



PERSPECTIVE

Towards a renaissance in international business research? Big questions, grand challenges, and the future of IB scholarship

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Abstract

In this article, we review critiques of international business (IB) research with a focus on whether IB scholarship tackles “big questions.” We identify three major areas where IB scholars have addressed important global phenomena, but find that they have had little influence outside of IB, and only limited effects on business or government policy. We propose a redirection of IB research towards “grand challenges” in global business and the use of interdisciplinary research methods, multilevel approaches, and phenomena-driven perspectives to address those questions. We argue that IB can play a more constructive and vital role by tackling expansive topics at the business–societal interface.

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INTRODUCTION

In a widely debated paper, Buckley (2002) expressed concern that the international business (IB) field might be “running out of steam” because IB scholars had failed to identify the next big questions relevant to the field. Buckley (2002) suggested that, in its early decades of development, IB research succeeded by seeking to answer big questions. It maintained its relevance by explaining phenomena that stand-alone theories were unable to address adequately. These included the presence and nature of foreign direct investment (FDI) flows, the emergence and evolution of multinational enterprises (MNEs), and the transition from internationalization to globalization. Subsequently, many others have echoed the call for research tackling big questions in IB research. For instance, Buckley & Lessard (2005) presented recommendations on how IB research could regain its past strengths. Shenkar (2004) urged IB scholars to contribute more directly to solving real problems in the global economy, while Cheng, Henisz, Roth, & Swaminathan (2009) outlined the potential role of interdisciplinary research in IB. These examples illustrate that IB scholars – like those in other management disciplines – are facing increasing pressure, both from within and outside the academy, to produce more

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relevant and impactful research (Shapiro, Kirkman, & Courtney, 2007).

Although the issue of practical relevance has been a recurring theme within the IB research community and the business and management disciplines more broadly, we take issue with the unhelpful, underlying contention that IB research has “lost its way.” Instead, we argue that the field as a whole has been somewhat successful in addressing a number of big questions in the decade and a half since Buckley’s (2002) seminal contribution. Specifically, although the field has largely de-emphasized its tradition of responding to questions that arise from empirical developments in the world economy, we think that IB scholars have identified important research themes, provided rigorous theoretical insights, and offered contributions with genuine practical relevance. Indeed, some – but not many – of these contributions have been successfully transferred to other areas of management and social science (Cantwell, Piepenbrink, & Shukla, 2014).

For illustrative purposes, we identify three major areas (among many) where IB scholars have addressed important and emerging phenomena in a way that is scientifically valid, practically relevant, and intellectually stimulating: (1) explaining and providing theoretical insights into the rise of MNEs from emerging economies (EEs); (2) exploring the growth, causes, and consequences of offshoring and the disaggregation of global value chains; and (3) understanding how MNEs respond to greater pressures for social responsibility and sustainability in their global operations.¹

Despite these relative successes, we do acknowledge and underscore two “translation” problems. *First*, IB scholarship appears to have had only a modest influence outside of the business disciplines. That is, allied social sciences – such as economics, political science, and sociology – rarely cite IB research, while the reverse is somewhat more common (Cantwell et al., 2014; Nerur, Rasheed, & Pandey, 2016). The issue here appears to be twofold. On the one hand, while IB scholars tend to embrace interdisciplinary perspectives initially when studying and explaining relatively new phenomena, as these research streams mature, scholars appear to become more inward-looking and self-referential. In fact, the criticism that IB research is “inward-looking” is one to which we will repeatedly return. On the other hand, truly interdisciplinary research that bridges fields of study is challenging to conduct and does not typically result in the kind

of focused contribution that top journals appear to prefer. Additionally, some “bridging” papers published in mainstream management journals fail to engage with the classic works of IB adequately.

Second, and consistent with broader reflections in the Academy of International Business and Academy of Management (to name just two particularly prominent scholarly associations), IB scholarship appears to be rarely picked up by popular or industry publications (Nerur et al., 2016) or applied by practitioners themselves.

We argue that IB research could do much more to contribute to efforts by social and behavioral scientists to make sense of critical global phenomena. Specifically, we call upon IB scholars to address grand challenges, with the purpose of advancing IB theory, contributing to important debates with scholars in allied social sciences, as well as actually helping to resolve these difficult challenges our generation is currently facing. Grand challenges are defined as “ambitious but achievable objectives that harness science, technology, and innovation to solve important national or global problems and that have the potential to capture the public’s imagination” (US Office of Science and Technology Policy, 2014). Grand challenges typically transcend geographic, economic, and societal borders, and are therefore multinational by nature. As such, IB scholars are in a particularly strong position to inform grand challenges, especially as they relate to the interaction of organizations and individuals across borders within the context of the global business system. Such challenges can include topics that would traditionally be considered part of the domain of IB, such as the anti-globalization sentiment and its impact on MNE strategy, location, and operations, as well as broader issues such as climate change, poverty, migration, terrorism, and infectious disease. These latter topics are examples of problems that affect both MNEs and the societies in which they are embedded, and for which IB scholarship could be leveraged. Tackling such issues, however, will require a widening in scope of what has come to be accepted as the appropriate expanse of the IB domain, and a loosening of the constraints that have been self-imposed regarding theory, method, and research approaches.

In the next section, we evaluate the assertion that IB research has become tame and uninspiring by examining three contributions made by IB scholars over the past decade. We assess the extent to which IB scholarship has (1) engaged with and influenced consideration of these topics by allied social sciences



such as economics, political science, sociology, and more specific areas such as international political economy and development, and (2) penetrated popular and business media as a proxy for influence on actual practice. Building on this analysis, we then explore the potential of IB scholarship to contribute to the grand challenges facing not only business, but also other stakeholders in the global environment. We suggest that researching such challenges, however, requires a broadening of the scope of recent IB research, including a focus on phenomena first, integration of insights from multiple disciplines, the direct application of multilevel methods, and the acknowledgement of interdependencies among business, government, and society in the global environment. We argue that IB scholars are uniquely positioned to address these challenges, and we propose specific topics where IB researchers could bring unique insights and make novel contributions. We conclude with some practical observations and suggestions regarding how to realize this agenda.

IS INTERNATIONAL BUSINESS RESEARCH RUNNING OUT OF STEAM? RECENT CONTRIBUTIONS TO BIG QUESTIONS IN IB

We reviewed IB research from the past 15 years, following Buckley's (2002) contribution to the debate on the relevance of studies in this field. We identified three major phenomenon-driven topics that have been addressed by IB researchers (we do not aim to be exhaustive in this context). These themes were selected for several reasons. First, they are part of the core IB research areas – as reflected by publications in major IB journals – and have attracted a critical mass of researchers. That is, they do not represent niche, fringe areas. Second, they focus on institutions that transcend an exclusive focus on MNEs, something for which critics of IB scholarship have consistently called. Third, they engage to some extent with multiple stakeholders and involve interdisciplinary methods and approaches often identified as important for IB research. As such, they at least partially correspond with John Dunning's proposed 4Is approach ("international business," "institutions," "interface with multiple stakeholders," and "interdisciplinary") (Collinson, Buckley, Dunning, & Yip, 2006). In addition, these topics share some common elements, in that each was initially "emergent," representing a real-world issue or trend unfolding in the IB environment, towards which IB researchers directed scholarly attention:

- (1) Explaining the emergence and strategies of MNEs from EEs;
- (2) Exploring the growth, causes, and consequences of offshoring and the disaggregation of global value chains; and
- (3) Understanding how MNEs respond to greater pressures for social responsibility and sustainability in their international operations.

Explaining the Emergence and Strategies of MNEs from Emerging Economies

The rise of EE MNEs has attracted significant interest among IB scholars (for a review, see Aguilera, Ciravegna, Cuervo-Cazurra, & Gonzalez-Perez, 2017; Luo & Zhang, 2016; Verbeke & Kano, 2015). Research on EE MNEs has enriched IB theory on various levels. Specifically, while a number of studies have relied on established theoretical perspectives to explain the emergence and behavior of EE MNEs, such as the eclectic paradigm (e.g., Cuervo-Cazurra, 2008; Sun, Peng, Ren, & Yan, 2012) or the institution-based view (e.g., Ang, Benischke, & Doh, 2015; Gaur, Kumar, & Singh, 2014; Liou, Chao, & Yang, 2016), at least two novel theoretical perspectives have emerged from this stream of work. The first, the springboard perspective, was introduced by Luo & Tung (2007). It addresses the paradox of rapid internationalization of EE firms. Such rapid internationalization is somewhat surprising, given that EEs are thought to lack the institutional framework needed to facilitate the development of traditional ownership advantages that are generally considered essential for successful internationalization. Instead, the springboard perspective suggests that EE firms in fact internationalize in a rapid fashion, in order to overcome their inherently inferior firm-specific resource base, due to weak home-country institutional and market conditions. This phenomenon prompts IB scholars to revisit the baseline assumption that strong firm-specific advantages in areas, such as R&D and branding, are necessary for successful internationalization (Makino, Lau, & Yeh, 2002; Mallon & Fainshmidt, 2017; Wright, Filatotchev, Hoskisson, & Peng, 2005). In support for this asset-augmenting perspective, Gubbi, Aulakh, Ray, Sarkar, & Chittoor (2010) show that EE MNEs access complementary assets that are not available in their home countries in foreign markets (see also Cui, Meyer, & Hu, 2014). Furthermore, Cuervo-Cazurra, Narula, & Un (2015) demonstrate that EE MNEs seek to reduce their



exposure to their weak home-country institutional environment by expanding across borders.

The emergence of EE firms in the global marketplace has also challenged traditional understanding of competitive advantage. Resource-based arguments predict that EE firms would find it difficult to compete successfully with MNEs from developed economies, given their lack of traditional firm- or country-specific resources. The big question here then relates to understanding how and why EE MNEs can take advantage of globalizing tendencies when they lack the typical resources and capabilities associated with MNEs' global success (Mathews, 2006). Research on this question is still in its infancy; however, early findings point towards the possibility that EE firms possess non-traditional capabilities that allow them to succeed in foreign markets despite these deficiencies (Contractor, Kumar, & Kundu, 2007). Such advantages may relate to the superior networking (Mathews, 2006) for instance, or resource-recombination (Gubbi et al., 2010; Kim, Hoskisson, & Lee, 2015). These insights are captured by the linkage-leverage-learning (LLL) framework introduced by Mathews (2006). The LLL framework suggests that EE MNEs can compete with incumbent MNEs due to their ability to access valuable resources through linkages with external firms, and to successfully leverage these links. Advantages achieved through linkage and leverage are subsequently reinforced through repetition, meaning that EE MNEs learn to perform these operations more efficiently.

The impact of these new theoretical insights beyond the IB domain, however, has remained somewhat limited. In fact, it appears that scholars outside the IB discipline find it difficult to appreciate the distinctive contribution of this research stream to the broader domain of business studies. For example, in a comprehensive review on EE MNEs, Luo & Zhang (2016) found that between 1990 and 2014 only six articles on EE MNEs were published in non-core IB journals (three articles in *Academy of Management Journal*, two articles in *Organization Science*, and one article in *Strategic Management Journal*).

We believe that there are at least two possible explanations for this lack of broader appeal to scholars outside the IB discipline. First, IB scholars studying EE MNEs tend to rely on the same theories that are also used to study traditional MNEs, such as the resource-based view or institutional theory (Luo & Zhang, 2016), thereby paradoxically often focusing on theories that are rather

peripheral to the phenomenon at hand, and in some cases, neglecting potentially applicable mainstream IB theory. It is thus not surprising that many within the IB domain conclude that existing theory – with some modifications – can broadly explain the emergence and behavior of EE MNEs (e.g., Buckley, Clegg, Cross, Voss, Xin, & Zheng, 2007; Dunning, Kim, & Park, 2008; Guillen & Garcia-Canal, 2009; Ramamurti, 2012; Verbeke & Kano, 2015). While IB scholars may see value in pursuing this question, scholars in related fields may ask why this research is necessary, if it does not generate insights beyond what is known from other work within and outside the IB domain. Second, IB scholars have focused on a rather narrow set of research questions, namely why EE MNEs exist, how they are different from developed-economy MNEs, and, to a lesser degree, how they compete in foreign markets.

The narrow scope of research has potentially hindered IB scholars from studying more impactful research questions. For instance, the IB literature offers little insight into the mechanisms by which EE MNEs' managers deploy and leverage the unique resources mentioned above or the specific micro-processes for EE MNE internationalization. More broadly, IB scholarship has not addressed the potential contribution of EE MNEs to host-country economic development, or their sociological effects on host-country societies. At a more micro-level, there have been few explorations of the role of leaders, founders, and family members in directing EE MNE strategy, a surprising shortfall, given the typically disproportionate role of such leaders and their families in EE MNEs. Furthermore, although this research trajectory has contributed important insights to IB scholarship, these do not appear to have spilled over to the world of policy or practice, a concern we take up below. Exceptions include a number of books seeking to translate the findings IB research has generated for more practitioner-oriented audiences, such as those by Agtmael (2007) and Chattopadhyay & Batra (2012). Even here, however, it is not clear that these volumes have experienced widespread readership, as evidenced by their relatively modest sales data on Amazon and other sites.

Exploring the Growth, Causes, and Consequences of Offshoring and the Disaggregation of Global Value Chains

Beginning in the late 1990s, managers, policymakers, and the public at large became increasingly



interested in the phenomenon of outsourcing, which ultimately often took the form of offshore outsourcing. While sourcing had been a common topic of IB research for some time, especially in the international marketing and operations areas, the addition of “out” or “off” connoted a new era, in which companies were disaggregating production and/or shedding activities that had been performed internally in favor of contracting out. In the popular and business press, these activities were sometimes portrayed as further evidence that manufacturing in developed countries was hollowing out, and that companies that engaged in offshoring were responsible for this process. The phenomenon is exemplified by a spectacular widening and deepening of the “market for market transactions,” extending the opportunities for the use of market-like arm’s length transactions and contracts in the global economy (Liesch, Buckley, Simonin, & Knight, 2012).

IB researchers were surprisingly quick to take on this topic, with an early contribution from Doh (2005) that squarely addressed the controversy and drew implications for the IB and strategic management fields, including those focused on ethics and social responsibility. Special issues of *JIBS* (see Kenney, Massini, & Murtha, 2009; Doh, Bunyaratavej, & Hahn, 2009) and the *Journal of Management Studies* (among others) followed, stimulating a rich stream of offshoring studies that examined antecedents, processes, and consequences, at multiple levels of analysis. Indeed, in their 2011 review of services offshoring research, Bunyaratavej, Doh, Hahn, Lewin, & Massini (2011) catalog a vast literature from IB, management, operations, and marketing, focusing on (1) external contextual factors (institutions, culture, risks, power/control in global governance), (2) services offshoring drivers (labor arbitrage and cost minimization, qualified personnel and skilled labor, offshoring as strategy, the role of information and communication technology), and (3) services offshoring management issues (performance and success factors, innovation and knowledge transfer, organizational governance).

More recently, the work on offshoring/outsourcing activities has prompted a broader and more refined research program on the changing nature of global value chains, sometimes referred to as research on the “global factory.” Offshoring scholarship systematically documents how firms are able to disaggregate their value chains into discrete, value-adding activities, each of which may be located in different geographic jurisdictions.

This method is exemplified by Apple’s origin labeling (on most products), which reads “Designed by Apple in California, Assembled in China.” One set of studies in this domain has focused on issues related to governance of the global value chain, which has become an increasingly important topic, as firms are often penalized by their customers for unethical activities for which they are not directly responsible (Griffith & Myers, 2005; Zhang, Cavusgil, & Roath, 2003). Funk, Arthurs, Trevino, & Joireman (2010), for example, show that consumers’ willingness to purchase a product is lower if its parts have been manufactured in animosity-evoking countries, such as Iran or India. Another issue relates to knowledge spillovers, in that MNEs often enable local partners to learn from their activities, as they separate value-chain activities across different locations and with different partners. In turn, this disaggregation of the value chain and decision to outsource low-margin activities to foreign firms has had the unintended consequence that these local partners were able to learn from them, thereby moving up the value chain and eventually competing with the incumbent firms (Kumaraswamy, Mudambi, Saranga, & Tripathy, 2012; Mudambi, 2008), although the empirical evidence is mixed in this regard (Buckley & Verbeke, 2016). Important, recent IB contributions in this context include Gooris & Peeters’ (2016) analysis of the benefits to MNEs that result from fragmenting their business processes, and Kano’s (2017) relational analysis of global value chains.

Taken as a whole, scholarship on offshoring/outsourcing has stimulated voluminous research, and the work on disaggregation and global dispersion of the value chain has allowed researchers to address interesting questions relevant to practitioners and policymakers. In this regard, offshoring research has often been conducted in close collaboration with practice and has informed basic questions related to its costs, benefits, and policy interventions. For example, the Offshoring Research Network established by Arie Lewin at Duke University, in conjunction with academic and industry partners, produced a number of highly relevant and actionable industry reports, in addition to many scholarly contributions that also had clear practical implications (Manning, Larsen, & Bharati, 2015).

Yet this body of research may also be described as introspective, in that it has had limited impact beyond the IB domain. Specifically, this research mainly draws from and extends established IB

theory, especially the OLI framework as articulated by Dunning (1980). One of its key contributions, in contrast to traditional applications of the OLI framework, is to emphasize competencies in organizing and workflow processes, rather than advantages related to production and markets. However, this extension of established IB theories appears to have resulted in IB scholars overlooking relevant work outside their own domain. For example, the research by Gereffi and colleagues on upgrading in global value chains (e.g., Barrientos, Gereffi, & Rossi, 2011; Gereffi, 1999; Sturgeon, van Biesebroeck, & Gereffi, 2008) has rarely been acknowledged by IB scholars. Indeed, the work on offshoring/outsourcing and global value chains inside the IB domain has evolved almost independently from research on the very same phenomena in other disciplines. To illustrate the point, a recent call for papers from a major conference on “Global Production Networks” at the National University of Singapore listed 22 topics for exploration, each of which would fit neatly into IB scholarship’s approach to the topic. The call also listed 16 featured academic speakers, all of whom are well known in this research realm, but few of which are associated with the field of IB or the Academy of International Business (National University of Singapore, 2017).

Understanding How MNEs Respond to Greater Pressures for Social Responsibility and Sustainability in Their Global Operations

From its inception, IB research has demonstrated concerns about the role and responsibilities of MNEs regarding broader societal interests, which is not unsurprising given the history and controversy surrounding the behavior and conduct of MNEs in their foreign operations, including high-profile scandals such as the Nike controversy in the 1990s relating to poor working conditions in contract manufacturing plants, Coca Cola’s controversial use of ground water in India and other locations in which water is scarce, or the more recent Volkswagen emissions scandal. In fact, in her review of the IB research on ethics, social responsibility, and sustainability, Kolk (2016) reports that a 1972 special issue of the *Journal of World Business* had already focused specifically on the United Nations Conference on the Human-Environment, and the broader impact of MNEs on the natural environment.

Most recently, IB research on corporate social responsibility (CSR) and sustainability has taken a

decidedly strategic focus, offering insights and findings that demonstrate how firms can overcome or mitigate risks or broader liabilities through their CSR and sustainability investments and/or disclosures. To illustrate this, Montiel, Husted, & Christmann (2012) investigate how corruption in the institutional environment influences firms’ decisions to obtain third-party certification of private management standards (see also Luo, 2006). They show that, in environments characterized by governmental, policy-level corruption, firms seek private certification (in this case, the adoption of ISO-1400 environmental quality standards in Mexico) to overcome the lack of trust in the government’s ability to regulate them.

In a similar vein, Marano, Tashman, and Kostova examine the link between the institutional deficits in emerging markets and CSR reporting by EE MNEs, finding that “home-country institutional voids push companies to internationalize as a way to escape the institutional constraints and inefficiencies in their own markets, but at the same time create legitimacy challenges for these companies abroad” (Marano, Tashman, & Kostova, 2017: 386). They observe that EE MNEs from less institutionally developed countries are likely to face liabilities of origin – negative perceptions in host countries about these firms’ willingness and ability to conduct legitimate business – and that CSR reporting is an effective strategy to overcome such liabilities and barriers to legitimation. Moreover, Bu & Wagner (2016) report that MNEs’ environmental capabilities and concerns help determine their location choices: while MNEs with greater environmental capabilities are more likely to locate their operations in regions with stricter environmental regulations, MNEs with greater environmental concerns prefer to select the so-called “pollution havens” as FDI destinations. Surroca, Tribo, & Zahra (2013) show that MNEs respond to increased stakeholder pressure to focus on CSR in their home countries by transferring socially irresponsible practices to their foreign subsidiaries: an interesting twist on the interaction between home- and host-country pressures on MNE CSR actions. Henisz, Dorobantu, & Nartey (2014) demonstrate that better stakeholder relations result in better financial performance for the investing firms and reduce the likelihood of challenges to the legitimacy of their projects, underscoring the strategic and economic benefits of stakeholder management in global investment projects.

The increased focus on the role and responsibility of MNEs to broader societal interests has also drawn



attention to the way that they manage stakeholder relationships and interact with local governments. The importance of managing local stakeholder relationships beyond CSR issues has also been illustrated in other studies. Darendeli & Hill (2015), for example, study the factors that enabled MNEs to continue to do business in Libya following the Arab Spring. They show that those MNEs that also nurtured relationships with local stakeholders who had few ties to the then ruling Qadhafi family had a greater likelihood of survival following the overthrow of Qadhafi during the Arab Spring. Similarly, Teegen, Doh, & Vachani (2004) argue that non-governmental organizations (NGOs) have become important actors in the IB environment, influential in their interactions with MNEs, but also in their own right. They contend that, as part of civil society, NGOs have become relevant participants in global governance, but also in the value creation process, providing perspective, intelligence, and insight that could be helpful to MNEs in their international strategies and operations. They further suggest that existing IB theory may not capture the relevance of NGOs to the field fully, and that newer theories, notably co-evolutionary perspectives, could be helpful to understand the role and influence of NGOs in their interaction with business and government in the global context.

These examples demonstrate that IB research has increasingly been considering CSR, sustainability, and stakeholder themes, and doing so in a way that has brought new insight. Specifically, IB scholars have (1) started to unpack MNEs' strategic responses to increased pressure to operate in a socially responsible manner and (2) drawn attention to the importance of government–business–society relationships. In the past, this research has occasionally borrowed – and integrated insights – from allied fields in development studies, political science, and sociology, arriving at novel theoretical insights that have, in turn, contributed to the advancement of the IB field overall. Yet current engagement with scholars in political science, development studies, and allied fields is somewhat limited.

As one example, while IB scholars have become enthusiastic appropriators of institutional theory as developed in sociology, they have not extended this fervor to sociological research on social movement organizations (SMOs) in their examination of MNE–NGO interactions. SMO research has developed and applied specific theoretical constructs

related to “framing processes” (Benford & Snow, 2000), “mobilizing structures” (McAdam, McCarthy, & Zald, 1996), or “repertoires” of actions and “opportunity structures” (Della Porta & Diani, 1999). So, while IB scholars (and management and strategy scholars more generally) have become increasingly interested in the interactions of MNEs and activist groups/SMO, they do not appear to have leveraged these now well-developed constructs from the allied sociology field. This is likely because in investigating MNE–NGO interactions, IB scholars are almost exclusively focused on understanding the MNE's resources, strategies, tactics, and responses, and rarely consider those same characteristics for its organizational counterpart (NGOs/SMOs), even when that counterpart is directly relevant to the constraints and opportunities facing the MNE and, in some instances, shares its international scope and operations with the MNE's (Luca & Doh, 2012).

To summarize, although IB scholars have made some progress in tackling big, important issues and problems, relative to the potential range of topics, these contributions are still somewhat limited, mainly because a great deal of IB research remains self-referential. Furthermore, and perhaps a result of the inward-looking focus of IB work, the extent to which IB contributions are cited and incorporated into broader disciplinary research in business management or in broader social sciences is also limited. In addition, the impact of these contributions on the world of practice and policy also appears to be modest.² In the next section we describe what we see as the potential for IB scholars to tackle grand challenges, and identify some defining characteristics of these challenges. We also highlight where the domain of IB scholarship may need to be broadened and we specify attributes of these challenges that command novel research approaches and the revisiting of extant theory.

THE FUTURE OF IB RESEARCH: EXPLORING GRAND CHALLENGES

That's why we're pursuing... grand challenges... They're ambitious goals, but they're achievable. We're encouraging companies, research universities, and other research organizations to get involved and help us make progress. (Remarks of President Barack Obama on Science and Technology, April 2, 2013)

Quo vadis IB research? Our analysis so far has identified two related problems with IB scholarship:



(1) that it is conducted in relative isolation, with too little engagement with – and contribution to – non-business scholarship in the allied social sciences, and (2) that it does not provide sufficient contributions to solving broader business and public policy challenges. While these problems may result from the IB field itself still being relatively young, and IB scholarship therefore still being concerned with proving its usefulness as an independent discipline, the quest to increase legitimacy as a field may come at the expense of relevance, given that it has resulted in self-imposed constraints regarding theory, method, and overall research approaches.

We believe that one possible means for IB research to address both these shortcomings is to address societies' grand challenges. The grand challenges concept can be traced back to the mathematician David Hilbert who, in 1900, developed a list of 23 unresolved problems in the field of mathematics (Grand Challenges Canada, 2011). The idea of grand challenges was subsequently revitalized by the Bill and Melinda Gates Foundation in 2003, when it described 14 grand challenges. Since then, the Foundation has invested over \$450 million in finding solutions to these challenges (Bill and Melinda Gates Foundation, 2016). Similarly, the United Nations adopted the United Nations Millennium Declaration in 2000, committing member countries to reducing extreme poverty by 2015. Specifically, the Millennium Declaration contains the millennium development goals (MDGs), i.e., eight grand challenges (United Nations, 2016). In 2015, the United Nations announced the successor to the MDGs: the Sustainable Development Goals, 17 objectives designed to help end extreme poverty, fight inequality and injustice, and address climate change. The World Economic Forum has similarly identified ten global challenges, many of which overlap with the MDGs.

Grand challenges are typically not confined to national, economic, or societal borders, but affect societies in a number of geographic locations; as such, they can be seen as a multinational phenomenon by nature. Examples of grand challenges include issues relating to climate change, poverty, migration, terrorism, financial literacy, mass entrepreneurship, and infectious diseases such as HIV. While the importance of addressing questions related to these challenges is seemingly self-evident, we also believe that IB scholars would particularly benefit from studying them and that grand

challenges may have important implications for IB theory. For example, they may alter MNEs' business models and how they create value; the way cross-border operations are designed and organized; the manner in which local employees are incentivized and managed; and the way international strategies are formulated and implemented.

IB research on grand challenges is not entirely unprecedented. For instance, Ansari, Munir, & Gregg (2012) describe how the bottom of the pyramid (BoP) approach can help to mitigate poverty through the transfer of social capital from MNEs to the BoP communities (see also Maksimov, Wang, & Luo, 2017). Oetzel & Doh (2009) review and critique the "spillovers" and liabilities of the foreignness perspective on the impact of MNE investment in host countries; they propose an alternative way MNEs could contribute to development, by pursuing collaborative relationships with NGOs that support host-country development as well as the joint pursuit of MNE and NGO strategic goals.

Yet IB researchers appear reluctant to address the underlying research questions related to grand challenges. This reluctance may be attributable, in part, to the phenomenological nature and sheer complexity of these challenges, characteristics that would require scholars to draw on a diverse set of theories, incorporate multidisciplinary perspectives, and use state-of-the-art (and often multiple) methodological approaches. Another layer of complexity is added by the fact that these grand challenges not only operate at multiple levels of analysis, but also affect MNEs at multiple levels, such as the firm, subsidiary, transaction, and individual level. These difficulties may explain why IB scholars have so far been hesitant to identify and answer research questions relating to these grand challenges as a community. However, it is exactly the multinational nature and complexity of grand challenges that should put IB scholars in a strong position to inform them.

In fact, by studying grand challenges, the IB domain may adopt a broader perspective and – at the same time – develop novel insights that advance our understanding of a set of empirical phenomena that cannot be explained or predicted by traditional IB theories or theories from other disciplines. Specifically, IB scholarship may move away from its historical, narrow focus on the differences between MNEs and domestic firms, and instead leverage existing and new knowledge to contribute to efforts by social and behavioral scientists to make sense of critical global



phenomena. While the world has witnessed tremendous economic, societal, and institutional changes over the past two decades, most of our work is still firmly embedded in the theories developed in the past century. We also believe that a focus on grand challenges would enable IB researchers to move away from the tendency to focus too much on identifying gaps in existing literature, and instead would produce work that keeps abreast of the “real” world. For example, we simply cannot assume that causal mechanisms and boundary conditions that have been identified in the past will remain constant over time – especially in an era characterized by constant change. Studies that consider grand challenges would allow IB researchers to revisit and refine their understanding of how firms manage their international operations, how these firms interact with local and global stakeholders, and, most importantly, what role MNEs play in addressing society’s most pressing issues. It is these insights that would be most likely valuable to other disciplines, policymakers, and practitioners. A focus on global challenges therefore offers a tangible direction for broadening the scope of IB research in a way that could increase recognition by allied social sciences and contribute more directly to broader debates about globalization and its many dimensions.

Taking these points together, we therefore believe there are four important elements to conducting research on grand challenges, each of which requires scholars to relax some of the historic tendencies that are common to IB research and empirical research more broadly.

Grand Challenges are Phenomena-Driven

A common theme in previous contributions that cast doubt on the state of IB research relates to the issue of whether and how IB research addresses real-world phenomena. As Buckley (2002) noted, a weakness of contemporary IB research is the shift away from studies addressing questions that emerge from observations in the world economy, towards questions that arise from theoretical puzzles. While we believe that theoretical development should remain at the core of IB research, it is also vital that IB research have impact, relevance, and a connection to the real world. For instance, in a recent contribution, Doh (2015) explicitly calls on IB researchers to begin their research with a focus on a phenomenon first, and to select the theory and method that best inform that phenomenon second. He laments the tendency of IB (and other management and

business) scholars to offer what are often incremental contributions to existing knowledge, instead of tackling bigger issues and emergent problems. A related argument that has consistently been advanced by IB scholars revolves around the importance of context. Specifically, IB theories often require attention to be paid to the contextual characteristics of the environment in which they are hypothesized to apply. The context may affect the strength, direction, and even the existence of the theorized effect (Buckley & Lessard, 2005).

Grand challenges are by their very nature phenomena-based. For instance, beyond the important question of how MNEs are affected by society’s grand challenges, a more fundamental line of inquiry relates to whether MNEs are part of the solution or the problem (Wright & Nyberg, 2016). We can also observe the emergence of new multinational consortia, combining public, private, and non-profit sector actors (e.g., global health institutions involving the WHO, the Gates Foundation, national governments, and pharmaceutical MNEs) that address grand challenges (Olsen, Sofka, Grimpe, 2016). Such groups are organizational forms that may not be readily explained by existing theory.

Studying these grand challenges may also help to revitalize IB research, as this process could lead to new theoretical insights and research directions. In addition, focusing on phenomena first may open new theoretical avenues for future IB research. For instance, while we have a better understanding of societal demands on MNEs to address climate change concerns, current theorizing tends to treat such concerns as a form of exogenous institutional pressure that prompts adaptation or accommodation by MNEs (e.g., Levy & Egan, 2003; Reid & Toffel, 2009). As such, issues relating to climate change are often “converted into the mundane and comfortable concerns of business as usual” (Wright & Nyberg, 2016). Therefore if IB scholars take grand challenges such as climate change seriously, and want to push the boundaries of IB research, then the field needs to strive to give these phenomena their own identity (Von Krogh, Rossi-Lamastra, & Haefliger, 2012; Merton, 1973). IB researchers have been successful in the past at identifying new phenomena, and giving them an identity that highlights their unique characteristics. Bartlett & Ghoshal (1989), for example, tackle a fundamental puzzle in IB related to balancing the dual pressures on MNE strategy of global integration and local responsiveness. The inevitability that a firm competing internationally needs to reconcile operating

in multiple different local environments with the necessity of overall control of the firm's strategy, represents a critical challenge at the center of IB studies. Bartlett & Ghoshal (1989) respond by identifying and theorizing a new organization form – the transnational enterprise – as a distinct organizational structure, different from other internationally operating firms.

One challenge facing phenomenon-based research is how to establish theoretical patterns within a situation that may be pre- or proto-theoretical. In the past, IB scholars have addressed this quandary by developing “special theories” nested within a general theory. This is an approach utilized in the seminal contribution by Buckley & Casson (1976) – *The Future of the Multinational Enterprise* – which had the following schematic form:

- (1) identify key empirical questions that require explanation;
- (2) construct general theory (of internalization);
- (3) acknowledge that within the general theory, reside nested special theories, where the general theory can be tested at a given time in a given arena (“context”); and
- (4) confront the special theory with the empirical evidence.

This approach may be a useful framework for addressing “new” phenomena, including grand challenges. An example in another context relates to MNEs from EEs. For instance, in Buckley et al. (2007), the special case of China was tested, applying the above schema to the determinants of Chinese overseas FDI. It identified certain anomalies arising from the Chinese context that allowed the refinement of the theory. As a result, the authors were able to uncover the perverse behavior of the “risk” variable, which led to a renewed inquiry into the risk-aversion or risk-assumption of Chinese MNEs and managers.

Grand Challenges Require Interdisciplinary Perspectives

In order to be able to deal with the richness of grand challenges and create meaningful special theories, IB scholars also have to be open to multidisciplinary perspectives (Buckley, 2002; Dunning, 1989; Eden & Lenway, 2001; Lambell, Ramia, Nyland, & Michelotti, 2008; Nielsen & Thangadurai, 2007; Young, 2001). Indeed, for IB scholars to understand and study the fundamentals of important and complex phenomena, greater integration across theories, topics, and methods is necessary to train managers and academics for the world as it is

(Henisz, 2011). Said differently, the “deeply embedded phenomena” of the global environment require “understanding related contextual processes” of multiple disciplines (Cheng et al., 2009: 1072). We believe that this is particularly important for studying grand challenges.

Given that IB is by definition interdisciplinary, it would seem only natural that IB scholarship would leverage this feature and more readily address society's grand challenges. Indeed, a good example of the interdisciplinary roots of IB research is internalization theory (e.g., Buckley & Casson, 1976), which has become a cornerstone of, and one of the most influential paradigms in IB research. Internalization theory achieves this salience by its resolute focus on IB phenomena. The key theoretical innovations were achieved by blending and integrating several concepts – from what are now termed transactions cost theory, the resource-based view of the firm, and entrepreneurship theory – into a unique synthesis: a novel theory of the MNE (Buckley & Casson, 1976). This innovation was not accomplished by a direct importation of perspectives from allied disciplines, but rather by a clear focus on encompassing the phenomenon of the MNE, its strategy, and its external impact (see Narula & Verbeke, 2015; Buckley, 2016 for recent extensions).

Moreover, recent efforts to integrate social science theories (such as institutional theory from sociology) into IB research have also attempted to use IB phenomena to contextualize and reformulate theory; however, the degree of adaptation has perhaps not been as substantial as with internalization. Rather than blend and integrate insights from multiple theoretical traditions to create novel approaches specific to the IB context, some of these efforts have unfortunately been more adoption than adaptation, and may underappreciate and fail to capture the uniqueness of the IB context. The paradox, therefore, is that despite frequent calls for more multidisciplinary research, authors continue to face difficulties publishing such work in the top management journals, given the frequent need to please multiple audiences with often different underlying assumptions and expectations. As a result, IB scholars tend to view broad empirical questions as opportunities for theory testing, thereby applying well-established theoretical lenses borrowed from adjacent disciplines. Such a subservient approach makes it unlikely that, even when focusing on grand challenges, scholars will push the boundaries of IB research.



Indeed, we believe that new phenomena can and will naturally invite new, innovative theoretical paradigms to explain them, some of which may combine and integrate existing theoretical insights in novel and unexpected ways. That is, truly interdisciplinary approaches are most likely to yield the theoretical insights that can really push the boundaries of IB research. According to Kenworthy & Verbeke (2015) and Verbeke, Von Glinow, & Luo (2017), the primary objective of multidisciplinary work in IB should not be the importation of an existing theory from an adjacent field in order to explain IB phenomena. Instead, multidisciplinary work is most valuable when it results in a substantive extension of an existing theory, meaning that this theory then becomes part of mainstream IB research, as was the case of internalization theory described above.³ Furthermore, addressing grand challenges will require new, innovative research designs, including truly interdisciplinary approaches, not only to make grand challenges researchable, but also to do the significance of these challenges justice. It may also be the case that interdisciplinary approaches will press IB scholars to engage with other disciplines in the social sciences that tend to have a more normative standpoint (such as anthropology or international political economy), and that such standpoints may need to be accommodated through interdisciplinary research.

In the context of grand challenges, we therefore envision interdisciplinary approaches that do not simply focus on importing and integrating theoretical perspectives from other disciplines, but instead mobilize interdisciplinary research teams combining expertise and insights from multiple fields (some of which is already happening in areas such as economic geography).⁴ For instance, IB scholars may find it useful to collaborate with health economists or epidemiologists to study the effects of MNE activities on local poverty and health. Similarly, health economists or epidemiologists may also be able to offer alternative explanations for the effect of grand challenges, such as poverty and child mortality, on local employees and subsidiaries, and how MNEs can deal with inequalities across the MNE network. The latter, in particular, may become a critical issue for MNEs, given that their subsidiary networks or value chains often span locations with different levels of economic and societal development, which can create tension within the boundaries of the firm. Here, the idea of competence carriers – i.e., scholars from the

discipline in which the original concept originates, or those with a deep understanding of the phenomena to be studied – is important. Competence carriers can facilitate the flow of ideas across disciplines, and the systematic and accurate combination of insights and ideas in ways that create novel understandings, valuable to both the IB community and other disciplines. Moreover, competence carriers may minimize the risk that the “original nuance” of the imported theory is lost when applied in a new context (Kenworthy & Verbeke, 2015): a problem that also plagues IB research. For example, Michailova and colleagues illustrate this issue by showing that the way in which IB scholars have used the concept of ethnocentrism “nullifies the nuances and rich explanatory potential of the concept” (Michailova et al., 2017: 15).

Grand Challenges Span Multiple Levels of Analysis

Much of IB research transcends specific levels of analysis because the cross-national variation that is intrinsic to IB manifests itself first in region- or country-level differences, and subsequently in industry-, firm-, and potentially group- and individual-level variation. In other words, higher-level variables (region, country) influence lower-level variables that are embedded within them (Mathieu & Chen, 2011). Andersson, Cuervo-Cazurra, and Nielsen, for example, note that while “lower level units share common characteristics and influences from the higher-level units, they are not independent from each other” (2014: 1068). Increasingly, hierarchical linear modeling, in which levels (country, industry, and firm) are nested within each other, has been used to capture the full extent of variation at different levels of analysis and, most importantly, the interdependencies between them (Bryk & Raudenbush, 1992; Aguilera, Flores, & Vaaler, 2007). Peterson, Arregle, & Martin (2012) identify two broad areas of IB research that lend themselves to multilevel methods: international comparative research and research about MNEs themselves. In the former category, they consider research that involves nations, groupings of multiple-nation clusters, within-nation regions, and society-wide data, as amenable to multilevel analysis. In the latter category, they recognize the distinction between globalized and semi-globalized MNEs, inter-organizational effects and clusters within countries, and teams and subsidiaries within MNEs, as potentially subject to multilevel modeling. Building on these insights, Andersson and



colleagues focus on the interaction effects of multilevel modeling: they propose that scholars should “identify the cross-level nature of the moderating relationships, specify the level of analysis of the main relationship and the nested nature of the cross-level influences, and theoretically explain these cross-level influences” (2014: 1063).

In the context of grand challenges, multilevel approaches are most likely needed to study their effects on MNEs and vice versa. Grand challenges, by definition, cross both geographic boundaries and levels. One example would be global migration: at the very macro level, governments (international, national, regional) obviously shape and influence policies related to the movement of people across borders, and face pressures from other governments, alongside industry and NGO stakeholders. At the industry level, trade associations and their members lobby and collaborate to facilitate immigration, while unions and some NGOs may seek to limit it. At the firm level, MNEs engage in recruitment, selection, deployment, and retention of immigrant workers, and at the individual level, executives, professionals, and others engage in choices about where to work, for whom, for how long, and under what conditions.

Furthermore, a multilevel approach may enable us to develop a rich understanding of how grand challenges affect MNEs and vice versa (Buckley & Lessard, 2005). Specifically, the task of IB researchers is to consider the interactions across multiple levels further when studying grand challenges. For instance, at the level of the individual manager, a more diverse outlook will be required (across cultures, genders, nations, and technologies) with a need for new techniques in the face of grand challenges, including managing across the traditional boundaries of “the firm,” networking, orchestrating, and coordinating activities. The multinational firm faces new challenges, including new forms of organization and contract (global information grid or “gig” operations) and political challenges arising from opposition to established and proven business practices, and even free trade and globalization. The political role of the corporation is becoming even more important to its existence and growth, in the face of a society that demands MNEs to become part of the solution to today’s grand challenges (Scherer & Palazzo, 2011). In terms of clusters of firms and sectors, new technology such as 3D printing is reconfiguring industries, both spatially and economically (Laplume, Petersen, & Pearce, 2016). At the level

of the national economy, the evolution of the state, the boundaries of public and private, and the role of state intervention and ownership are all being radically repositioned: there is a greater awareness that new organizational forms – blending public and private initiatives, and spanning multiple geographic regions and even continents – may be required to solve grand challenges (Olsen et al., 2016). Therefore acknowledging that the field of IB is concerned with the entire global system and the subsystems it envelopes, and focusing on multiple levels and the interaction across these levels, can help broaden the scope of much IB research, and acknowledge the complexity of grand challenges as global and interconnected phenomena.

Grand Challenges Involve Interactions among Business, Government, and Society in a Global Environment

At times, IB scholars have been faced with the misconception that IB and MNEs are synonymous. While we agree that work on MNEs has shaped the field and created a space for the IB discipline within the broader management field, there is increasing awareness of the embeddedness of MNEs within their local, regional, and global context (Peng, Wang, & Jiang, 2008). While MNEs therefore remain – and rightly so – the focal point of IB research, its scope has expanded, offering deep and rich insight into the interactions between MNEs and their external environment. For instance, Cantwell, Dunning, & Lundan (2010) describe how MNE activities and external institutions co-evolve over time. As another example, Regner & Edman (2014) examine the factors that enable MNE subunits to shape local institutional contexts to their advantage. IB research therefore already offers important insights into the evaluation and nature of interactions among business, government, and society, which could be leveraged in the context of grand challenges. Given the very nature of grand challenges, the degree to which they affect MNEs, and how MNEs themselves respond to them, also depends on the interaction among business, government, and society. Similarly, effective private-sector efforts to tackle these challenges are also only possible when business, government, and society cooperate.

Take climate change as one example. Scientists and policymakers are increasingly recognizing that climate change is unlikely to be reversed or stabilized in the short term, nor satisfactorily addressed through broad-scale mitigation strategies. Instead,



attention has turned to more targeted efforts to alleviate the impact of climate change and/or initiatives that help systems, communities, and other stakeholders to adapt to its effects. According to the United Nations Framework Convention on Climate Change, adaptation refers to “adjustments in ecological, social, or economic systems in response to actual or expected climatic stimuli and their effects or impacts. It refers to changes in processes, practices, and structures to moderate potential damages or to benefit from opportunities associated with climate change” (UNFCCC, 2016). Although adaptation naturally involves government and non-profit organizations, the private sector has emerged as a key element to adaptation projects (Cohen & Winn, 2007; Dean & McMullan, 2007). However, these private-sector initiatives require close coordination with governments and NGOs to be successful. Therefore examining grand challenges also requires the study of the interaction between business, government, and society. These initiatives must be set up in a climate in which we observe the disintegration of regional organizations (the EU, NAFTA, Brexit). As such, in the evolution, or stalling, of globalization, a host of challenges awaits: is the reaction against freer trade permanent and what are its consequences? Is globalization fracturing and is the global project dead, destroyed by its own patterns of success and failure? These questions are not only important to the field of IB in general, but particularly in the context of grand challenges, because a move towards renewed nationalism will make it more difficult for MNEs and non-traditional organizational forms to address these challenges – or may further accentuate the impact of grand challenges on MNEs themselves.

Focusing efforts on understanding these phenomena can also result in greater insight into how these cross-sectoral interactions can be shaped and influenced and, in so doing, could contribute to addressing these challenges. Early IB research has had an enormous impact on government policies towards FDI and MNEs (both inward and outward), and on international bodies such as the United Nations Conference on Trade and Development (UNCTAD), the United Nations Industrial Development Organization (UNIDO), and the Organisation for Economic Co-operation and Development. Perhaps the best single cumulative contribution of IB scholarship was to the UNCTAD’s annual world investment reports from 1991 onwards (Buckley, 2010), many of which drew directly from IB research and were then translated into policy

recommendations for – and sometimes actual policy changes by – international bodies, individual nations, and regional and city governments. Importantly, many IB scholars have contributed informally and formally to these particular reports as ad hoc, specialized researchers. This impact was replicated in the context of other international bodies (see UNIDO’s successive studies of FDI in Africa; contributions to the annual corruption report from Transparency International) and many individual nations (the work of UK Trade and Investment, the China–Britain Business Council, and many others). These arrangements are common in most host and source countries where FDI is significant. Importantly, the big issues that are situated at the intersection of business and government in the global environment are of high interest to allied disciplines, such as political science, economics, sociology, and others. As such, IB can serve as a bridge among these disciplines, given its inherently interdisciplinary nature and global scope.

In sum, the analysis above suggests that to address grand challenges, IB scholars need to adopt multidisciplinary approaches and develop innovative research designs. Such a trajectory would allow them to explain these complex phenomena involving multiple levels of analysis, as well as the interaction among business, government, and society. Specifically, we have identified several strengths in current IB approaches and tools that can be leveraged to address grand challenges. We have also described weaknesses and offered guidance about the new and different approaches and tools that are needed to study grand challenges successfully. We believe that the IB research community is well positioned to join broader efforts to contribute to understanding society’s most pressing problems and finding solutions to them. Specifically, when considering the unique IB constructs that have guided much scholarly work in previous decades, it is clear that the field will also be able to draw on these rich theoretical insights when addressing grand challenges, in combination with ideas and insights from other social and behavioral scientists. By tackling these grand challenges, and leveraging the research approaches described above, IB scholars can once again contribute to broader interdisciplinary approaches to major global issues, and present tangible, actionable insights that can inform practical actions and solutions for business, government, and civil society. As such, directing IB scholarship towards grand challenges and employing the range of approaches described above could



Table 1 Strengths and weaknesses of current IB approaches and tools in the context of research on grand challenges

	Strengths of current IB approaches and tools	Weaknesses of current IB approaches and tools	What different approaches and tools are needed to address grand challenges?
Grand challenges are phenomena-driven	<ul style="list-style-type: none"> • Approach of creating “special theories” nested within a general theory, in order to establish theoretical patterns within a situation that may be pre- or proto-theoretical 	<ul style="list-style-type: none"> • Shift away from research addressing questions that emerge from observations in the world economy, towards questions that arise from theoretical puzzles 	<ul style="list-style-type: none"> • IB scholars need to strive to give grand challenges their own identity and develop new theory that explains how these grand challenges confront conventional thinking about IB theories
Grand challenges require interdisciplinary perspectives	<ul style="list-style-type: none"> • IB is, by definition, interdisciplinary and therefore much of IB research does, in principle, draw on different disciplinary traditions 	<ul style="list-style-type: none"> • Tendency to simply “import” a theoretical perspective from allied disciplines, rather than blend and integrate insights from multiple theoretical traditions to create novel approaches that highlight the uniqueness of IB perspectives on critical developments in the world economy • IB scholars have viewed new phenomena primarily as opportunities for theory testing, thereby applying well-established theoretical lenses borrowed from adjacent disciplines to these phenomena 	<ul style="list-style-type: none"> • IB scholars need to adopt new, innovative research designs, including truly interdisciplinary approaches to study grand challenges
Grand challenges span multiple levels of analysis	<ul style="list-style-type: none"> • Hierarchical linear modeling, in which levels (country, industry, and firm) are nested within each other, has increasingly been used by IB scholars to capture the full extent of variation at different levels of analysis and, most importantly, the interdependencies between them 	<ul style="list-style-type: none"> • Focus on different levels of analysis, rather than the interdependencies across them 	<ul style="list-style-type: none"> • IB scholars need to focus on the entire global system and the subsystems it envelopes, to acknowledge the complexity of grand challenges as global and interconnected phenomena
Grand challenges involve interactions among business, government, and society in the global environment	<ul style="list-style-type: none"> • IB research has expanded and offered deep and rich insights into the interactions between MNEs and their external environment 	<ul style="list-style-type: none"> • Little focus on the policy implications of contemporary IB research • Lack of understanding of how private-sector initiatives are coordinated with governments and NGOs 	<ul style="list-style-type: none"> • Developing wide-ranging theory on the place of MNEs in a changing global environment

address the “translation” problems introduced at the beginning of this article. IB scholarship could become a bridge across disciplines, a conduit through which insights are applied in real-world settings. Table 1 provides a summary of our perspectives – where IB is well positioned, or less so, to tackle grand challenges – and identifies new approaches that may be required.

What Next? Grand Challenges Ripe for IB Research

The critical next step in the evolution of the IB discipline is that IB scholarly work would become an integral part of the larger conversation in the social sciences. We have focused on research on grand challenges as the way forward, but other work could also push the boundaries of the IB field.



However, grand challenges are extreme phenomena that allow IB researchers to uncover novel mechanisms that may be more difficult or even impossible to identify in more traditional settings (Pettigrew, 1988). In fact, focusing on grand challenges may very well change the way IB scholars do research in two important ways. Internally, it may bring into the mainstream the work of those who undertake valuable research, but who find it difficult to publish their findings in the mainstream IB journals. For example, those who have experience of multi-year fieldwork related to specific firms or IB phenomena – work that is currently often published in lower-ranked journals – may become important members of multidisciplinary research teams, studying these complex grand challenges. An increased focus on grand challenges may also relax some of the assumptions about what constitutes valuable IB research, which, at this point, mainly consists of work that focuses on a narrow range of research questions associated with the nature of MNEs. Instead, grand challenges emphasize the embeddedness of MNEs in social systems and how MNEs influence these systems, and vice versa. Externally, if IB scholars take the lead in efforts by social scientists to solve society's grand challenges, then they can also begin to act as competence carriers, driving the creation of richer work that could stretch into other disciplines. Only then will interdisciplinary approaches generate truly novel insights that could be considered valuable beyond the IB discipline.

In order to illustrate our arguments, we have identified a few potential topics emanating from grand challenges that could potentially be explored by IB scholars. To do so, we returned to the 4Is described above. Each topic can (1) draw on core ideas extant or nascent in IB theory; (2) transcend institutional actors and levels of analysis; (3) integrate the perspectives of multiple stakeholders; and (4) be analyzed using interdisciplinary methods. These topics are anchored in real-world phenomena and can therefore be analyzed at many levels. There is clearly much “bridging” work to do with other communities and across boundaries, whether national boundaries, or epistemological, disciplinary, and institutional ones. Table 2 lists some specific grand challenge-related topics and a number of relevant IB constructs that can be used to inform them, illustrating how addressing grand challenges may revitalize the IB research agenda.

Having identified potential topics IB scholars could and should explore as part of this grand

challenge agenda, in the final section below we offer some practical observations and suggestions for how this agenda could be realized.

DISCUSSION AND CONCLUSIONS: THE ART OF THE POSSIBLE

There have been repeated calls for more relevance in IB research (e.g., Buckley, 2002; Buckley & Lessard, 2005; Shenkar, 2004; Cheng et al., 2009). Overall, there appears to be an increasing sentiment that various external and internal demands have incentivized IB researchers to produce work that is “publishable,” rather than relevant and interesting. The objective of our contribution has been to draw a somewhat more balanced picture of the current state of IB research. In this regard, it is worthwhile reminding ourselves that “breakthrough” publications are often only possible if they can draw on a number of previous studies, each of which may be perceived as incremental when evaluated in isolation. In other words, in the near term, researchers – especially junior researchers – may be subject to pressures, from their immediate academic community and externally, to focus on narrow, highly specialized questions that can, given a limited time period available and a focused skillset (e.g., in the realm of statistical analysis applicable to large databases of firms, which researchers may not really know well, if at all), produce a “contribution” or achieve a publication target. As such, conformity is the rule, rather than the exception, with publications typically drawing on established theory or competing theories, and data that can be obtained at reasonable cost and within a reasonable time frame. Sometimes, this results in small accumulations of knowledge: incremental research findings. When studying important subject matter, this is not to be disparaged. A higher-level scientist may be able to agglomerate these findings into a breakthrough: “unconsidered trifles” can be useful in constructing a narrative of grand theory. A superb example of this is Parker's (2013) *Global Crisis*, whereby previously minor or incremental findings were assembled into a grand narrative and theory of war, climate change, and catastrophe, to explain social, political, and economic change in the 17th century.

Parker (and authors of equivalent grand narratives and theories) would not have been able to craft a convincing explanation unless other scholars, working on incremental findings, had done



Table 2 Important “Grand Challenges” research topics: IB concepts, constructs, and approaches for crafting effective responses

Example research topics	IB concepts, constructs, and approaches (examples)
The political challenge to globalization/understanding the opposition to open trade and foreign investment with its concomitant flows of goods, services, capital, technology, and people	Institutional quality and related perspectives in IB; liabilities of foreignness; trade theory; theories of international integration; regional strategy analysis
Urbanization, changing global demographics, and scarcity and distribution of resources such as water, land, air, and food, and their individual and collective impact on MNEs/MNEs’ potential contribution to addressing them	Macro-level governance and related institutional analysis in IB; integration-responsiveness; international comparative and longitudinal comparisons drawing on history and geography
The rise of the middle class in emerging economies, implications for consumption patterns, and how MNEs can cater to these consumers in a sustainable manner	Foreign entry strategy; comparative and longitudinal studies
The continuing impact of technology and social media on geopolitics, business networks, and civil society, and the use of technology in modern MNEs to organize, distribute, and conduct work across borders	Global value chains; global factory; internalization theory
The growing power of big data and advanced analytics that can be used to track, process, monitor, and influence range of business relationships or interactions/how MNEs can take advantage of these emerging opportunities and safeguard against associated risks such as cyber-attacks	Knowledge-based theories of the MNE; collaborative strategies; global value chains; intellectual property; comparative legal perspectives
Changes in trading relationships resulting from Brexit, the renegotiation of NAFTA, the abandonment of the Transpacific Partnership/Transatlantic Trade Alliance, and their individual and collective impact on MNEs	International political economy; theories of international integration/disintegration
The differing pace of efforts to promote gender equality across countries and how MNEs respond to these country-level differences in their strategy, operations, and employment practices	Institutional perspectives on IB; comparative case analysis; qualitative case studies; “distance”-related scholarship
The impact of base erosion in global operations due to tax differences and tax inversion, and the impact on global strategy, structure, and interactions with governments and public policy matters	Location theory; theories of international finance; internationalization theory; eclectic paradigm
The problem of increasing income inequality within and across countries and how MNEs balance inequality within their network and supply chain	FDI theories; international business and development; international process models

such a diligent job. Following Parker, it is now entirely conventional to look at long-term changes in climatic conditions as fundamental drivers of previously unconnected events. Hymer (1960) performed a similar service for IB scholars, focusing on FDI as a central phenomenon and moving its explanation from a capital movement theory to industrial economics, business strategy, and the new-fangled “international business.” Theories of the MNE also contributed in this manner, by focusing attention onto the MNE as an international organizational actor. Without the diligent work of Dunning (1958), Hymer would have had little phenomenological evidence from which to construct a theory of FDI.

Adopting such a holistic perspective, we have identified and discussed three relative successes,

where the IB community as a whole has addressed important and emerging phenomena in a way that is scientifically valid, practically relevant, and intellectually stimulating. However, while we believe that the approach noted above – that is, assembling incremental findings into a grand narrative and theory – may and should continue to offer a path to groundbreaking theoretical insights, we challenge the IB community to aim even higher. Specifically, we have identified two “translation” problems, related to the lack of practical relevance in our work and the limited spillover of IB theories and concepts to other disciplines. Simultaneously, we also observe that IB scholars have been slow to join efforts by social and behavioral scientists to make sense of critical global phenomena – particularly those that we describe as grand challenges.



Given the need to meet new phenomena-based grand challenges, the task for IB researchers becomes the advancement of the frontiers of knowledge within the constraints of abilities, resources, and institutions. This effort must, of course, maintain a commitment to theoretical and methodological rigor, but it must also go beyond the incremental, seeking to reveal new insights that can illuminate broader global and societal issues and challenges. In particular, we believe that it is critical that IB scholars do not fall into the trap of approaching grand challenges as a welcome opportunity for theory testing, but rather strive to give these challenges their own identity. Moreover, as a community, IB scholars have much to offer in their ability to bring together the best approaches from multiple disciplinary and epistemological traditions, and to leverage this competitive advantage to build new theoretical insights. Yet the IB field has sometimes become beholden to a set of norms and practices that emphasize marginal extensions of existing research, relatively incremental contributions, and sometimes reductionist findings, with an emphasis on “method over phenomenon,” and an unwarranted fear of being drawn into normative debates. Only if IB scholars take grand challenges seriously, accept that these phenomena are distinct, develop innovative research designs, and concede to the sometimes-equivocal nature of their findings, will they develop novel and interesting theoretical insights that are also relevant to society at large. Therefore building on the heritage of John Dunning and Stephen Hymer, among many others, we believe that IB scholars require a widening, rather than a narrowing, of their theoretical and epistemological horizons. IB must become, once again, an aspirational discipline.

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NOTES

¹The objective of our contribution is not to offer a comprehensive literature review of these three areas, nor do we seek to provide a systematic analysis of the intellectual progression of the IB discipline; rather, we use these three examples to illustrate how IB scholars have approached new phenomena, and to discuss the limitations of current IB approaches.

²One interesting exception to this rule was the argument and empirical support from Rugman & Verbeke (2004, among others) and Ghemawat (2007, among others) that most global trade and business is regional, not global. Although never receiving the widespread attention extended to the initial contrary perspective of Friedman’s “The World is Flat,” these contributions did a service in providing a simple empirical correction to a misunderstood and misreported reality.

³Kenworthy & Verbeke (2015) offer a model consisting of seven quality tests to assess the quality of theory borrowing.

⁴The problem of simple importing of concepts from other areas into the IB field is explored by Verbeke, et al. (2017).



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