



COMMENTARY

Identifying the big question in international business research

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Abstract

Buckley (2002) argues that the international business (IB) research agenda may be running out of steam, because no big research question has currently been identified. Buckley also asks whether the field needs a big question, and if so challenges IB scholars to discover it. Buckley and Ghauri (2004) elaborate on the third question of globalization discussed in Buckley (2002) as a possible candidate for the big question. In response, this article is written to take up Buckley's challenge and also to comment on Buckley and Ghauri's more recent work. I agree that IB needs a big question, the pursuit of which can serve to unite and energize scholars, make scientific progress, and enhance the status and prestige of the field. Toward that end, I argue that 'What determines the international success and failure of firms?' has always served as a fundamental research question, which has permeated IB research in the past and present and is likely to propel its progress in the future. Therefore, I am of the opinion that the IB research agenda is not likely to run out of steam, because focusing on this question will leverage IB's comparative advantage and keep the field engaged in generating exciting and disciplined theories and findings in the 21st century.

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Introduction

In a provocative essay titled 'Is the International Business Research Agenda Running Out of Steam?' Peter Buckley (2002) argues that this may indeed be the case. He suggests that past international business (IB) research has succeeded because it has focused on three big research questions which arise from empirical developments in the postwar world economy and which serve to unite the field. These are: (1) how to explain the flows of foreign direct investment (FDI), (2) how to explain the existence, strategy, and organization of multinational enterprises (MNEs), and (3) how to understand and predict the internationalization of firms and the new developments of globalization (Buckley, 2002, 365). Most alarmingly, Buckley (2002, 370) concludes that 'The [IB research] agenda is stalled because no such big question has currently been identified'. Further, he raises the question, 'Do we need a 'big question'?' (p. 370), and ends his essay challenging IB scholars to 'discover a new 'big question'' (p. 371). More recently, Buckley and Ghauri (2004) elaborate on the question of globalization as a possible next big question for IB research.

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In response, this article is written primarily to take up Buckley's (2002) challenge and also to comment on Buckley and Ghauri's (2004) more recent work. While agreeing with Buckley's (2002) review of the postwar IB research agenda, I beg to differ from his conclusion. I first suggest that IB needs a big question. Second, I argue that 'What determines the international success or failure of firms?' has always been the leading question guiding IB research, and will continue to remain so in the 21st century. This differs from the globalization question Buckley and Ghauri (2004) suggest. Finally, this article critiques Buckley's (2002) view which is labeled as 'scholarly mercantilism' and makes a set of recommendations. Overall, in contrast to the bleak outlook of the field that Buckley (2002) has painted, I believe that the big question on the determinants of international firm performance is likely to leverage IB's comparative advantage and propel its research agenda to new heights in the years to come.

Buckley's three 'candidate' big questions

While Buckley masterfully and succinctly summarizes five decades of postwar IB research organized around the three big questions identified above, he has identified three possible contenders for the next big question (2002, 371):

1. 'Can we explain the sequence of entry of nations as major players in the world economy? (Great Britain, USA, Germany, Japan, Singapore, Korea, China)'
2. 'Why are different forms of company organization characteristic of individual and cultural backgrounds? Or is this an artifact?'
3. 'In what empirical measures can we identify trends to (and away from) globalization?'

However, Buckley (2002) is unable to suggest which one is 'it', thus leading to his statement that no big question has currently been identified. Extending earlier work, Buckley and Ghauri (2004) argue that some variant of the third question may contend for the next big question status. While implicit in Buckley (2002), he seems to believe that a big question needs to be broad enough to appeal to most (if not all) IB researchers and yet narrow enough to carve out a distinctive segment in the intellectual marketplace for IB (p. 370). I agree, and based on these criteria, let us go over these 'candidate' questions.

Take a look at the first 'candidate' question, whose unit of analysis is nation. Given that 'IB' is

commonly defined by leading textbooks as 'any firm that engages in international trade or investment' (Hill, 2003, 29; see also, Griffin and Pustay, 2003; Shenkar and Luo, 2004), this question obviously is at a level higher than the firm-level analysis typical of IB research. Although IB research may involve multiple levels of analysis 'from the subindividual to the suprasocietal' (Toyne and Nigh, 1998, 872), firm-level analysis remains at the heart of IB inquiry. While IB researchers are interested in the competitive advantage of nations, our interest mainly builds on the more foundational understanding of how firms and industries within different countries compete (Porter, 1990). In other words, given the higher level of analysis (nation), this question, while fascinating, is not at the core of IB research. On the other hand, this question is at the core of the research agenda for some historians (Diamond, 1997; Kennedy, 1987), political scientists (Wallerstein, 1974–89), and institutional economists (North, 1990). Given that the work of some of these social scientists now traces the origin of competitive advantage to 13,000 years ago (!) since the beginning of the Agricultural Age (Diamond, 1997), it is difficult to imagine how the insights of IB research, even after we break some new ground on the sequence of the rise of different nations starting with Great Britain approximately 300 years ago as suggested by Buckley, can compete in the intellectual marketplace on such long-run historical dynamics.

Buckley (2002) himself rules out whether his second 'candidate' question, on cultural differences, can serve as a big question. He argues that 'issues [related to cultural differences] are perhaps best understood as exemplars of a particularly fruitful methodological approach – the comparative method, rather than as answering particular issues or confronting radically separate agendas and stylized facts' (p. 369). While cross-cultural researchers (such as contributors to Gannon and Newman, 2002) may disagree, I agree with Buckley that this particular question, in itself, is probably unable to become a big question unifying the IB field. The reasons are two-fold. First, in terms of the positioning of the field, pursuing this question (i.e., what are the differences?), as opposed to an explanatory and predictive focus characterizing much social science research, may lead IB studies to become excessively descriptive and less theoretical, thus providing ammunition to critics that IB research lacks rigor. Second, in terms of actual research practice, given the weak and sometimes



confusing conceptualization and measurement of culture (as critiqued by Shenkar, 2001), the relative decline of culture in the IB research agenda has already been documented (Sullivan, 1997). Overall, this question is less likely to be appealing to a majority of IB researchers.

The third 'candidate' question, as phrased by Buckley (2002, 371), is simply an empirical question and not a theoretical one. Even when engaged in, this question's substantive domain, globalization, is not a primary domain of IB (at least historically), but rather is a domain that is shared with fields such as international political economy, economic geography, business and society (or social issues in management), and business ethics (Freeman, 1997). Buckley and Ghauri (2004) now try to push this interesting and increasingly relevant question to the center stage of IB research, and some IB scholars (Doh and Teegen, 2003; Eden and Lenway, 2001) have started to explore these important issues – efforts which I support. Although this question may become more important, it is less likely to become the big question given IB researchers' historically lack of interest in pursuing it and the consequent lack of cumulative literature (Doh and Teegen, 2003; Eden and Lenway, 2001).

Overall, it seems plausible that none of the three 'candidate' questions that Buckley (2002) suggests, including the globalization question Buckley and Ghauri (2004) put forward more recently, can meet the criteria of being the big question for IB. But, this does not mean that IB has no clearcut big question. However, even before we entertain a different big question, we need to address the necessity of having a big question as raised by Buckley (2002).

Does IB need a big question?

Before we proceed to discuss the big question for IB, it seems imperative that the conceptual domain of IB be specified. This specification is nontrivial, and has engendered some significant debate (Boddeyn, 1997; Toyne, 1997). Since spilling further ink on this debate is beyond the scope of the present article, I start with a most basic (and hopefully least controversial) proposition that IB has two essential components: 'international' and 'business'. That is, IB is primarily (but not only) concerned with business activities that cross national boundaries ('international') and that occur at the firm level ('business') (Hill, 2003, 29). In other words, I agree with Wilkins (1997, 32) that 'what research on IB

must consider first and foremost (and what is our unique contribution) is the study of enterprise: the international—multinational—transnational—global business—enterprise—firm—company—corporation'.

While Buckley (2002, 370) asks: Does IB need a big question?, he has already indirectly answered it by noting that IB has experienced some vigorous growth by pursuing three earlier big questions. In fact, his dissatisfaction with the current IB research is associated with the absence of any new big question that he can identify. I agree that IB needs a big question. To the extent that IB aspires to become a scientific inquiry (Toyne, 1997, 64), it is important for the field to reach some consensus on the importance of a big question (or a few of them), the pursuit of which serves to unite (most) IB scholars, make scientific progress, and enhance the status and prestige of the field. Otherwise, a field unable or unwilling to reach such consensus is likely to experience tremendous or even excessive diversity. Wilkins (1997, 41), for example, argues that the danger for letting IB to remain 'an interdisciplinary collage of different approaches' without identifying a core theory and a set of core concepts, engaged by a set of core questions, is that IB may become 'no field at all'. Stopford (1998, 636) suggests that 'if IB needs a unifying theory, then it needs to become narrower in its scope'.¹ A field characterized by a wide scope that is difficult to reach consensus is likely to make little scientific progress and permanently remain in the straight-jacket of a preparadigm stage,² which does not confer status, prestige, and resources in the community of social sciences and business disciplines (Pfeffer, 1993).

While IB's neighboring disciplines, such as management, strategy, and marketing, have often been regarded as in a preparadigm stage (McKinley *et al.*, 1999), they have nevertheless made progress in identifying some of their most fundamental research questions, to which (most) research energy of the field can be channeled (e.g., Rumelt *et al.*, 1994). IB, being more eclectic, has historically been characterized by a significant emphasis on representativeness, inclusiveness, and theoretical and methodological diversity. 'Although these values are attractive ideals, there are consequences for the field's ability to make scientific progress' (Pfeffer, 1993, 599). The primary consequences for IB are likely to be the field's continued classification – by both IB scholars and others – as a preparadigm field with little hope of becoming a more respected discipline characterized by a widely accepted

paradigm and a set of core questions around which (most of) the field's research activities are organized. In general, areas of inquiry do not become distinct scientific disciplines until they adopt a paradigm (Kuhn, 1970), and 'there is no reason to think that IB is an exception to this rule' (Peng, 2001, 822). However, *before* IB (or any field) can move toward a common paradigm, identifying a big fundamental question (or a few of them) is a prerequisite, because 'what defines a field of inquiry such as IB is the type of questions asked' (Hennart, 1997, 645).

There is no doubt that Buckley (2002) has made a significant contribution by articulating the existence and contributions of three big questions which have propelled IB's development. However, in all due respect, I fundamentally disagree with his conclusion that IB is running out of steam for lack of a new big question. That Buckley, despite his prominent status in and significant contributions to the field, is unable to identify a new big question for the current IB research agenda does *not* mean that such a big question does not exist. In contrast, in the remainder of this article, I argue (1) that IB has always had a big question, (2) that Buckley's (2002) three previous big questions can be conceptualized as different aspects (branches) of this 'bigger' question, and (3) that this question will continue to propel the field in the 21st century.

Continuity, novelty, and scope

Fundamental questions 'serve to highlight the issues and presumptions that differentiate a field of inquiry' (Rumelt *et al.*, 1994, 40). Given IB's twin focus on 'international' and 'business' noted above, I argue that 'What determines the international success and failure of firms?' has always been the core question of IB that has served to unite most (but perhaps not all) IB researchers and to delineate IB's boundaries relative to other fields. Given the nontrivial costs associated with the 'liability of foreignness' when doing business abroad, IB researchers have for decades sought to understand the source of competitive advantages possessed and developed by non-native firms in foreign markets (Hymer, 1976, 1960; Peng, 2001; Wilkins, 2001; Zaheer, 1995). McKinley *et al.* (1999) argue that whether a particular school of thought, as exemplified by the pursuit of a core question, gains widespread acceptance depends on its (1) continuity, (2) novelty, and (3) scope. I argue that the question of 'What determines the international success and failure?' entails these three attributes.

First, this question exemplifies a great deal of *continuity*. Although not explicitly spelled out as such, this question underlies the three historical big questions Buckley (2002) has identified. The determinants of the flows of FDI (the first question) boil down to how firms engaging in FDI are able to attain better performance in international markets relative to entries using non-FDI modes such as exporting and licensing (Buckley and Casson, 2002, 1976). The existence, strategy, and organization of MNEs (the second question) center on how these firms overcome the 'liability of foreignness' and outcompete local rivals (Caves, 1996; Dunning, 1993; Hymer, 1976, 1960; Zaheer, 1995). The internationalization of firms (the third question) similarly depends on whether firms can successfully develop and deploy resources and capabilities which contribute to their performance abroad (Johanson and Vahlne, 1977; Peng, 2001). In a nutshell, the pursuit of all these three big questions can be viewed as organized around the *bigger* question of 'What determines the international success and failure of firms?'

Second, this question is sufficiently *novel* so as to engage (most of) the IB field characterized by a wide diversity of disciplinary backgrounds, research interests, and methodological tools. While some IB scholars may argue that they are not particularly concerned with the performance *per se* and that they may be interested in certain IB phenomena (e.g., the existence of institutions and practices such as MNEs), ultimately, the successful, long-term existence of certain phenomena carries strong performance implications in the sense that these institutions and practices (e.g., MNEs) outcompete others (e.g., non-MNE firms trading at the arm's length across international borders).³ Therefore, Hennart (2001, 144) argues that 'A theory of the MNE must also be a theory of why the firm can be efficient'. In other words, to paraphrase the Mahoney theorem (2001, 656), if MNEs were to have a voice, they might have said: 'We outperform other organizational forms in IB, therefore we exist'.⁴

Finally, the question on the determinants of international performance excels in its *scope*. A broad scope helps increase the potential number and variety of empirical tests, leading to a higher likelihood that a coherent stream of empirical research can be established (McKinley *et al.*, 1999). The many possible factors which may influence firms' international performance thus allow for numerous ways of theorizing and testing, resulting in an expanding and cumulative body of



knowledge (e.g., Trabold, 2002). Yet, despite decades of research, we are still far from achieving a complete and definitive answer to this vast, complex, and intriguing question. The rapidly moving events of the global environment, such as the rise of emerging economies as the new IB battleground (Hoskisson *et al.*, 2000; Peng, 2003) and the impact of antiglobalization activities on IB (Buckley and Ghauri, 2004; Doh and Teegen, 2003; Eden and Lenway, 2001), necessitate innovative theoretical perspectives and empirical methodologies to provide new answers to this question and modify old answers (Dunning, 2001). The broad scope of this question ensures that its value is undiminished by the fact that it has not been completely and satisfactorily answered.

Overall, in the same spirit as Buckley and Casson (2001, 91) suggest that 'Investing in new theory is extremely wasteful if existing theory is perfectly adequate', I argue that searching for a new big question may be wasteful if an existing big question on the determinants of international firm performance has enough continuity, novelty, and scope to do the job. This view does not imply that economic geography-related insights as suggested by Buckley and Ghauri (2004) are not important. There is no doubt that IB fundamentally is about a *spatial* perspective on business, that is, why and how to do business outside one's home country. It is important to note, for example, that location (L) is right in the middle of Dunning's (1993) influential OLI paradigm. My point here is that the pursuit for location-specific advantages have always been a defining feature of IB practice and research, which can be well captured by the question 'What determines the international success and failure of firms?' This focus on firm performance is also evident in Buckley and Ghauri's (2004) explication on how MNEs deploy various location strategies to gain competitive advantages.

IB's boundaries, imports, and exports

Another aspect of the big question that Buckley (2002, 370) touches on is its ability to demarcate the boundaries separating IB from other disciplines. While much ink has been spilled on whether IB should have distinct boundaries relative to other disciplines (Boddewyn, 1997; Toyne, 1997), this article focuses on whether the big question identified above helps define the IB field. I believe that the question on international firm performance has the potential to do that, because no other question

better captures both the 'international' and 'business' aspects of IB than this question.

On the other hand, I believe that given today's global economy and increasingly interdisciplinary scholarship, to argue that this question (or in fact, any other question) leads to a domain so unique to IB that it is not relevant to other disciplines is probably indefensible, if not foolhardy. IB's permeable boundaries have historically been a great strength of the field (Bartlett and Ghoshal, 1991; Dunning, 2001; Peng, 2001; Rugman and Brewer, 2001; Toyne and Nigh, 1997), and it is not realistic to believe that IB can now erect a sort of Chinese Wall – by invoking one or a few big questions – that separates itself from other disciplines, now that many other disciplines (e.g., strategy, marketing) have been significantly 'internationalized' (often at the urging of IB scholars!). In our particular case, the performance question of course confronts all firms, domestic and international. Following Hennart (1997), I believe that although the issues which IB needs to focus on may arise in both domestic and international contexts, these issues need to be 'more salient internationally than domestically' (Hennart, 1997, 645). As a result, while other disciplines may have some interest in the question 'What determines the international success and failure of firms?' I am confident that no other discipline is likely to be as passionately interested as IB in the pursuit of this question.

Moreover, I agree with Hennart (1997, 645) in that good IB research does *not* deal exclusively with international phenomena, and that IB contributions are likely to have general applicability beyond the IB field. In other words, there may be no IB-only phenomenon.⁵ IB, fundamentally, is a discipline about 'business' and not merely a discipline about 'international'.⁶ It is interesting to note that Rumelt *et al.* (1994, 564) argue that 'What determines the international success and failure of firms?' is one of the top four fundamental research questions in strategy. This overlap between IB and strategy suggests that IB contributions will not only propel the IB research agenda but will also help strategy tackle one of its most fundamental questions (Bartlett and Ghoshal, 1991). Peng (2001, 820) has argued that some recent IB research, such as work on global strategies, subsidiary capabilities, strategic alliances, and emerging economies, 'is clearly at the leading edge of strategy research, thus helping set the terms for the strategy research agenda'.⁷

Buckley (2002, 370) observes that 'In its successful era, IB researchers not only imported concepts and paradigms, they also exported them to neighboring areas', and goes on to suggest that 'This does not seem to be occurring at the moment'. At the moment, it probably is true that IB has experienced a 'trade deficit' in its scholarly exchanges with other disciplines. In other words, the scale and scope of IB's intellectual 'exports' to neighboring disciplines are less than IB's intellectual 'imports', and many IB scholars (including Buckley) naturally would like to see IB's 'export' market share in the intellectual marketplace increase. However, to argue that IB does not export at all fails to acknowledge IB's emerging influence in the social science research enterprise (Markusen, 2001, 74). There is no doubt that IB has made numerous empirical contributions (Kogut, 2001). In fact, Buckley (2002, 370) posits that 'One response [to the criticism on IB's lack of 'export' market share in the intellectual marketplace] is to argue that IB is defined by its distinctive methods'. Again, I disagree with this argument, because it downgrades IB to an empirical branch of other disciplines and suggests that IB can go on without developing its own theoretical basis. This is no less than accepting the criticism of non-IB scholars (and, unfortunately, of some IB scholars) that 'IB has no theory'. In the long run, the very existence of a discipline without a theoretical basis, if pushed to the extreme (especially during times of resource and budgetary hardship), may be endangered (Pfeffer, 1993). This probably is not the destiny of the IB field that Buckley and other concerned IB scholars (e.g., contributors to Rugman and Brewer, 2001; Toyne and Nigh, 1997) would like to see.

While there may be other examples of IB 'exports', I use two recent examples – one *macro* and the other *micro* – of how IB research has been 'exported' to a major source of IB 'imports', economics, to refute Buckley's argument above. In the macro area, IB's development of an internalization theory of the MNE, pioneered by some of Buckley's earlier work (Buckley and Casson, 2002, 1976) and later articulated by Dunning (1993), Hennart (1982), Rugman (1981), and other IB scholars, directly contributes to traditional economic theory, which previously had only regarded FDI as an export of capital as opposed to a control vehicle to reduce transaction costs (Markusen, 2001). Further, this IB theory is not a mere application of Williamson's (1975) transaction costs economics (TCE) framework (Kogut, 2001).

Instead, the internalization theory of the MNE 'antedates it [TCE] and has proceeded quite independently even though Williamson's influence has subsequently been significant' (Hennart, 1997, 647; see also, Hennart, 2001, 132; Kogut, 2001, 787).

In the micro area, IB scholars have drawn upon a well-established cross-cultural literature on individualism and collectivism (Hofstede, 1980; Triandis, 1995) to inform TCE research with a focus on its core assumption: opportunism (Chen *et al.*, 2002). Although TCE scholars never assume that all individuals are opportunistic all the time, they have nevertheless built a theoretical framework based on an underdeveloped assumption of opportunism, citing the inability to differentiate opportunists, who may be a minority in any given population, from non-opportunists *ex ante* (Williamson, 1975). Equipped with empirical evidence from IB research that individuals in different cultures exhibit different opportunistic propensities, Chen *et al.* (2002) suggest a cultural perspective on TCE. They specify that individualists may have a higher opportunistic propensity in *intra-group* transactions and collectivists in *inter-group* transactions. Chen *et al.* (2002) maintain that indiscriminately assuming an equal level of opportunism may explain critics' dissatisfaction with TCE and, more importantly, may backfire when firms attempt to contain opportunism based on this assumption in operations around the world. Clearly positioned as an IB 'export', the aim of this work is to 'help TCE to more effectively accommodate some criticisms and more realistically deal with problems of economic organization in today's global economy' (Chen *et al.*, 2002, 567).

Overall, Buckley (2002) may have exhibited a tendency which can be labeled as 'scholarly mercantilism', characterized by his advocacy for having possibly sealed and protected boundaries for IB set by whatever 'big questions' the field can establish, interest in seeing more IB 'exports' than 'imports', and frustration with IB's 'trade deficit' in scholarly exchanges.⁸ Although this is a very natural and intuitive tendency, I believe that Buckley (and other IB scholars who share his view) can do better, because *every* IB textbook has convincingly indicated that mercantilistic thinking, characterized by its zero-sum mentality in favor of more exports and less imports, has become outdated (e.g., Griffin and Pustay, 2003; Hill, 2003; Shenkar and Luo, 2004). According to the theory of mercantilism, the United States, which has the world's largest trade deficit, should have the world's



lowest standard of living; but instead, it has enjoyed one of the world's highest. It is evident that the theory of mercantilism does not work in this case. Similarly, in today's increasingly integrated global economy where national boundaries are significantly permeated, what counts for a nation's trading position is not its absolute advantage in pushing its exports around the world; instead, it is its *comparative* advantage that determines how it can contribute to the global economy (Porter, 1990; Ricardo, 1967 (1817)). Consequently, there is reason to believe that IB as a field can thrive by leveraging its comparative advantage (Shenkar, 2004).

Recommendations

It is the contention of this article that IB's comparative advantage lies in the interest and expertise in the pursuit of the question: 'What determines the international success and failure of firms?' Therefore, I believe that if IB scholars truly believe and practice some of our own teaching, it is plausible to recommend the following:

1. IB's boundaries should remain reasonably open for more vibrant and beneficial scholarly exchanges, just as national boundaries should remain the same if nations aspire to be contributing members of the global economy. Intended to bring out the best of IB and other disciplines in a Ricardian sense for 'enhanced global welfare' of the research enterprise (Dunning, 2001, 62), this 'pro-free trade' position is not necessarily driven by the default position that today it is practically impossible to maintain closed boundaries for the IB field (or for national boundaries). In other words, IB scholars need not complain too much on non-IB scholars's 'encroachment' or 'expropriation' of IB research. As Boddewyn (1997, 642) paraphrases the Gospels: 'there are many rooms in the IB mansion'. When non-IB scholars enter the vast IB mansion through different entrances, IB scholars should welcome these new entrants and seek to 'convert' some non-IB scholars to become members of the IB family (Toyne and Nigh, 1998, 869).
2. As long as IB scholarship is generating some respectable 'exports' to other disciplines, IB's 'trade deficit' is *not* a grave concern and we need not be too nervous about it. Instead, it motivates us to work harder and smarter (Kogut, 2001, 811). Scholars who are worried that IB may experience a permanent 'trade deficit' perhaps

need to revisit Vernon's (1966) insights on the changing trade pattern among nations during different product life cycle stages and Porter's (1990) account on how different forms of the national 'diamond' can interact to facilitate a global expansion of a nation's industries (or, if we may, a discipline's intellectual products). While IB is currently in a preparadigm stage and experiences a 'trade deficit', it is not hopeless in possibly moving toward a more paradigmatically developed field and changing its 'balance of trade'.⁹ In short, IB scholars need to be in the game, play hard, and aim high. Stated differently, 'IB researchers need not be afraid of the potential competition of other disciplines, nor should they have any inferiority complex about the field's theoretical achievements' (Hennart, 1997, 651) and 'methodological accomplishments' (Kogut, 2001).

3. Although perhaps IB may not have an absolute advantage in competing against some of the more established disciplines, it nevertheless has a *comparative* advantage in carving out an intellectual space in the community of social sciences and business disciplines (Shenkar, 2004).¹⁰ Toward that end, I believe that pursuing the question, 'What determines the international success and failure of firms?' best capitalizes on IB's comparative advantage in continuing with its past tradition and present trajectory, providing sufficient novelty as a unique (but not closed) field, and maintaining a broad scope to facilitate a cumulative body of theoretical and empirical knowledge. Therefore, I agree with Hennart (1997, 651) that 'IB will continue to develop as a viable and distinct field of inquiry, and that the fears that it might get absorbed by more theoretical disciplines (such as economics or organization theory) are exaggerated'.

Conclusions

There is no doubt that Buckley (2002) has made a significant contribution by challenging the IB field to think hard about itself. Yet, in all due respect, this article begs to differ from his conclusion that the IB research agenda is running out of steam. Given that Buckley himself recently has stated that 'the [IB] research agenda is still being actively developed' (Buckley and Casson, 2002, x) and that his more recent work on globalization (Buckley and Ghauri, 2004) has sought to develop it, his

conclusion in the 2002 essay is puzzling. In contrast, I argue that 'What determines the international success and failure of firms?' has always served as a fundamental research question for IB. Although it is true that not every IB scholar directly answers this question, I believe that the various research questions IB research seeks to address all relate to it in one way or the other. Explicitly focusing on this question may help the field better organize its research activities, reach some consensus (as a first step, on what the big question is as opposed to on what the answers are), and strive to become a more mature discipline organized around some paradigm(s). Theoretical and methodological diversity is still encouraged as long as there is some agreement on fundamental questions, such as the one suggested here, and 'on a set of rules to winnow the measures, methods, and theories on the basis of accumulated evidence' (Pfeffer, 1993, 616). In other words, while creativity and imagination are commendable in the research enterprise, what IB needs is *disciplined* imagination (Stopford, 1998; Weick, 1989). Overall, I am of the opinion that the IB research agenda is not likely to run out of steam, because focusing on the international firm performance question will leverage IB's comparative advantage and keep the field engaged in generating exciting and disciplined theories and findings in the 21st century.

In conclusion, I agree with Buckley (2002, 370) that 'the way forward is, paradoxically, to look back'. It is exactly by looking back have we discovered the long-run core question on 'What determines the international success and failure of firms?', the pursuit of which has permeated the IB research agenda in the past and present, and will likely continue to propel it in the future. While not everyone will agree with the big question identified here, if this article, like Buckley (2002) and Buckley and Ghauri (2004), can provoke more debate on what IB's big questions are, then my purposes for joining this debate will have been well served.

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Notes

¹On the other hand, Sullivan (1998) argues that IB may suffer from a 'narrow vision'. Nevertheless, it is widely agreed by many IB and non-IB scholars that the scope of IB is *wider* than that of many other business disciplines.

²See Kuhn (1970) for an influential discussion on the difference between a paradigm stage and a pre-paradigm stage in the development of scientific disciplines. IB does have several paradigms, including Dunning's (1993) 'eclectic' paradigm and the three paradigms identified by Toyne and Nigh (1998), namely, extension, cross-border management, and evolving interaction paradigms. What the field seems to lack is a *unifying* paradigm.

³Wilkins (2001, 23), for example, notes that 'The business historian is interested in what constitutes 'advantage' over time in the spread of international business ... This brings business historians to the question of performance'.

⁴There is a parallel debate in transaction cost economics (TCE) in that some scholars argue that the original development of TCE does not focus on firm performance; rather, it focuses on governance choices (Williamson, 1975). However, governance choices have clear performance implications in that firms that choose the most appropriate governance structure will encounter the lowest transaction costs and, hence, attain the highest performance (theoretically at least). Therefore, firm existence and firm performance cannot exist independently; instead, they are intertwined (Mahoney, 2001).

⁵My argument on there may be no IB-only phenomenon is similar to the following argument made by Casson (2000, viii) on the relationship between economics and non-economics disciplines: 'In truth, it seems that there is no purely 'economic' aspect to social science phenomena, and conversely, no purely 'non-economic' aspects either'. However, this does not prevent economists from choosing research questions and topics that are more salient to economics than to other non-economics disciplines, and *vice versa* (Casson, 2000).

⁶This is consistent with the publishing strategy of many IB scholars: While they often publish their work in the *Journal of International Business Studies* and other IB journals, they are also interested in publishing (or, if we may, 'exporting') their research in other non-IB, 'mainstream' business journals (Inkpen, 2001).



⁷It is interesting to note that among many leading business schools in North America (e.g., Illinois, Ivey, Michigan, NYU, Ohio State, Washington, Wharton), Europe (e.g., INSEAD, LBS), and Asia (e.g., CUHK, HKUST, NUS), IB and strategy groups are often housed in the same management department (Peng, 2001, 822).

⁸Buckley is not alone in this regard. Toyne and Nigh (1998, 870), for example, complain that 'IB borrows ['imports'] too much [from other disciplines]'

⁹Political science serves as an example of how a discipline can be transformed from a preparadigm stage to 'probably one of the more paradigmatically developed social sciences' in the last three decades (Pfeffer, 1993, 615).

¹⁰This intellectual space is more than a 'niche'. For example, at the Academy of Management that, as of August 31, 2003, has 13,733 members and 24 divisions and interest groups, the International Management Division is the sixth largest division with 2261 members (16% of all members).

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