for Japanese companies on the list in 1962, see Amsden and Hikino, 1994: 116). Similarly, EMNEs from countries other than South Korea, Singapore, and Taiwan held forty-nine spots on the 2007 list, but could easily double or triple that number by 2020 or 2030. In 1999, China set an explicit goal to get fifty of its companies on to the Fortune Global 500 list by 2010, a target unlikely to be realized, because only twenty-four had made the list by 2007. But China’s goal is indicative of its ambitions, and the extent of support for achieving them. For those interested in how new entrants can displace incumbent global giants, EMNEs will provide an interesting domain for further study.

EMNEs have also represented attractive financial investments, compared to their incumbent Western rivals. The point is illustrated by the profitability and valuations of Indian software service firms. In 2006, two such companies, Infosys and Wipro, had sales of only about $2 billion, compared to $18–20 billion for US rivals such as Accenture and EDS; yet, their after-tax profit margins were in the range of 20–25%, compared to 1% for EDS and 5% for Accenture, and their market capitalization was of the order of $30 billion, compared to $22 billion for Accenture and $13.7 billion for EDS (valuations as of January 23, 2007). This was one reason that Goldman Sachs, in its famous report on the BRICs, urged its clients to increase the weight of emerging economies in their global investment portfolios (Wilson and Purushothaman, 2003). Indeed, portfolio investors in emerging markets earned some of the highest returns during the period 2003–2007.

None of the above implies that the rise of EMNEs will be monotonic and permanent. It is quite possible that some of these firms will stumble or even collapse, because of overambitious strategies or poor execution (recall Korean Daewoo’s experience after the Asian
financial crisis). Emerging economies also face many economic and political risks that could derail their upward trajectories. Rising labor costs or currencies may undermine the low-cost advantage that many EMNEs enjoy in 2008. Protectionism may also rise in the advanced countries, slowing down the internationalization of EMNEs (Aharoni and Ramamurti, 2008). It does not take much imagination to construct negative scenarios of this sort. On the other hand, it is also quite possible that the turn towards free markets among emerging economies, particularly in Asia, will not be reversed, and that rapid domestic-market growth and openness to global competition will produce more EMNEs in the future, not just in low-technology or commodity businesses but quite possibly also in industries employing sophisticated technologies and requiring sophisticated marketing skills.

Research questions and prevailing IB theory

In studying emerging multinationals from developing and transition economies (i.e., Cells 3 and 4 in Figure 1.1), we are interested in answers to the following questions:

1. What competitive advantages and capabilities do EMNEs leverage in international markets, and how are those advantages and capabilities shaped by the home-country context?
2. What internationalization strategies do they follow, and why?
3. What impact is their rise having on global industry dynamics, including established Western MNEs?

The first two research questions are not unlike those asked by researchers who have studied Cells 1 and 2, and they were also probed by researchers studying Cell 3 when the first wave of outward FDI from developing countries occurred in the 1960s and 1970s. The more fundamental question is whether the concepts and theories developed by studying Cells 1 and 2 are equally relevant for Cells 3 and 4. It is possible, a priori, to argue for either side of this issue.

On the one hand, all four cells entail firms making cross-border direct investments, and for that reason a common set of concepts, frameworks, and theories may well explain them equally effectively. Several mainstream IB ideas, such as Dunning’s Ownership-Location-Internalization (OLI) framework (Dunning, 1977), the motivations for internationalization (market-seeking, resource-seeking, strategic
asset-seeking), the notion of “liabilities of foreignness” (Zaheer, 1995), or the stages-model of internationalization (Johanson & Vahlne, 1977), may apply as well to Cell 3 and 4 situations as they do to Cell 1 and 2 situations. After all, the only difference across the four cells is the state of one contextual variable – a country’s level of development. While level of development is clearly important, it may not be so powerful a contextual variable as to nullify the explanatory power of mainstream IB theories for how and why firms become multinational enterprises. This is particularly true of abstract frameworks, such as the OLI framework, which posits a set of general conditions that an organization must meet in order to become multinational. It states, for instance, that a firm cannot become multinational unless it possesses firm-specific or ownership advantages that offset the disadvantages of operating in a foreign country, or that firms expand internationally only if there are location-bound advantages in foreign countries that cannot be exploited without a presence in those countries, or that a firm will internalize international transactions only if alternative arm’s-length arrangements for exploiting foreign opportunities are less profitable. These assertions are general enough that they may hold regardless of context. Indeed, in his 1983 work on Third World multinationals, Wells posed the question raised here and came to the following conclusion:

Can the same concepts that have proved useful in studies of the traditional multinationals help in understanding the new foreign investors [Third World multinationals]? My contention is that they can and the process of applying the concepts to the new firms aids in understanding both the concepts and the different kind of multinationals. (Wells, 1983: 6)

On the other hand, context becomes much more relevant if one is interested in substantive answers to the questions that motivated this research project. If one would like specifically to know what ownership advantages multinational firms from different countries enjoy and why, or what the location advantages of different countries are, or what particular internationalization paths firms are likely to follow in different contexts, then context-free frameworks are inadequate. For instance, the ownership advantages most commonly attributed to MNEs from the West include proprietary cutting-edge technologies, marketing prowess, and powerful brand names. None of these is usually a source of competitive advantage for MNEs from developing
countries, a point Lall (1983) noted in his study of the first wave of outward FDI from developing countries:

Studies of MNEs have been strongly flavored by the activities of firms from the USA. The literature has barely started to take note of the possibility that the nature of monopolistic advantages of MNEs from other countries may be quite different. (Lall, 1983: 2)

Early work on Third World MNEs suggested a distinctive set of advantages for such firms (Wells, 1983; Lall, 1983; Amsden, 1989), but the relevance of those findings for EMNEs in the changed context of the 2000s is an open issue (see Wells, Chapter 2 in this volume). Recent work on EMNEs suggests a wider range of possible competitive advantages, including the extreme view that EMNEs internationalize to acquire competitive advantages rather than to exploit pre-existing ones (Mathews 2002), a point also made by Luo and Tung (2007). Narula (2006) offers a passionate rebuttal of the latter argument, while Dunning (2006) concedes that Mathews’ view may have some merit. The country studies in this volume shed light on this interesting controversy.

The other half of the first research question brings into focus country- and industry-level variables that shape the competitive advantage of firms. We know from prior work on clusters and the competitive advantage of nations (Porter 1990) that a country’s endowments – such as labor, skills, capital, home demand, quality of suppliers and customers, and market-supporting institutions – affect the international competitiveness of its firms. The question is what kinds of distinctive competitive advantages emerging markets may bestow on their firms, because of distinctive contextual factors, such as their lower level of development, their status as late-industrializing countries (Amsden, 2001; Amsden and Chu, 2003), the weakness of their institutions (Khanna and Palepu, 1997 and 2005), or the faster growth of their markets.

The second question was designed to look at the usual aspects of internationalization, such as which foreign markets a firm targets and why, how rapidly it internationalizes, and what modes of entry it uses to enter chosen markets. Of particular interest was the question of when EMNEs target their exports and investments towards other emerging markets and when they do so towards rich-country markets,
such as the US, Europe, and Japan. In terms of modes of entry, a question of interest was when an EMNE pursues organic growth and when it engages in cross-border acquisitions, and why.

The internationalization process, again, is an area in which extant literature offers some answers, but the relevance of those findings for EMNEs is unclear. The dominant framework here is the stages-model of internationalization, which grew out of research on Scandinavian manufacturing firms internationalizing within Europe in the 1970s (Johanson and Vahlne, 1977). While offering excellent insights, its contextual parameters were not always recognized in subsequent studies, as a result of which it yielded quite mixed findings when applied, for example, to Japanese firms expanding into the US in the 1980s or to Dot Com firms globalizing rapidly in the 1990s (Rhee and Cheng, 2002). The relevance of the stages-model to EMNEs is brought into question by the fact that emerging economies are different from developed economies in many respects, that EMNEs frequently invest up-market into advanced countries (a scenario not considered in the stages-model), and they are internationalizing at a time when the technological context and economic policy environment are quite different from what prevailed in the 1960s or 1970s.

In summary, a good part of the IB literature consists of context-free generalizations at high levels of abstraction, or context-dependent generalizations at much lower levels of abstraction. The former apply universally, but are too broad to guide managerial decision making or policy making. The latter are much more specific and useful to decision makers, but their applicability is often limited to the particular contexts (space and time) in which they originated. What is needed is theory in between that incorporates contextual variables as contingencies.

Some IB theories explicitly recognize the role of context. An example is the product cycle hypothesis (Vernon, 1966), which grew out of an effort to explain the nature of US exports, outward FDI, and imports, to or from Western Europe and developing countries. Its reasoning included key contextual variables, such as America’s lead in per-capita income and technology relative to Europe, and the latter’s lead in these same areas relative to developing countries. When America’s income and technological lead over Europe narrowed, the original product cycle hypothesis needed modification (Vernon, 1979).
And there’s the difficulty with context-dependent models – they often do not survive unscathed across space (countries) and time. Countries differ so much from one another on so many dimensions and the world changes so much from one decade to the next that context-dependent theories must be continually revised. It is daunting to develop models that incorporate so many contextual variables, or to distill the complexity down to a parsimonious and stable set of variables.

From our point of view, there are at least four dimensions of context that need to be taken into account in IB research. First, there is the home-country context, one aspect of which is highlighted in Figure 1.1 – that is, a country’s level of development. But countries differ along other important dimensions as well, such as their endowment of natural resources, the quality of their institutions, the size of their economies, their human capital, and so on.

Second, to understand firm internationalization, one must recognize industry context, because the globalization of potential of industries varies widely, as argued by Yip (1989), and the dynamics of internationalization varies in different stages of the product life cycle, as illustrated by the product cycle hypothesis.

Third, the issues that arise in internationalization are quite different in the early stages of that process, when firms are building global presence, than in the later stages, when the firm has already built a sprawling network of overseas subsidiaries. The impact of the home-country on a firm’s competitive advantages, for instance, is more pronounced in the early stages of internationalization than in later stages. It is easy to forget that IB, as a serious field of study, began only in the 1960s, shortly after the Academy of International Business (AIB) was established in 1959; AIB’s main organ, the Journal of International Business Studies, began publishing only in 1970. By this time, US firms had already built a significant international presence and accounted for 55% of the world’s outward stock of FDI, compared to only 6% in 1914 (see Table 1.1). It is only natural that as the IB field took off in the US, researchers would focus on contemporary problems facing large American MNEs, which included managing their global networks, coping with conflicting pressures from different markets and governments, or growing the enterprise at the margin by entering new markets, such as developing countries. Only business historians paid much attention to the question of how large US or
European MNEs became multinational firms in the first place (see, e.g., Wilkins, 1974; and Franco, 1976). On the other hand, building global presence was the key challenge facing EMNEs in the early 2000s. Even the pioneering EMNEs listed in the first paragraph of this chapter relied heavily in 2007 on exports, and much of their overseas production came into being only after 2000. Thus, studying EMNEs provides an opportunity to revisit the issues that arise as firms internationalize – and that too in a contemporary twenty-first century context.

That brings us to the final contextual factor that IB theory must take into account, namely the temporal context, because the world can change so profoundly from one decade to the next. One important area of change in the past two decades has been the economic policy environment, which in many emerging economies has changed from being highly closed to relatively open, in matters such as domestic regulation, international trade, and international investment. The

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Notes:
* Europe’s share fell secularly from 1914 to 1980 but then began to reverse course, with the growth of intra-EU (European Union) FDI, following the Single European Act of 1986 and the creation of the euro.
® Reported as “developing economies” in UNCTAD’s FDI statistics.
Source: Aharoni and Ramamurti (2008), with 2006 data from UNCTAD (2007: 255)
international policy environment has also changed in the past two decades, with agreements such as the Uruguay Round taking effect and institutions such as the World Trade Organization (WTO) coming into existence. The other profound change has been in the realm of technology, especially in information, computing, and communications technologies, which have lowered the costs and increased the benefits of internationalization, while opening up services to international trade in unprecedented ways.

Given that the aim of this project was to obtain substantive answers to the three research questions we began with, we could not ignore context, even though the contexts we are interested in are under-researched (Cells 3 and 4) and manifestly different from the US and Europe, where mainstream IB ideas were developed. Cell 4, in particular, has not been studied much at all, and it could well warrant new explanations or significant modification of existing theories.

To sum up, our goal is to understand how firms originating in emerging markets and operating in different industries build global presence in the contemporary economic environment. In pursuing this goal, we have to be careful not to fall into the trap of assuming that Cells 3 and 4 are so unique that none of the past research on Cells 1 and 2 will be relevant to our understanding of them, nor should we limit ourselves to only those concepts or frameworks developed from studying Cell 1 and 2 situations.

Overview of the book

Each country study in Part II focuses on the three research questions mentioned earlier. No particular theoretical lens was prescribed, although most authors drew frameworks from the IB literature. Authors were requested to build their country analysis through micro-level case studies of important firms that had internationalized, rather than focus just on macro trends, such as the volumes of outward FDI flow, its sector breakdown, its geographic distribution, and so on. They were encouraged to explore why certain firms in certain industries in each country were in the vanguard of internationalization. The studies were expected to be descriptive or positive rather than normative; no attempt was to be made to assess the effectiveness of internationalization strategies, using performance metrics such as a firm’s growth rate, market share, profitability, or market capitalization.
In the mid-2000s, the jury was still out on the long-term viability or performance implications of the strategies being pursued by EMNEs.

Our sample of countries includes the famous quartet of Brazil, Russia, India, and China, as well as two other large emerging economies, Mexico and South Africa (see Table 1.2). Notable omissions include Asian economies that opened up to globalization in the 1970s and 1980s – such as South Korea and Taiwan, whose trailblazing experiences would have served as a valuable backdrop for studying the experiences of late-globalizing countries. Unfortunately, authors who we had expected to write on those countries were unable to participate in the project. Nevertheless, Amsden’s essay (Chapter 4) draws on the experience of both countries, given her extensive prior work in those settings (Amsden 1989; Amsden and Chu, 2003). On the other hand, our sample includes two countries – Thailand and Israel – that may surprise some readers. We thought Thailand was interesting because it had not spawned many EMNEs, even though it was a high-growth economy, in a high-growth region, and the recipient of considerable inward FDI. Labeling Israel an “emerging economy” is debatable, because its 2006 per-capita income of $23,300 is comparable to that of many industrialized countries and thrice that of the next richest country in our sample, Mexico. Yet, the country’s very small size and its exceptional record in spawning EMNEs made it an interesting case for inclusion. In short, while the project’s sample selection was mostly deliberate, it was opportunistic at the margin, taking advantage of leading IB scholars willing to collaborate on the project.

The book consists of three parts. The first introduces the topic and the issues surrounding EMNEs. The second, which is the heart of the book, analyzes the experience of fledgling MNEs in eight emerging markets, guided by the project’s research questions. The final part presents conclusions and suggestions for future research.

Part I includes an essay by Louis T. Wells Jr., who pioneered research on multinationals from developing countries in the 1970s, resulting in the seminal work, *Third World Multinationals* (1983). Wells’ essay highlights areas in which his original work is still valid as well as new issues raised by EMNEs. In the next essay, Alan M. Rugman argues that EMNEs are neither truly novel (compared to MNEs that came before) nor particularly important actors in the global economy, compared to US, European, or Japanese MNEs that
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<th>GDP, 2006 (US$ B)</th>
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<td>China (Williamson and Zeng)</td>
<td>2,527</td>
<td>1,914</td>
<td>1,320</td>
<td>China International Marine Containers, Haier, HiSense, Huawei, Lenovo, Pearl River Piano, Wanxiang, Xi’an Aircraft Co.</td>
</tr>
<tr>
<td>India (Ramamurti and Singh)</td>
<td>805</td>
<td>712</td>
<td>1,130</td>
<td>Dr. Reddy’s, Hindalco, Infosys, Mahindra &amp; Mahindra, Ranbaxy, Suzlon Energy, Tata Group, Wipro</td>
</tr>
<tr>
<td>Russia (McCarthy, Puffer, and Vikhanski)</td>
<td>734</td>
<td>5,205</td>
<td>141</td>
<td>Evraz, Gazprom, Lukoil, Norilsk Nickel, Rosneft, Severstal, United RusAl, VimpelCom</td>
</tr>
<tr>
<td>Brazil (Fleury and Fleury)</td>
<td>967</td>
<td>5,089</td>
<td>190</td>
<td>COTEMINAS, CSN, Duratex, Embraer, Gerdau, InBev/AmBev, Marcopolo, Petrobras, SABO, TIGRE, Vale (CVRD), WEG</td>
</tr>
<tr>
<td>South Africa (Goldstein and Prichard)</td>
<td>201</td>
<td>4,568</td>
<td>44</td>
<td>ABSA, Aveng, MTN, Old Mutual, SABMiller, Sasol, Standard Bank, Telkom, Woolworths Holdings</td>
</tr>
<tr>
<td>Mexico (Lessard and Lucea)</td>
<td>743</td>
<td>6,880</td>
<td>108</td>
<td>CEMEX</td>
</tr>
<tr>
<td>Thailand (Pananond)</td>
<td>198</td>
<td>3,046</td>
<td>65</td>
<td>Charoen Pokphand Group, Siam Cement Group, PTT, S&amp;P Group</td>
</tr>
<tr>
<td>Israel (Aharoni)</td>
<td>140</td>
<td>23,300</td>
<td>6</td>
<td>Check Point Software, Elscint, Scitex, Teva Pharmaceutical Industries</td>
</tr>
</tbody>
</table>
account for most of the world’s trade and FDI stock. In Chapter 4, Alice Amsden takes the opposite view to Rugman’s, arguing that the developmental implications of indigenous firms in emerging economies are quite different from those of foreign-owned firms.

Part II, which presents the country studies, begins with the two low-income, mega-population economies in our sample – China and India (see Table 1.2). These are followed by five middle-income countries with varying degrees of natural resource endowments: at the high end of natural resource endowment is Russia, followed by the other four countries – Brazil, South Africa, Mexico, and Thailand. The last case, Israel, is unique in that it is both much richer than the other countries and much tinier in terms of population and gross domestic product (GDP). The three parameters used to describe these eight countries – economic size, per-capita income, and natural resource endowment – appeared, a priori, to be important country-level variables that would influence the types of EMNEs produced by countries. China and India, with their vast home markets and low incomes, were expected to spawn quite different EMNEs than South Africa or Russia, with their vast, exportable mineral resources, and still different from those spawned by tiny Israel. However, this initial guess about which country-level variables were important was only a provisional, working hypothesis, to be revisited after the country studies were completed. In the end, other country-specific variables, such as the quality of human capital and the availability of local entrepreneurs, turned out also to be important determinants of whether and how indigenous firms would internationalize.

References


