

A UNIFIED FRAMEWORK FOR INTERNATIONAL BUSINESS

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January 2009

[*Acknowledgment*] This research was supported in part by a National Science Foundation CAREER Grant (SES 0552089) and a Hong Kong Research Grants Council Grant (CUHK4650/06H). Earlier versions of this paper was presented at the University of Sydney (December 2007), Western Washington University (May 2008), University of Hong Kong (July 2008), and National Taiwan University (December 2008). We thank Christine Chan, Cheng-Min Chuang, Hong-Jen Chiu, Lorraine Eden, Chuck Kwok, Steve Globerman, Syd Gray, Jongwook Kim, Tatiana Kostova, Tom Roehl, Yu-Shan Su, Ben Tipton, Gracy Yang, and seminar participants for their helpful comments. All views expressed are ours and not those of the funding agencies.

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[Abstract]

International business (IB) is known for its diversity and consequently its lack of central frameworks unifying the discipline. At AIB50/JIBS40, it is time to "stand and deliver." In this spirit, we argue that a unified framework has emerged. Our proposed fundamental question is: What determines the success and failure of firms around the globe? To address this question, IB literature can be broadly organized along two core perspectives: (1) the institution-based view and (2) the resource-based view. We suggest that this framework helps clarify the direction of our discipline, acknowledges its intellectual roots, and moves the discipline ahead.

As the Academy of International Business (AIB) celebrates its 50th anniversary and the *Journal of International Business Studies (JIBS)* celebrates its 40th, international business (IB), as an academic discipline, can legitimately claim a proud history of 40-50 years. As a result, IB is no longer young. Yet, we continue to debate among ourselves and with non-IB colleagues: Is IB a “discipline”? If so, what is the shape and form of the IB discipline?¹ We suggest that part of the reason behind the continuous debate regarding whether IB is a discipline is IB’s lack of unifying frameworks that may eventually lead to a paradigm (Boddeyn, 1997; Peng, 2004; Rugman, 2008; Shenkar, 2004; Toyne & Nigh, 1997, 1998; Wilkins, 1997, 2008). In general, areas of inquiry do not become distinct 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). There seems to be some consensus among both IB scholars (Toyne & Nigh, 1997: ix) and non-IB colleagues (Hambrick & Chen, 2008: 49) that IB lacks central frameworks. A field without some central frameworks, by definition, is fragmented and “does not share the consensus characterizing more paradigmatically developed disciplines” (Pfeffer, 1993: 608). Consequently, such a fragmented field may be subject to attacks and challenges from more established disciplines (Pfeffer, 1993).

In response, we argue that a unified framework for IB has emerged, consisting of one fundamental question and two core perspectives (Figure 1). Our proposed fundamental question is: What determines the success and failure of firms around the globe? To address this question, the vast IB literature can be broadly organized along two core perspectives: (1) the institution-based view and (2) the resource-based view. It is our contention that the identification of this framework helps define the IB field by channeling the attention and energy of scholars, students, and practitioners to a certain direction. We believe that this framework helps clarify the direction

¹ One of the most recent and most public debates took place in summer 2007 over the AIB net, which was initiated by John Mezias of the University of Miami and generated over 50 responses from AIB members.

of our discipline, acknowledges its intellectual roots, and energizes most (if not all) IB scholars as the discipline moves ahead.

---Insert Figure 1 about here---

It is important to note that our proposed framework, an attempt to help unify the IB field, is *not* positioned as a paradigm. Following Porter who also uses the word “framework” (for his five forces framework), we believe that “frameworks identify the relevant variables and the questions that the user must answer in order to develop conclusions” (Porter, 1994: 427). Our proposed unified framework builds on and extends a series of previous work that takes on the big question (Peng, 2004) and leverages the two core perspectives (Peng, Wang, & Jiang [2008] on the institution-based view in IB and Peng [2001] on the resource-based view in IB). Having cumulatively constructed the components, we believe that time is ripe to push this work further by proposing a unified framework for IB building on these components.

All theories and frameworks simplify to make a point. We are clearly aware that the framework we are proposing, due to its inevitable simplification,² may be controversial and contentious to some scholars, and we certainly welcome critiques and debates. However, we wish to avoid debates on our underlying assumptions and would prefer debates focusing on our proposed framework. As a result, we proceed by outlining our most basic (and hopefully least controversial) assumptions. Then we spell out the fundamental question and the two core perspectives. Next, we discuss four areas of IB research as examples of how the proposed framework may help unify these diverse areas. Discussion and conclusions follow.

² Einstein once said, “Everything should be made as simple as possible, but not simpler” (cited by Butler, 2006: 7).

ASSUMPTIONS

This section outlines our three basic assumptions. Obviously each of these is debatable and we acknowledge that there are lively debates for them. However, debating them is beyond the scope of this article.³ Because our framework is built on these assumptions, it is important to explicitly discuss them.

IB = I + B

We assume (1) that as a discipline, IB has two components—international (I) and business (B)—and (2) that the unit of analysis of IB research is primarily (but not exclusively) at the firm level (or the B level). In other words, “what research on IB must consider first and foremost (and what is our unique contribution) is the study of enterprise” (Wilkins, 1997: 32). This is well summarized by the current Editor-in-Chief of *JIBS* Lorraine Eden in her inaugural editorial: IB interests are “in I + B” (Eden, 2008: 3).

While such a basic focus on the firm level may seem non-controversial at first glance, it has been criticized as being “unnecessarily restrictive” by Toyne and Nigh (1998: 874), who go on to argue that IB needs to involve “multiple levels of analysis from the individual to the suprasocietal” (p. 872). Our view is that while there is indeed a great diversity of IB research covering all levels of analysis suggested by Toyne and Nigh (1998), the starting point to conduct such multilevel research is an interest in the firm—the B part in IB.

IB Research ≠ MNE Research

Having argued that IB fundamentally is about the study of enterprise (a point, as noted above, we agree), Wilkins (1997: 32) spells out what kind of enterprise she has in mind: “the international-multinational-transnational-global business-enterprise-firm-company-

³ See Toyne and Nigh (1997) for a collection of these debates. Pfeffer’s (1993: 618) characterization of basic debates in organizational science seems valid for basic debates in IB: “They are repeated periodically, often covering the same ground.”

corporation”—in short, the multinational enterprise (MNE). We disagree with this point. While MNE research represents one of the most insightful and most influential lines of IB research (Collinson & Rugman, 2007; Dunning & Pitelis, 2008; Hennart, 2001; Hymer, 1960/1976; Roth & Kostova, 2003; Rugman & Verbeke, 2004), recent consensus seems to be that IB research is much broader than MNE research (Shenkar, 2004; Toyne & Nigh, 1998).

Non-MNE, but IB-active firms certainly exist. Examples include born-global firms (Knight & Cavusgil, 2004), exporters (Peng & York, 2001), and outsource service providers (Doh, 2005). More importantly, IB also needs to consider the response of *domestic* firms. Traditionally IB has been one-sided, focusing on the foreign entrants (often the MNEs) while ignoring the other side, the domestic firms. Of course, the other side does exist and it does not just sit around (Bruton, Dess, & Janney, 2007; Zhou & Li, 2007). Facing foreign entrants, domestic firms actively compete and/or collaborate with foreign entrants (Dawar & Frost, 1999). Such domestic firms clearly deserve the attention from IB researchers. As IB competition intensifies around the globe, IB needs to be serious about the B part—consisting of not only the international (cross-border) foreign entrants but also the domestic firms. In other words, we assume that IB research is more than MNE research. It is more accurately characterized as business (B) research that not only involves MNEs but also (1) non-MNE but IB-active firms and (2) domestic firms whose action may affect the success and failure of foreign entrants.

IB Needs a Central Framework

IB is fabled for its richness, diversity, and complexity. Dunning (2001: 62) writes that “IB, after all, comprises an eclectic set of related disciplines, and this is one of its main strengths”—note the *plural* for “disciplines.” Does IB need a central framework? Dunning’s (1993) eclectic framework certainly signals a “yes” on this question. However, since (1) Dunning’s work explicitly focuses on MNEs (see Dunning & Lundan [2008] for a most recent

extension) and (2) MNE research, as noted above, does not cover the entire IB universe, it is widely acknowledged that at present, IB does not have a central framework (Stopford, 1998; Sullivan, 1998; Toyne & Nigh, 1997; Tsui, 2007).

Why does IB need a central framework? Wilkins (1997: 41) argues that the danger for letting IB to remain “an interdisciplinary collage of different approaches” without identifying a central framework is that IB may become “no field at all.” 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 straightjacket of a preparadigm stage,⁴ which does not confer status, prestige, and resources in the community of social sciences and business disciplines (Pfeffer, 1993). “While the lack of an organizing conceptual framework has not stopped the IB field from advancing,” according to Sullivan (1998: 879), “it certainly has slowed progress (and, in some cases, fueled the burnout or provoked the defection of once interested scholars).” Therefore, to facilitate further progress on the development of the IB discipline, we assume—and do not wish to debate—that IB needs a central framework.⁵

IB’s richness, diversity, and complexity have defied efforts to unify the discipline.⁶ Behrman goes as far as saying that “the IB researcher is not prepared to develop an integrated theory.”⁷ However, as George Homan writes in *Nature of Social Science* (1967),

Sooner or later, we must stop “being about” to talk about something and actually say it . . . We are always getting around to saying something we never actually come out with. But sooner or

⁴ See Kuhn (1970) for the difference between a paradigm and a preparadigm stage in the development of disciplines.

⁵ We also assume that IB needs one (or a few) big question(s) (Butler, 2006). Since Peng (2004: 101) makes extensive arguments supporting this point, we do not elaborate it here. A field characterized by disagreements over such basic issues as whether it needs a central framework and a (or a few) big question(s) “will almost certainly be unable to make progress of any consequence” (Pfeffer, 1993: 616).

⁶ IB does have several proposed 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* framework.

⁷ Jack Behrman, a co-founder, past president, and fellow of the AIB, made these remarks as part of the panel discussion at a major conference in 1992 (whose proceedings were published as Toyne & Nigh, 1997—see p. 662).

later a science must actually stick its neck out and say something definite . . . **STAND AND DELIVER!** (Homan, quoted by Feldman, 1997: 446, original italics and capitalization)

As AIB celebrates its 50th anniversary and *JIBS* its 40th, we believe that “it is our job, now, to stand and deliver”—a point that Sullivan (1998: 883) made ten years ago and that few have followed up. It is with this spirit that we put forward the following unified framework for IB, despite the inevitable risk and criticisms that our endeavors are likely to encounter.

ATTRIBUTES OF A FRAMEWORK

As part of the theoretical structure of a discipline, a framework needs to demonstrate both comprehensiveness and parsimony. Given the inherent diversity of IB, Stopford (1998: 636) suggests that “if IB needs a unifying theory, then it needs to become narrower in its scope.” In other words, parsimony can only be achieved with a relatively narrower scope that may lead to a reasonable consensus on its focus (Pfeffer, 1993). However, the challenge for us is that it cannot be too narrow. Any framework we propose also needs to be sufficiently broad and comprehensive that will make most (if not all) IB scholars comfortable and excited.

The aim of building a framework is “to speculate, discover, and document, as well as to provisionally order, explain, and predict” processes and structures that characterize IB (Van Maanen, Sorensen, & Mitchell, 2007: 1145). While there are many ways to build a framework, we believe that for IB, the best way forward is to start with a fundamental question around which the field can rally (Peng, 2004). Further, we believe that any single theoretical perspective may not be able to address such a big question. On the other hand, introducing too many theoretical perspectives into the framework will certainly make it less parsimonious and less focused. Overall, our unified framework suggests that there are two core perspectives that combine to shed light on the big question. Each of these three components of our framework is outlined next.

ONE FUNDAMENTAL QUESTION

What is it that we do in IB? Why is it so important that practically every student in a business school around the world is either required or recommended to take an IB course?

While there are certainly a lot of questions we can raise, a relentless interest in “What determines the success and failure of firms around the globe?” serves to unite the energy of our field (Peng, 2004). Overall, the focus on firm performance around the globe, more than anything else, defines the field of IB. The Appendix shows that a majority of the most influential (most cited) papers published in *JIBS* address this question. Numerous other questions all relate in one way or the other to this most fundamental question (Butler, 2006). Therefore, following Peng (2004), we argue that the primary focus of our field is to answer this question—What determines the success and failure of firms around the globe?

We can anticipate some of the likely reactions, and we believe that we can address them. These reactions would include: (1) This is a strategy question and (2) There is more to IB than a focus on firm performance. Let us respond in turn.

Is the Firm Performance Question a Strategy Question Only?

There is no doubt that strategy is interested in firm performance (Ricart et al., 2004) and the question we suggest above is “clearly appropriate for international strategic management scholars” (Eden, 2008: 2). At issue, then, is whether this is a strategy question *only* and whether IB (which has many non-strategy components) has a legitimate claim to this question. We believe that strategy does not and can not monopolize the firm performance question. In fact, every discipline in a business school cares about firm performance—this is what distinguishes business disciplines from traditional social sciences (such as sociology), which do not care about firm performance. For example, the American Marketing Association (AMA) defines marketing

as “an organizational function and a set of processes . . . that *benefit* the organization and its stakeholders”—clearly indicating an interest in enhancing firm performance (www.marketingpower.com). Human resource management (HRM) has always maintained a focus on organizational effectiveness and firm performance (Lawler, 2005). Even business history deeply cares about “the question of performance and how it is measured” (Wilkins, 2008: 23—see also Jones & Khanna, 2006).

Does IB have unique advantage to pursue this question: “What determines the success and failure of firms around the globe?” We believe so because this question not only deals with firm performance, which attracts interests from virtually all business disciplines (the B part), but also cares about such performance *around the globe* (the I part). This clearly speaks to the importance of *local* (or context-specific) knowledge, often built from research in one country after another, that no other discipline is as passionately interested as IB (Shenkar, 2004; Tsui, 2007). Although not spelled out as such, this question underlies much of IB research efforts in the past four to five decades (Peng, 2004: 102). It can also help focus IB efforts in the future.

There is More to IB than a Focus on Firm Performance

It is true that firm performance is not the dependent variable for every IB study—it would have been very boring if this were so. For example, some scholars are interested in the international environment, and others would study MNEs as an organizational form, in and of itself.⁸ However, IB’s interest in the international environment is driven fundamentally by an interest in knowing how to channel this knowledge to help firms compete and perform better (Henisz & Delios, 2001; Makino, Isobe, & Chan, 2004; Tong, Alessandri, Reuer, & Chintakananda, 2008). Otherwise, there is no difference between research in IB and research in

⁸ Private communication to the first author from Professor Lorraine Eden, September 18, 2007.

political science, sociology, and area studies—these other disciplines primarily deal with the environment but do not care about firm performance.

Likewise, studying MNEs as an organizational form (Roth & Kostova, 2003) carries strong performance implications in the sense that a certain organizational form, the MNE, outcompetes others (such as non-MNE firms trading at arm's-length across international borders). 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, the very existence of MNEs as an organizational form is indicative of its more superior performance—relative to non-MNEs—in value creation. 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.”

In summary, of the “garden variety” of IB research, we believe that almost all such research, deep down, is fundamentally interested in firm performance. It will be challenging to find a group of IB scholars who claim that they do not care about firm performance.

FIRST CORE PERSPECTIVE: AN INSTITUTION-BASED VIEW

For decades, IB scholars have labored on issues associated with the environment, such as context, culture, and political risk. We argue that to consolidate this literature and to theorize further, all these lines of work associated with the external environment of the firm can be labeled an *institution*-based view. Institutions are broadly defined as the “rules of the game” (North, 1990). Doing business around the globe requires intimate knowledge about the formal and informal rules governing competition in various countries. It is difficult to imagine firms that do not know the rules of the game in a certain country will emerge as winners. In a nutshell, an institution-based view suggests that the success and failure of firms around the globe, at least in part, are enabled and constrained by the different rules of the game (Peng et al., 2008—see also

Chang, 2006; Dunning & Lundan, 2008; Kotabe & Mudambi, 2003; Rodriguez, Siegel, Hillman, & Eden, 2006).

Several streams of IB research, such as those dealing with cross-cultural differences in values and norms, reflect a deep interest in the *informal* institutions governing individual and firm behavior (Chen, Peng, & Saporito, 2002; Hofstede, 2007; Leung et al., 2005; Redding, 2005; Salk & Brannen, 2000; Woldu, Budhwar, & Parkes, 2006). Other streams of research, focusing on political economy and institutional economics, tend to concentrate on the *formal* aspects of institutions (Globerman & Shapiro, 2003; Henisz & Delios, 2001; Hill, 2007; Lee & Oh, 2007; Lee, Peng, & Barney, 2007; Rodriguez, Uhlenbruck, & Eden, 2005; Singh, 2007; Vaaler, Schrage, & Block, 2005; Wan & Hoskisson, 2003). Collectively, these streams of research enhance our understanding of the environment (context) that firms confront around the globe (Peng, 2003). This focus on institutions helps ensure that IB continues to appreciate research focusing on local (context-specific) knowledge and guards against the Western- and US-centric mentality typical in other disciplines (Meyer, 2006, 2007; Tsui, 2007).

This institution-based view of IB can be positioned as part of the broader intellectual movement centered on new institutionalism throughout the social sciences in recent decades. One interesting point to discuss is: Why do we use the particular label an “*institution-based* view”? Is it the same as an “institutional view”? How about “institutional economics”? Or “institutional theory”? What is the value of a new label “institution-based view”?

The adoption of the term “institution-based view,” coined by Peng (2002), stems from (1) a decision to minimize confusion and (2) a desire to let IB have a label of its own that reflects its diverse heritage. First, in our own (value-neutral and impartial) view, any concept that invokes a new institutionalism framing can be legitimately labeled “institutional theory.” However, there is confusion in the literature. The dominant convention in the literature is that the term

“institutional theory” is increasingly used to refer to the *sociological* version of the institutional literature represented by DiMaggio and Powell (1983). The economic version, represented by North (1990), is often simply labeled as “institutional economics.” Therefore, these labels cannot be used in a scholarly impartial way. Second, because the IB literature draws on a broad range of institutional insights, not only including those from economics and sociology but also from political economy, area studies, and other allied fields, we need to search for a label that reflects such diverse heritage. We cannot use labels such as “institutional theory” (read “sociology”) or “institutional economics,” which would signal a heritage limited to a certain discipline.

The value of the label, institution-based view, is that this is a progeny that the IB field can lay claim to as its own. It not only integrates “institutional economics” and “institutional theory,” but also consolidates decades of IB research in various areas ranging from cultural differences to political risks. As our discipline gradually matures, we need to have the self-confidence (Meyer 2006, 2007) to declare that we are doing neither second-class economics nor second-class sociology. Instead, we are doing first-class IB research. The new label helps us differentiate from existing work in economics, sociology, and other fields while acknowledging the intellectual lineage extending from them. Overall, an institution-based view suggests that the formal and informal rules of the game, known as institutions, shed a great deal of light on what is behind firm performance around the globe.

SECOND CORE PERSPECTIVE: A RESOURCE-BASED VIEW

While the institution-based view primarily deals with the *external* environment, the resource-based view (RBV) focuses on a firm’s *internal* resources and capabilities (Peng, 2001). For IB, this broad view not only includes work inspired by Barney (1991), but also includes related research such as organizational learning theories (Foss & Pedersen, 2004), evolutionary

perspectives (Kogut & Zander, 1993), and dynamic capabilities views (Sapienza, Autio, George, & Zahra, 2006; Teece, 2007). The crucial idea is that firm performance is primarily driven by the valuable, unique, and hard-to-imitate firm-specific resources and capabilities.

While the RBV is a major scholarly movement originating from strategy since the 1990s, IB scholars have significantly contributed to its development (Kogut & Zander, 1993; Meyer & Peng, 2005). In our view (first voiced in Peng [2001: 820]), IB's most significant contributions to the RBV lie in the identification of international knowledge and experience as a valuable, unique, and hard-to-imitate resource that differentiates the winners from the losers in global competition (Chetty, Eriksson, & Lindbergh, 2006; Fang, Wade, Delios, & Beamish, 2007; Hitt, Bierman, Uhlenbruck, & Shimizu, 2006; Luo & Peng, 1999). In many ways, the idea of local embeddedness—that is, idiosyncratic expertise gained through in-country learning despite the liability of foreignness—predates the formal emergence of the RBV (Johanson & Vahlne, 1977).

For those IB scholars who may feel uncomfortable embracing a theoretical perspective coming from strategy as one of IB's two core perspectives, we suggest that looking back to IB's roots may alleviate such concerns. First, both the RBV and IB share some common intellectual ancestors. For example, Edith Penrose (1959) is respected by both communities as a pioneering scholar, and she was an MNE researcher that the IB community can legitimately claim to be one of us (Pitelis, 2004). Second, Stephen Hymer's (1960/1976) earlier work argues that "unequal ability of firms is a sufficient condition for foreign operations" (1976: 46). This insight refers to a crucial assumption of the RBV, resource heterogeneity (that is, firms have different endowments of resources and capabilities), in all but the term. Therefore, the more recent development of "the RBV is arguably a complement to Hymer's focus, and helps strengthen his argument" (Dunning & Pitelis, 2008: 172). In other words, the RBV is not an "alien" or "foreign" view—it is a more recent follow-up of earlier path-breaking IB research. IB scholars

should be very proud of embracing this insightful perspective as one of IB's two core perspectives (Peng, 2001; Sapienza et al., 2006).

WHAT'S NEW? FOUR EXAMPLES

Having outlined the proposed framework that we believe will unify most (if not all) IB research, we need to confront the “What’s new?” challenge. We agree with Buckley and Casson (2001: 91) that “radical changes in theory are very expensive, because a whole new set of concepts needs to be developed and disseminated.” In our view, neither the fundamental question nor the two core perspectives represent such radical changes. What we are proposing is an extension to link these components, which have been codeveloped with numerous colleagues, extensively reviewed, and published earlier (Meyer & Peng, 2005; Peng, 2001, 2003, 2004; Peng et al., 2008; Wright, Filatotchev, Hoskisson, & Peng, 2005). But still, what’s new here? In response, we use the following four examples to demonstrate how this framework can add new insights: (1) international trade, (2) foreign exchange, (3) market entry, and (4) corporate social responsibility.

International Trade

International trade appears in every IB textbook, and the opening question is usually: Why do nations trade? Driven by our interest to bring IB's level of analysis down to the firm level, we suggest that instead of asking why nations trade, we should really ask: Why do *firms* from different nations trade? It is important to clarify that “nations trade” is misleading or confusing. Unless different governments directly buy and sell from each other (such as arms sales), the majority of trade is conducted by firms, which pay little attention to country-level ramifications. For example, Wal-Mart imports a lot into the United States and does not export much. Wal-Mart thus directly contributes to the US trade deficit, which is something the US

government does not like. However, in most countries, governments cannot order firms, such as Wal-Mart, what to do unless firms engage in illegal activities. It is individual firms such as Wal-Mart and its suppliers around the world that trade with each other. Why do they do that?

Applying the RBV insight, we can suggest that it is valuable, rare, and inimitable products generated by firms in one nation that lead to the competitive advantage of a nation's exports. Further, according to the institution-based view, different rules governing trade are designed to share (or prevent) gains from trade. These rules range from the more pro-free trade policies imposed by the WTO to the more protectionist policies that erect tariff and non-tariff barriers. Ultimately, the question, "Why do nations trade?" can be transformed to "What determines the success and failure of exporting firms around the globe?" In short, the answer boils down to institutions and resources (Baggs & Brander, 2006; Peng & York, 2001).

Foreign Exchange

Foreign exchange is another topic that appears in every IB textbook. Coverage typically includes the determinants of foreign exchange rates and the evolution of the international monetary system. Clearly, these deal with institutions—the "rules of the game" governing foreign exchange movements (such as floating versus fixed exchange rate policies). Since most IB professors do not have an international finance background, why is this topic always included in textbooks and typically taught? This is because from an institution-based view, knowledge about foreign exchange rates and the international monetary system helps paint a broad picture of the rules of the game that govern financial transactions around the world (Butler & Joaquin, 1998; Cosset & Delarianderie, 1985).

Armed with this knowledge, savvy IB managers need to develop firm-specific resources and capabilities to rise to the challenge—or at least to avoid being crushed by unfavorable currency movements. The success and failure of these activities dealing with foreign exchange

obviously have major implications for firm performance in IB (Beaulieu, Cosset, & Essaddam, 2005; Kouvelis, Axarloglou, & Sinha, 2001). Therefore, why the topic of foreign exchange is usually included is because discussion of this topic helps answer the fundamental question on firm performance based on the institution- and resource-based views.

Market Entry

Central to the IB field, especially to market entry, is the concept of liability of foreignness, which is the *inherent* disadvantage that foreign firms experience in host countries because of their non-native status (Mezias, 2002; Miller & Parkhe, 2002; Zaheer, 1995). How foreign firms overcome such a liability obviously has major implications for overall firm performance.

How to overcome liability of foreignness? Our two core perspectives suggest two clues: (1) The institution-based view posits that foreign entrants need to develop an intimate understanding of the rules of the game—both formal and informal—in host countries. It is very difficult to imagine firms not doing such “home work” in a given context will be successful. (2) The resource-based view argues that the primary weapon foreign firms employ is overwhelming capabilities that after offsetting the liability of foreignness, there is still some significant competitive advantage left. Thus, our framework can significantly help enhance our understanding of market entry (Yamakawa, Peng, & Deeds, 2008).

Corporate Social Responsibility

Corporate social responsibility (CSR) is likely to emerge as one of the new frontier areas of IB research and practice (McWilliams, Siegel, & Wright, 2006). While our proposed framework seems capable of helping us understand a great deal of traditional topics such as international trade, foreign exchange, and market entry, how does it help enhance our understanding of a new frontier area such as CSR? We argue that our proposed big question on

firm performance is directly relevant to CSR, whose advocates argue that firms need to strive for *triple* bottom line, consisting not only of economic but also social and environmental performance dimensions (Mackey, Mackey, & Barney, 2007; O’Shaughnessy, Gedajlovic, & Reinmoeller, 2007).

Let us consider a leading CSR issue globally: Kyoto Protocol-based initiatives to reduce emissions of greenhouse gases linked to global warming. Formally, since the United States has refused to sign the Kyoto Protocol, US firms, in theory, do not need to care about this from a *formal* institutional standpoint. However, *informally*, pressures are mounting. Numerous US states, industries, and nongovernment organizations (NGOs), in the absence of Federal mandate, have joined forces to combat climate change—think of Al Gore’s winning of both the Oscar award and the Nobel Peace Prize in 2007. Globally, EU and Japanese firms have taken Kyoto seriously and have attempted to differentiate themselves as “green” leaders. Consumers everywhere, including many in the United States, now increasingly prefer “green” products (Christmann & Taylor, 2006; Doh & Guay, 2006). The upshot is that, as predicted by the resource-based view, many US firms are now investing aggressively to develop firm-specific resources and capabilities that will enable them to prosper in the “Kyoto marketplace.” Clearly, many executives realize that these capabilities may make or break the firm in the future (He, Tian, & Chen, 2007; Mackey et al., 2007; Schuler & Cording, 2006). In summary, our proposed framework suggests that how institutions impact firm decisions concerning CSR and how firms rise to the challenge to develop firm-specific CSR capabilities around the globe will consist of an exciting IB research agenda in the years to come.

DISCUSSION

This article contributes to the literature by endeavoring to propose a unified framework for IB. McKinley, Mone, and Moon (1999) suggest that whether a particular framework gains widespread acceptance depends on its (1) continuity, (2) novelty, and (3) scope. We argue that our unified framework entails these three attributes and therefore predict that it is likely to gain wide acceptance—albeit with the inevitable disagreements and criticisms it may attract.

First, our framework exemplifies a great deal of *continuity*. As first argued by Peng (2004), IB research is fundamentally driven by an interest in enhancing firm performance. Therefore, identifying the fundamental question on “What determines the success and failure of firms around the globe?” for IB does not call for radical changes in direction. Likewise, organizing the vast IB literature around the two core perspectives—namely, the institution- and resource-based views—does not deviate from the central thrusts of much existing IB research.

Second, our framework 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. Moreover, a unifying framework needs to help the field tackle novel phenomenon in the future. The rapidly moving events of the global environment, such as the rise of emerging economies as the new IB battleground (Luo & Tung, 2007; Mathews, 2006; Wright et al., 2005; Yamakawa et al., 2008) and the impact of antiglobalization activities on IB (Eden & Lenway, 2001), necessitate innovative theoretical perspectives and empirical methodologies to provide new answers to the performance question and modify old answers (Dunning & Lundan, 2008; Tsui, 2007). While none of the components that we identify for the framework is totally strange to most IB scholars, putting them together and arguing that this consists of a unified framework for the discipline is novel—the *first* in the literature. It helps clarify the direction of

our discipline, acknowledges our intellectual roots, celebrates our theoretical and empirical contributions, and points out the likely future trajectories as the field moves ahead.

Finally, our framework excels in its *scope*. A broad scope helps increase the potential number and variety of theoretical insights and empirical tests, leading to a higher likelihood that a coherent stream of literature can be established (McKinley et al., 1999). Previous efforts in proposing central frameworks for IB, such as Dunning (1993), center on the MNE. Insightful as these earlier frameworks are, MNE research, as argued earlier, does not exhaust the universe of IB discipline (Shenkar, 2004; Toyne & Nigh, 1998). The challenge is to identify a framework that not only consolidates earlier accomplishments drawn from decades of MNE research, but also incorporates elements crucial to non-MNE but IB-active firms as well as domestic firms that interact with foreign entrants. We believe that our proposed framework reflects such reality that IB is a “big tent.” The many possible factors that may influence firm performance around the globe, viewed from institution- and resource-based lenses, allow for numerous ways of theorizing and testing, resulting in the expansion of the scope of our cumulative knowledge.

Overall, we submit that our proposed framework unifies research on diverse IB research topics—ranging from international trade to CSR—under one umbrella. Of course, additional questions and perspectives can and should be invoked and developed. However, we argue that they would all relate in one way or the other to our framework. In other words, we suggest that every IB topic can be tackled *starting* with the fundamental question and the two core perspectives that we outline here, but not necessarily ending with this framework.

CONCLUSION

“Why, after decades of study, do we not have a more definitive sense of the purpose and value of our research?” Sullivan (1998: 881) complains in a high-profile review of a high-profile

book *International Business: An Emerging Vision* (Toyne & Nigh, 1997)⁹ ten years ago. “Why,” Sullivan (1998: 881) continues, “after reading this text [Toyne & Nigh, 1997], do we have the sad suspicion that the eloquent calls for resolution and forceful exhortation for change will fall upon deaf ears?” While much ink has been spilled on the necessity for IB to generate and rally around an integrated, central framework, few have stood and delivered what is such a framework (Feldman, 1997). As the field celebrates its 40th or 50th anniversary, it will be a shame if none of us has tried to do it. Therefore, at the risk of attracting significant criticisms, we have decided to stand and deliver by making the following two points:

- IB’s fundamental purpose and value lies in our relentless interest in answering the big question: What determines the success and failure of firms around the globe?
- IB’s primary perspectives to address this question center on the institution- and resource-based views. Firm performance is primarily driven by the *combined* impact of institutions and resources.

In conclusion, if this article can provoke more IB scholars to stand and deliver what a central framework for IB should look like, then our purposes for proposing the unified framework for IB will have been well served.

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⁹ The Toyne and Nigh (1997) volume is the published proceedings from a major conference on the state-of-the art in IB held at the University of South Carolina in 1992. The conference was attended by approximately 150 scholars, and the published volume was contributed by more than 40 scholars.

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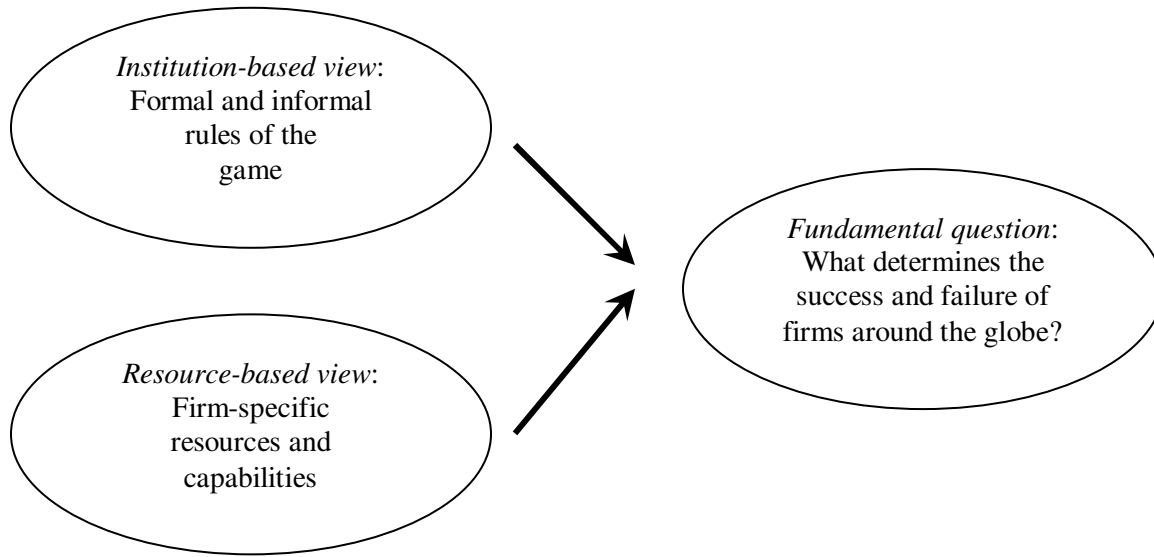
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FIGURE 1. A Unified Framework for International Business



**APPENDIX: SSCI Rankings of Most Heavily Cited *JIBS* Articles
(Based on Citations Per Year)¹**

	Articles	SSCI Citations	Average Citations per Year
1	Kogut, B., & Singh, H. 1988. The effect of national culture on the choice of entry mode. Vol. 19: 411-432.	458	21.81
2	Kogut, B., & Zander, U. 1993. Knowledge of the firm and the evolutionary theory of the multinational corporation. Vol. 24: 625-645.	271	18.07
3	Johanson, J., & Vahlne, J. E. 1977. The internationalization process of the firm: A model of knowledge development and increasing foreign market commitments. Vol. 8: 23-32.	528	16.50
4	Dunning, J. H. 1988. The eclectic paradigm of international production: A restatement and some possible extensions. Vol. 19: 1-31.	246	11.71
5	Oviatt, B. M., & McDougall, P. P. 1994. Toward a theory of international new ventures. Vol. 25: 45-64.	159	10.60
6	Parkhe, A. 1991. Interfirm diversity, organizational learning, and longevity in global strategic alliances. Vol. 22: 579-601.	166	9.76
7	Anderson, E., & Gatignon, H. 1986. Modes of foreign entry: A transaction cost analysis and propositions. Vol. 17: 1-26.	201	8.74
8	Dunning, J. H. 1998. Location and the multinational enterprise: A neglected factor? Vol. 29: 45-66.	94	8.55
9	Knight, G. A., & Cavusgil, S. T. 2004. Innovation, organizational capabilities, and the born-global firm. Vol. 35: 124-141.	42	8.40
10	Geringer, J. M., & Hebert, L. 1991. Measuring performance of international joint ventures. Vol. 22: 249-263.	151	8.39

11	Shenkar, O. 2001. Cultural distance revisited: Towards a more rigorous conceptualization and measurement of cultural differences. Vol. 32: 519-535.	67	8.38
12	Dunning, J. H. 1995. Reappraising the eclectic paradigm in an age of alliance capitalism. Vol. 26: 461-491.	117	8.36
13	Geringer, J. M., & Hebert, L. 1989. Control and performance of international joint ventures. Vol. 20: 235-254.	146	7.30
14	Mullen, M. R. 1995. Diagnosing measurement equivalence in cross-national research. Vol. 26: 573-596.	102	7.29
15	Lerner, E. E., & Storper, M. 2001. The economic geography of the Internet age. Vol. 32: 641-665.	51	7.29
16	Hofstede, G. 1983. The cultural relativity of organizational practices and theories. Vol. 14: 75-89.	187	7.19
17	Lyles, M. A., & Salk, J. E. 1996. Knowledge acquisition from foreign parents in international joint ventures: An empirical examination in the Hungarian context. Vol. 29: 154-174.	86	7.17
18	Agarwal, S., & Ramaswami, S. N. 1992. Choice of foreign market entry mode: Impact of ownership, location and internalization factors. Vol. 23: 1-27.	121	7.12
19	Luo, Y., & Peng, M. W. 1999. Learning to compete in a transition economy: Experience, environment, and performance. Vol. 30: 269-296.	69	6.90
20	Kim, W. C., & Hwang, P. 1992. Global strategy and multinationals' entry mode choice. Vol. 23: 29-53.	116	6.82
21	Reuber, A. R., & Fischer, E. 1997. The influence of the management team's international experience on the internationalization behaviors of SMEs. Vol. 28: 807-825.	74	6.73
22	Newman, K. L., & Nollen, S. D. 1996. Culture and congruence: The fit between management practices and national culture. Vol. 27: 751-779.	77	6.42
23	Ralston, D. A., Holt, D. H., Terpstra, R. H., & Yu, K. 1997. The impact of national culture and economic ideology on managerial work values: A study of the United States, Russia, Japan, and China. Vol. 28: 177-207.	76	6.33
24	Kuemmerle, W. 1999. The drivers of foreign direct investment into research and development: An empirical investigation. Vol. 30: 1-24.	62	6.20
25	Leonidou, L. C., & Katsikeas, C. S. 1996. The export development process: An integrative review of empirical models. Vol. 27: 517-551.	79	6.08

1. SSCI citations are as of February 1, 2008. Since older papers obviously would have more opportunities to accumulate citations, citations per year would control for this “age” effect and therefore would paint a more informative picture of a paper’s influence.